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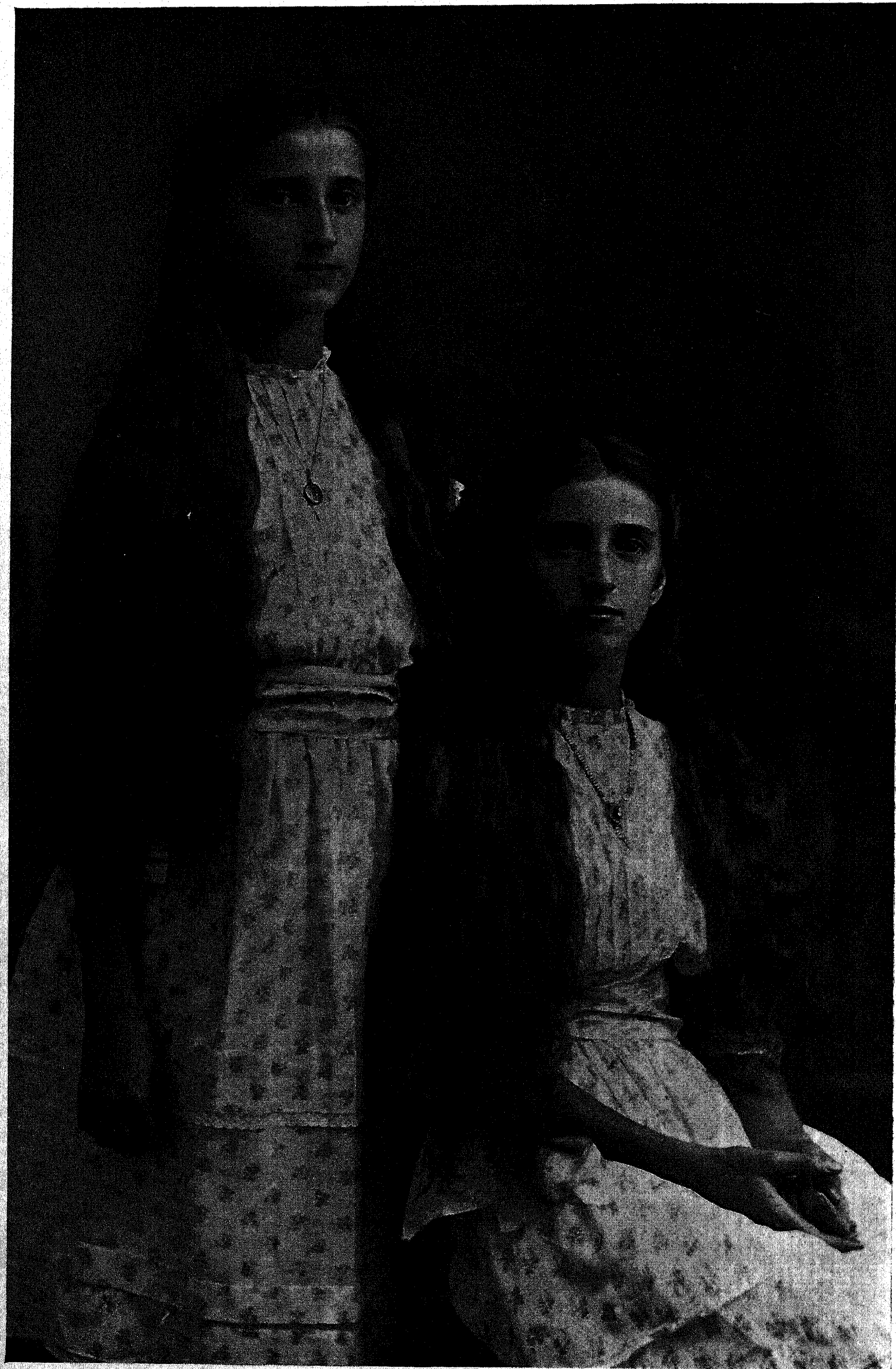


ARE THEIR TEST-DIFFERENCES DUE TO ENVIRONMENT?

Frontispiece

Living in the same state, less than a hundred miles apart, Mary (left) grew up in a small town, and Mabel on a farm. They differ more in test reactions mentally, temperamentally and physically than any of the other twin pairs studied up to this time. This in spite of the fact that in this case the environmental factors are apparently less different than in at least three of the cases studied. When the picture was taken the twins were seventeen, at which time they still looked very much alike. At the present time Mabel weighs 27 pounds more than Mary and their similarity is not so striking.

MENTAL AND PHYSICAL TRAITS OF IDENTICAL TWINS REARED APART

Case IV. *Twins Mary and Mabel, and Review of the First Four Cases Studied*

H. H. Newman
University of Chicago

It has been nearly three years since the three earlier cases ("A" and "O", "E" and "G", and "C" and "O") were studied. These earlier cases came to my attention almost simultaneously, and there was some reason, at that time, to expect to discover and to study at least ten cases in four or five years. My somewhat sanguine expectations have been doomed to disappointment.* Only three additional cases have come to my attention. Of these, one pair has steadfastly refused to submit to examination, a second pair are still only about nine years of age and will not be ready for examination for some five years at least, and the third pair have just recently left us after spending two days in our laboratory taking tests. The data on this pair constitute the main material of the present report.

It now seems certain that there can be but few cases in existence of identical twins separated in infancy and reared apart. With Muller's single case and the four I have studied we have now a total of only five. In the course of time the cases may possibly mount up to ten, but I have very little hope of ever exceeding that number. The extreme rarity of such cases, together with the fact that they constitute the only human material in which the potencies of heredity and environment can be directly measured, renders it imperative that we should make a strong effort to obtain the maximum information about each case and to arrive at a correct diagnosis of the special environmental differences and their effects.

The Physical Environment of Mary and Mabel

These twins are in marked contrast with the other four pairs reared apart in that they have been more or less constantly in communication, visiting back and forth ever since early childhood. Until the last nine years, however, they have not been especially well acquainted, nor have they been much interested in each other. Only about nine years ago did they begin to be really companionable; now they exhibit a great mutual affection.

Mary and Mabel, now twenty-nine years old, were separated at five months of age, both being adopted by near relatives. They have always lived in the same part of the state of Ohio, less than one hundred miles apart. Hence the climatic features of the environment have been about the same. Both lived on farms until six years old, when Mary moved to a small town where she has resided ever since. She has never lived a very active, outdoor life, and she has done little manual labor. Since finishing high school she has clerked in a store daytimes and given piano lessons in the evenings.

Mabel has lived all her life on a prosperous 130 acre farm, leading the

*In correcting the proof of this paper I am surprised at the pessimistic tone of the introduction, for, since this paper went to press, certainly three and probably four new cases of identical twins reared apart have been found. It now seems probable that my goal of ten cases may soon be reached.

life of a typical energetic farm girl. She has enjoyed doing all sorts of farm work, such as milking, churning, feeding chickens and stock, and rather heavy household work. Her well-calloused hands bespoke her industry. She says that nothing could induce her to leave the farm.

Both twins have had the usual children's diseases. Mary had measles twice while in early grades, chickenpox when two or three, whooping cough before entering school, and she has had "influenza" almost annually during the winter. Mabel had measles once as a baby and again at eighteen years, whooping cough at about six years of age, and is rarely troubled with "influenza" or colds. The only difference of note in the health record is the rather bad record of frequent severe colds ("influenza") on Mary's part, and the almost complete freedom from colds on Mabel's part. This may possibly account partly for the very pronounced difference in physical condition seen in the twins at the present time (See Table I). Mabel, the farm girl, is very much more robust. She is an inch taller, a size larger in all physical measurements, has firm, hard muscles, and weighs 138½ pounds as compared with 110¾ for Mary, a difference of 27¾ pounds. This difference in weight amounts to nearly 25% of the total weight of Mary. Mary is distinctly under-weight, has inferior muscular development and lacks the muscular firmness of Mabel. Mabel has a clearer and somewhat ruddier complexion than Mary. Mabel walks with an erect posture and a rather masculine gait. Her movements are assured and rather quick. Mary is less erect, has a more ladylike walk, is slower in her movements and more inclined to sit still and avoid exertion

This contrast in physical condition is much greater than in any of the four earlier cases, though differences of considerable extent were noted in twins "A" and "O" and in twins "C" and "O".

Formal and Informal Educational Record of Mary and Mabel

Mary went through grade school in her home town and attended for three years the small home town high school, a school of about forty pupils. She had her final year in a city high school of sixteen hundred pupils. In grade school she preferred English and arithmetic, while in high school she preferred English and Latin. She was always in the upper twenty-five per cent of her class. Mary began the study of music, principally piano, at the age of about ten years and has taken this study rather seriously. She now gives piano lessons in the evenings. She has never had much time for books, her reading confined largely to popular magazines and newspapers. She was brought up in a religious environment. The other children in the family were ten or more years older than she, and did not seem very much like brothers and sisters to her.

Mabel went through the primary grades in the nearby country school. She started to attend high school in a neighboring city, but left after six weeks because she was needed at home to take care of her foster-mother's new baby. She does not remember being especially interested in any of her school studies. She seldom reads anything except magazines and newspapers. She was brought up in a family of children of not very different ages from her own, one foster brother about twenty months older and one about eight years older. She has always been treated as one of the family. The family is distinctly a religious one and Mabel has been much influenced by religious training.

There is no marked difference in economic status between the two families, both being in very good circumstances. Neither girl has traveled to any great extent, the trip from Ohio to Chicago being the longest either has ever taken. Other trips have been limited to one-day rides in automobiles or short trips by train to near-by towns.

MARY AND MABEL AS BABIES

Figure 1

At this age the twins were said to be indistinguishable. Today, at twenty-nine, Mary (left) the town girl, weighs 27¾ pounds less than Mabel, the country girl, but the evidence of monozygosity are conclusive. In spite of the pose in the picture both are right-handed. (See Figures 4-6.)

Are Mary and Mabel Monozygotic Twins?

The children were extremely similar as babies, hardly distinguishable when two years old when the second photograph (Figure 2) was taken. They continued to be extremely similar up till the time of the third photograph (Frontispiece), which was taken when they were seventeen. Since that time they have changed somewhat in appearance. Mary has her hair medium long and waved, while Mabel has a hair-cut practically like that of a man. The great difference in weight (27¾ pounds) increases the lack of resemblance. To the experienced student of twins, however, they are still recognizable as monozygotic twins, for they are practically identical in features, ears, teeth, voice, expression, and coloring of eyes and hair. Both are right-handed and both have clockwise hair-whorl on the right side of the crown. In this case, as in many others, the finger-print and palm-print evidences of monozygosity are conclusive. There is hardly a pair of twins in my collection whose dermatoglyphics more closely approximate identity than those of Mary and Mabel. The finger-prints are reproduced in Figure 6. The formulae are as follows:

	Left hands	Right hands
Mary:	$W^u.R^w.W.W.U$	$W^u.R^w.W^u.W.U$
Mabel:	$U^w.W^u.W^u.W.U$	$W^u.R^w.W^u.W.U$

The two right hands are more similar

MARY AND MABEL AT TWO YEARS

Figure 2

Their marked physical differences in later life had not appeared at this time.

than either right is like own left. In fact, the two right hands are very nearly identical, finger for finger. This is one excellent criterion of monozygosity. The total ridge count of Mary, using the Bonnevie method of rating, is eighty-four, while that of Mabel is eighty-three, a difference of only one point. This difference is very small even for monozygotic twins.

The palm patterns are even more strikingly similar than are the finger patterns (Figures 3 and 4). It was impossible to get perfect prints of Mabel's palms because of the large callouses. The blank space in the center of her left palm print (Figure 4) is due to large blister that had been peeled off. In spite of the imperfections in the prints their extra-

ordinary similarity will be obvious to the observer. The formulae of the palmar main lines and patterns are given in the legend to Figure 4.

In formulae the two right hands are exactly alike and the two left hands are exactly alike, but neither right is like the left of the same individual. Only monozygotic twins show correspondences of this sort. In fact, a close comparison of the palm prints should convince any one that these twins are monozygotic.

The facts that both girls are very positively right-handed, both have exactly similar clockwise hair whorls, that there are no indications at all of asymmetry reversal in palm or finger patterns, in dentition, or in bodily dimensions, indicate that these twins are true duplicates, not even mirror-image duplicates. They therefore belong to that group in which the asymmetry mechanism has played little if any part in producing differences. In this respect the present case is like my case II (twins "E" and "G") and like Muller's case. There can therefore be no question about the monozygotic origin of the twins Mary and Mabel.

Physical Resemblances and Difference

Something has already been said about the very pronounced differences in physical condition and in weight. One of my assistants, Miss Helen Pris, has recently examined and measured fifty left-handed girls for me, to discover whether there exist any bodily asymmetries correlated with handedness. Experienced in the use of anthropometric instruments, she made the measurements on Mary and Mabel that are given in Table I.

Note that Mabel is larger in every measurement. She is one inch taller, and shows greater bone length everywhere except in "alecranon to middle finger tip" of left hand, where she and her sister are equal. The greater differences are seen in the circumference measurements, where muscular development is reflected. Mabel's left fore arm is 1.4 cm. larger than Mary's, her right calf is 3.1 cm. larger. There is a very great difference in foot size, Mabel's left foot being 1 cm. longer and 1.2 cm. wider than Mary's, necessitating a size larger and a size wider shoe. The differences in hands are not so great, due possibly to the fact that Mary's hands have been used almost as much in piano playing as Mabel's in farm work.

Both girls have worn glasses for a long time, both being "far-sighted." Until quite recently they could use each other's glasses, but lately Mary's have been made "stronger" than Mabel's.

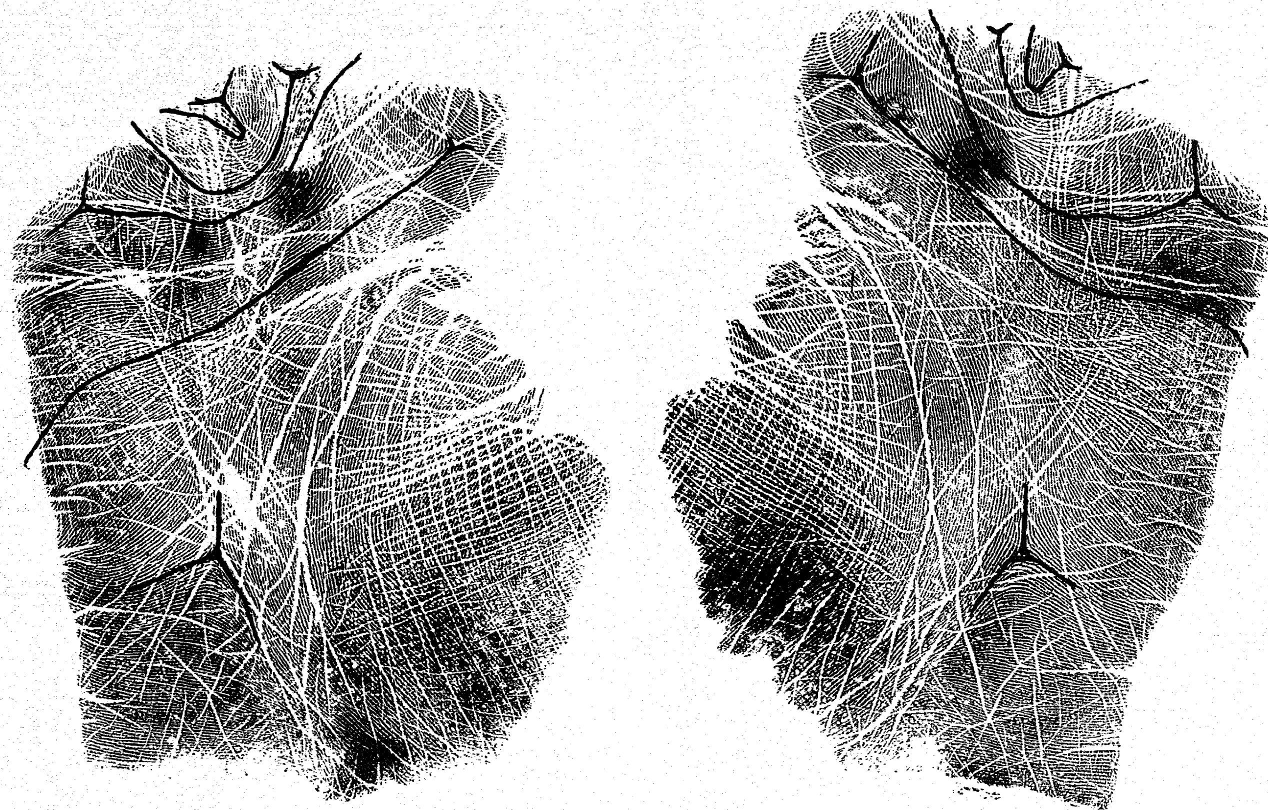
The teeth of both are of almost exactly the same shapes and color and are in equally good condition. Neither showed any definite irregularities. The same amount of repair had been done on both.

When examined, Mabel's hair, which was quite short, seemed a little softer and of a slightly lighter shade than Mary's, but she thinks that this difference is due to its having just been washed, while Mary's longer, waved hair had not been washed for a week. It was impossible to judge whether or not there was a real difference in hair color. At best the difference was slight, though noticeable.

At the age of seventeen there was very little difference in physical condition between Mary and Mabel. Hence the very great difference that now exists must have been the product of the last twelve years. This great physical difference is one of the most striking of the effects of different environments upon these duplicate hereditary constitutions. Noteworthy differences of the same sort were recorded for twins "A" and "O" and twins "C" and "O", but the differences were far less marked than in the present case.

Psychological Examinations

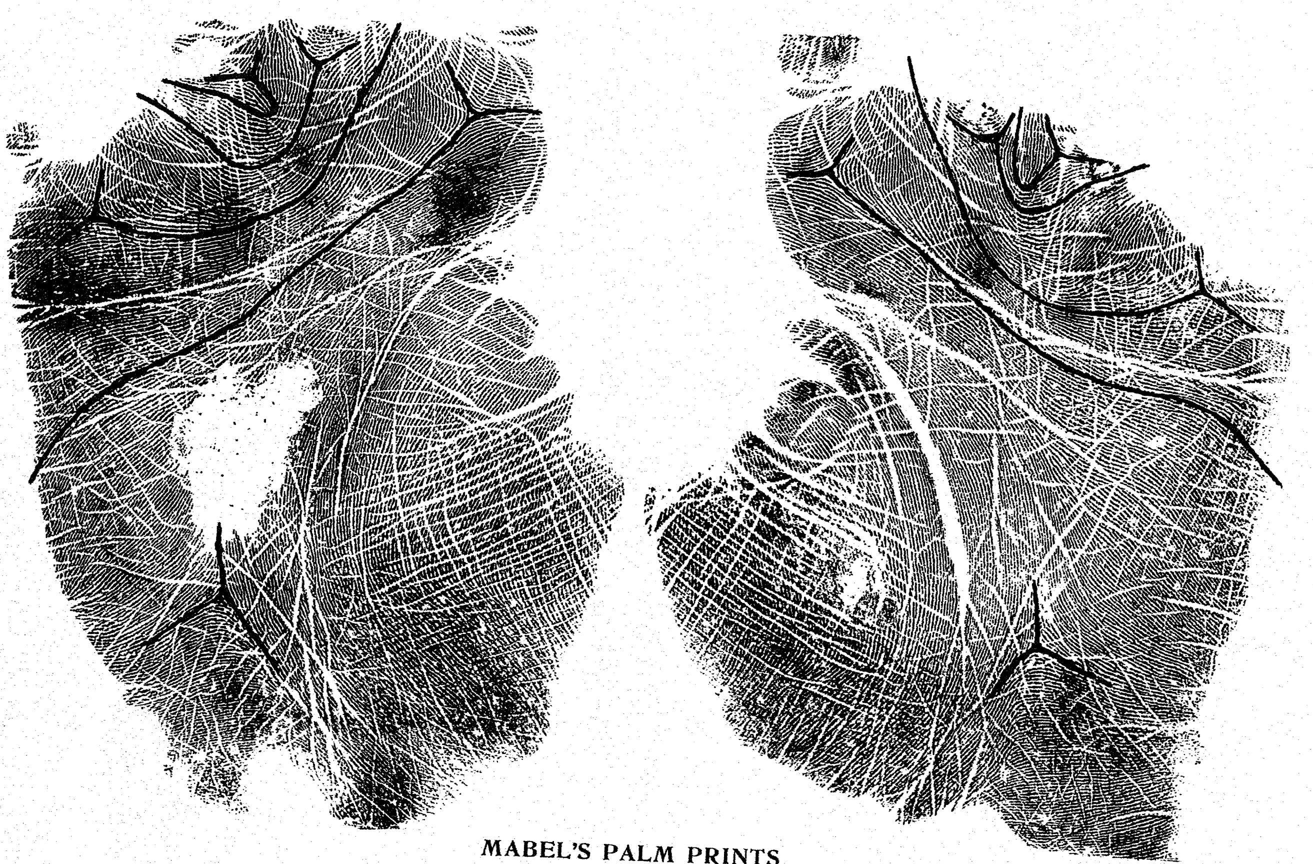
Practically the same tests were used for Mary and Mabel as for the other



MARY'S PALM PRINTS

Figure 3

The two right palms are strikingly similar as are also the two left palms. Neither right palm is as much like the left of the same individual as like the corresponding palm of the other individual.



MABEL'S PALM PRINTS

Figure 4

Formulae for the palms is as follows:

Mary:	11.7.7.5′-t″-A^{u}/A^{c}.O.O.O.L	11.9.7.5′-t″-A^{u}/A^{c}.O.O.L.O
Mabel:	11.7.7.5′-t″-A^{u}/A^{c}.O.O.O.L	11.9.7.5′-t″-A^{u}/A^{c}.O.O.L.O

All four hands are decidedly similar, Mabel's being larger and stronger.

three cases except that in this case the motor tests for handedness were omitted because of lack of facilities at the time the tests were given. The tests were administered and rated as before by Mrs. Blythe Mitchell and confirmed by Dr. F. N. Freeman. The following tests were given and will be reported in this order:

1. Stanford-Binet Test.
2. Thurstone Psychological Examination Test.
3. Otis Self-Administering Test.
4. International Test—as worked out by Dr. Stuart C. Dodd.
5. Stanford Achievement Test.
6. Downey Will-Temperament Test.
7. Pressy X-O Test of Emotions.
8. Kent-Rosanoff Association Test.
9. Woodworth-Mathews Questionnaire of Emotional Stability.

The first five are classed as intelligence tests and the last four as temperamental and emotional tests.

I. Intelligence Tests

1. Stanford-Binet Test

Both girls worked steadily and without showing any signs of worry. The same amount of time was allowed to both. Mary's paper showed an I. Q. of 106.2, which is higher than that of any of the separated twins studied and within the range designated "normal." Mabel's I. Q. was 88.5, which, though higher than those of twins "E" and "G" and "A," is still in a group designated as somewhat "backward." The difference in I. Q. between Mary and Mabel is 17.7 points, or over 3.3 times as great as the average of fifty pairs of *identical* twins reared together, and even nearly twice as great as the average of fifty pairs of *fraternal* twins reared together, which is 9.9 points. This is the most striking difference yet found between identical twins. Mary was rated with a mental age of seventeen years and Mabel with one of fourteen years, two months. The difference is thirty-four months, while the average difference for fifty pairs of identical twins reared together is 8.4 months. Hence the difference in mental age between Mary and Mabel is 4.2 times as great as the average difference of fifty pairs of *identical* twins reared together and over twice as great as the average difference of fifty pairs of *fraternal* twins reared together, which is 15.9 months. This difference is even more striking than that expressed by the I. Q.

2. Thurstone Psychological Examination

This is a test commonly given at the University of Chicago to determine scholastic aptitude. It consists of five different elements testing five different types of mental ability. The following is the score of the twins as compared with the maximum possible:

SCORES

	Maximum	*Mabel*	*Mary*
Completion	80	14	20
Artificial language	74	0	35
Analogies	48	6	14
Arithmetic	80	16	16
Opposites	81	9	30
Totals	363	45	115

This test proved too difficult for both twins, but Mary handled it better than either of twins "C" and "O" of Case III and nearly as well as twin "O" of Case I. It will be remembered that twins "E" and "G" of Case II could do nothing at all with this test. The grade of zero made by Mabel in the artificial language test was due to her complete inability to understand the printed directions, while Mary understood what was wanted and did as much as she could in the time allowed. Mary had a percentile rank of 32, which means that she is 7 above the minimum of the second quartile, while Mabel had a percentile rank of 2, a very low rank indeed. Thirty per cent of all students taking this test fall between the rankings of Mary and Mabel. The difference here is not quite so great as was the case in twins "A" and "O" of Case I, but is nevertheless a very striking one. Since this test was not given to twins reared together no comparison is possible.

3. Otis Self-Administering Test

The results of this test may be tabulated as follows: Mary answered 48 questions, out of which 41 were correct; Mabel answered 44, out of which 30 were correct. The items of the test were classified as follows:

	Mabel	*Mary*
1. Hard directions	1	2
2. Arithmetic	5	6
3. Logical Relations	12	16
4. Opposites	5	6
5. Disarranged Sentences	0	0
6. Numbered Series Completion	1	1
7. Analogies	3	2
8. Proverbs	3	6
9. Syllogisms	0	2
10. Geometrical Relations	----	----
Totals	30	41
I. Q. Rating	91	111

There is a difference of fourteen points in I. Q., which is 3.1 times as great as the average difference between fifty pairs of *identical* twins reared together and over 1.5 times as great as that between fifty pairs of *fraternal* twins reared together. The difference between Mary and Mabel is in this test not quite as great as was that between twins "A" and "O" of Case I or that between twins "E" and "G" of Case II, but was four points greater than that between twins "C" and "O" of Case III.

4. International Test

This test is entirely pictorial and does not at all depend upon language. Hence it is perhaps more of a test of native brightness as distinguished from achievement.

SCORES

	Mabel	*Mary*
1. Cube counting and matching	8	12
2. Matching associated pictures	22	23
3. Pictorial similarities	11	6
4. Similarities in facial expression	16	15
5. Mazes	7	7
6. Rhythm series completion	11	12
7. Pictorial analogies	15	23
8. Narrative completion	4	4
Totals	94	102

The remarkable feature of this test is that Mabel is so nearly equal to Mary, although far behind in all the other intelligence tests. This is not because Mabel made a good showing, but because Mary made a particularly bad showing, considering her mental ability as demonstrated by the other tests. On the day when the tests were taken Mary had a cold and seemed very tired. This, rather than lack of ability, may account for her failure to make a good record in this test. Mabel made just about the score her mental rating in the other tests would lead us to expect, but yet persons with as high an I. Q. as Mary showed in the Stanford-Binet test should score at least 150 on the International Test. The International Test is a test of common sense rather than education, and it may well be that in this respect Mary is only slightly superior.

5. Stanford Achievement Test

The scores made in terms of "subject age" as shown below.*

The difference in educational age as shown by this test of educational achievement is thirty-four months,

*SCORES IN TERMS OF MONTHS OF EDUCATIONAL AGE

		Mabel	*Mary*
Reading	Sentence meaning	15 yrs., 10 mos.	17 yrs., 4 mos.
	Word meaning	15 yrs., 2 mos.	17 yrs., 1 mos.
	Both	15 yrs., 6 mos.	17 yrs., 2½ mos.
Arithmetic		11 yrs., 2 mos.	17 yrs., 6 mos.
Nature study		15 yrs., 7 mos.	16 yrs., 3 mos.
History and literature		13 yrs., 3 mos.	15 yrs., 7 mos.
Language usage		15 yrs., 4 mos.	18 yrs., 6 mos.
Spelling		15 yrs., 9 mos.	18 yrs., 4 mos.
Educational Age		14 yrs., 5 mos.	17 yrs., 3 mos.

which, remarkably enough, is exactly the same as was the "mental age" difference in the Stanford-Binet test, although the latter is supposed to be more of a native ability test than an achievement test. The results, however, both in this and in the other three cases in this series, make us suspect that both tests are measures of somewhat the same mental characteristics. Since the average divergence in mental age between individuals of a pair in fifty pairs of identical twins reared together was 6.2 months, the difference between Mabel and Mary is over 5.6 times as great. This difference is even somewhat greater than was found in the Stanford-Binet test.

II. Temperamental-Emotional Tests

6. Downey Will-Temperament Test

The differences between Mary and Mabel as shown by this test are graphically represented in the Will-Temperament Profile (Figure 5). Mary's total score is 76 and Mabel's 52, a difference of 24, or nearly 50 per cent of Mabel's total. Since a score of around 90 designates a strong personality and a score below 40 or 50 indicates some striking deficiency in will-temperament qualities, it is interesting to note that Mary has a moderately strong personality and Mabel is just above the level of deficiency. The greatest difference is in "coordination of impulses," where Mary ranks 10, the maximum, and Mabel 1, or nearly the minimum. On the other hand, they both took the maximum rating of 10 in "resistance to opposition." Both took 6 in "motor inhibition." In most other respects they were definitely unlike, and the whole test indicates great contrast in will-temperament. They are almost as unlike temperamentally as any two persons chosen at random would be expected to be.

7. Pressey X-O Tests

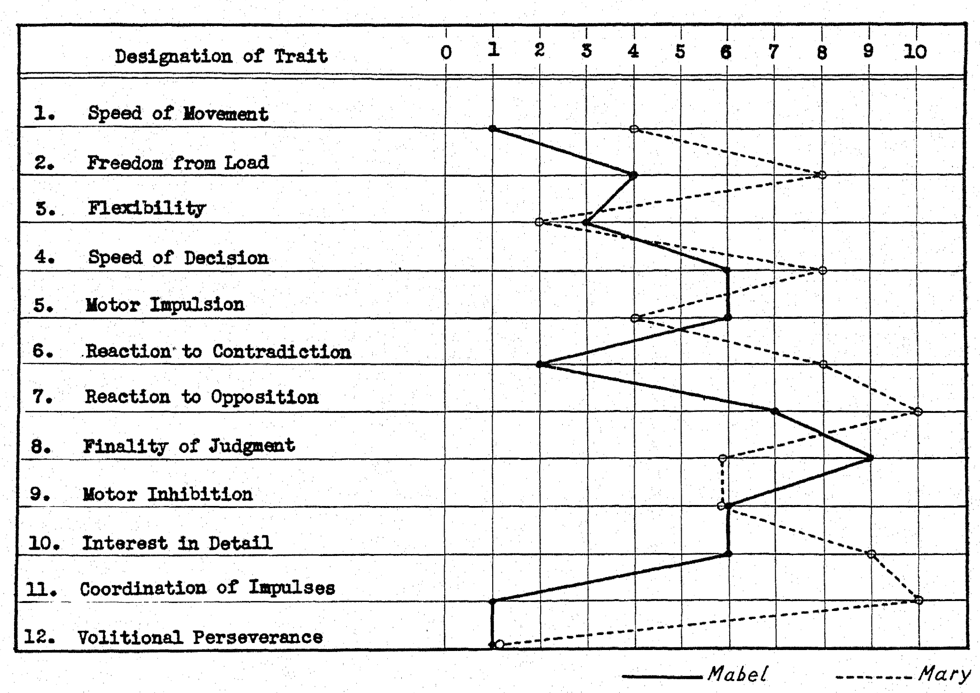
This test consists of four minor tests; the scores are shown below.*

Type of words crossed out as *"unpleasant"*	*Mabel*	*Mary*
Disgust	11	18
Fear	14	19
Sex	6	11
Self-Feeling	12	19
Total	43	67

Mabel, brought up on a farm, is much less easily disturbed and more callous

**Scores of Pressey X-O Tests.*

		Normal	*Mabel*	*Mary*
Test I. "Unpleasant" words	25th percentile	27		
	Median	41	43	67
	75th percentile	52		
Test II. "Words associated in mind with given word"	25th percentile	41		
	Median	55	33	52
	75th percentile	70		
Test III. "Things thought wrong"	25th percentile	60		
	Median	73	80	66
	75th percentile	86		
Test IV. "Things you have worried or felt nervous about"	25th percentile	33		
	Median	46	39	46
	75th percentile	55		
Total number of words crossed out in four tests	25th percentile	200		
	Median	230	195	231
	75th percentile	260		
The total deviation from mode in words circled	25th percentile	41.6		
	Median	47.2	53	50
	75th percentile	51.7		



THE "WILL-TEMPERED PROFILE" OF THE TWINS

Figure 5

The twins are almost as unlike tempermentally, as measured by this test, as two persons picked at random.

about unpleasant ideas than is Mary.

Worries classified	*Mabel*	*Mary*
Suspicious (paranoid)....	4	7
Jumpy (neurotic)	7	11
Self-conscious (shut-in personality)	4	3
Melancholic	6	8
Hyperchondriacal	18	17
Total	39	46

Again Mabel is more normal, or stable, temperamentally, than Mary. On the whole, both girls are decidedly normal and wholesome in their reactions, but Mabel is superior in stability.

8. Kent-Rosanoff Association Test

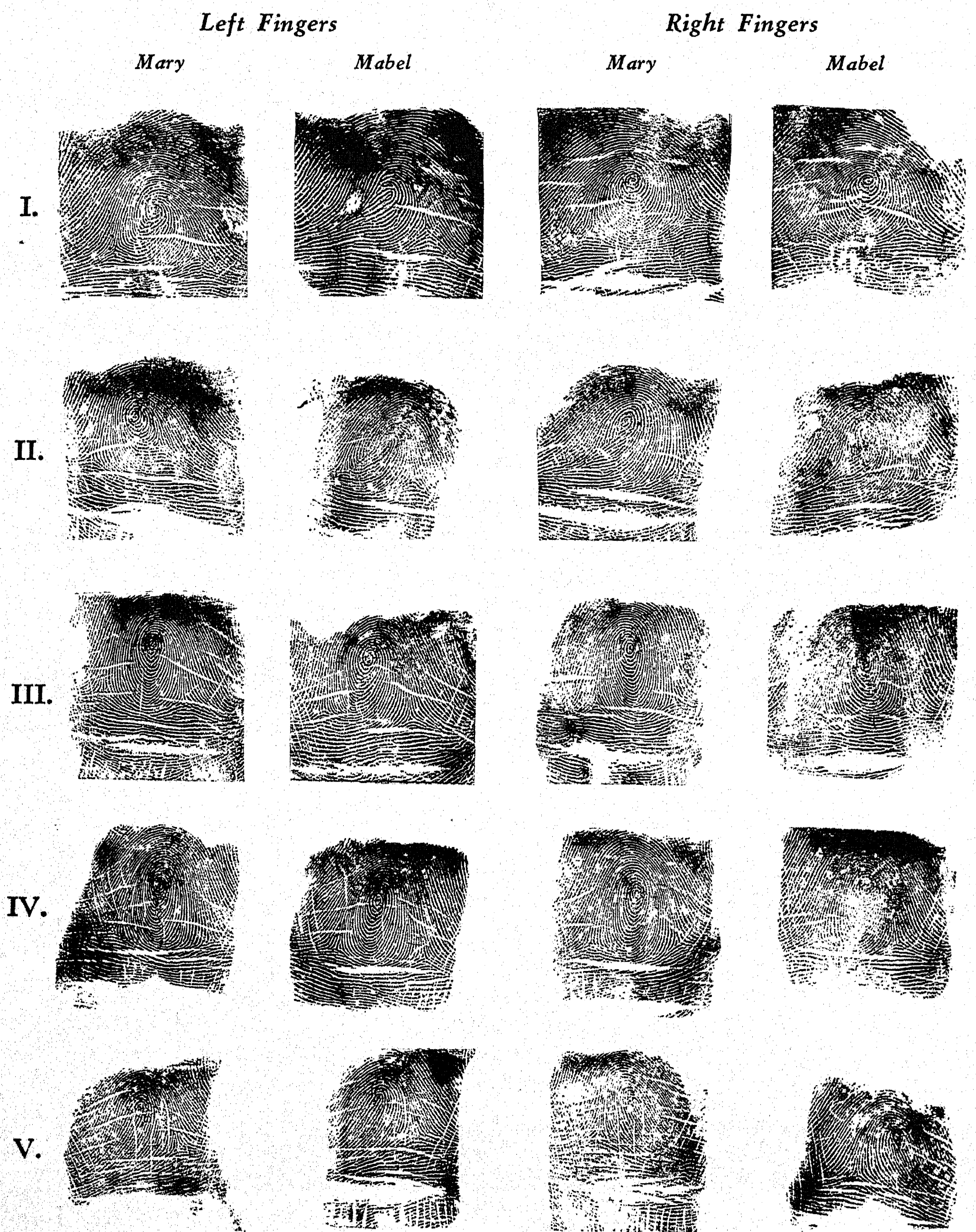
In this free association test (100 words) the twins made very similar records. Their responses were identical for 18 of the 100 words. Mabel gave the "normal" response to 95 of the 100 words, while the other 5 were "doubtful." Mary gave the "normal" response to 96 words, 3 were "doubtful," and 1 "individual." As based on the results with 1,000 normal subjects Mary rates 120.8, Mabel 129.8. Both rank high, but Mabel is a little higher. They are decidedly similar and normal.

9. Woodworth-Mathews Questionnaire of Emotional Stability

Mabel gave only 3 unfavorable responses, Mary 8. Both of these are very low numbers, but Mabel is more stable than Mary, as other tests had showed quite clearly. Of the 75 responses given by the two girls 68 were identical, indicating a high degree of similarity in emotional stability.

Summary of Case IV and General Discussion of all Cases Thus Far Studied

Mary and Mabel represent the most extreme differences so far observed in identical twins reared apart. This is astonishing in view of the facts that they live in the same climate and have been in communication with each other



FINGER PRINTS OF MABEL AND MARY

Figure 6

Equivalent fingers of Mabel and Mary are placed side by side to facilitate comparison. The dermatogtyphic similarities are more striking than the average similarities found in identical twins, and no such similarities are encountered in any other individuals. Same-sided similarity, characteristic of the most similar category of identical twins, is shown in this case.

since early childhood. The environmental differences are of two sorts, educational and occupational. Mary has had the superior educational experience, but Mabel the superior occupational experience.

Mary ranks much higher intellectually and in will-temperament, while Mabel is very decidedly superior physically and somewhat more stable and normal in her emotional reactions as brought out by the Pressy and Woodworth tests of emotional stability. Some of the emotional tests show strong resemblance, others show notable differences.

While in Cases I and II (twins "A" and "O" and twins "E" and "G") the twins were very different intellectually but very similar temperamentally, in my Case (twins "C" and "O") and Muller's case the twins were decidedly similar intellectually and very different temperamentally. Thus the four cases seemed to fall into two categories: one in which the intelligence was modified without greatly altering the temperament, the other in which the temperament was modified without greatly altering the intelligence. Also in two cases (twins "A" and "O" and twins "C" and "O") there was a pronounced difference in physical condition, while in the other two cases (twins "E" and "G" and Muller's case) there was no such difference.

The present case (Mary and Mabel) differs sharply from all previous cases in that they are strikingly different in all three respects: intellectually, temperamentally and physically. This is all the more remarkabe since the environmental differences are less striking than in at least three of the other cases.

While Mabel, the farm girl, on the basis of the intelligence tests given, ranks so much lower than Mary, she makes an impression upon those who meet her of being decidedly intelligent. When the twins were with us, both Dr. F. R. Lillie and I thought that Mabel was the abler of the two. She always took the lead and did the planning for the pair. She seemed much more communicative and took the major share in conversation. She seemed to dominate her sister in every way and gave the impression of having the stronger personality. Certainly she has as much "sense" as Mary, if we give that term its accepted usage. Her mind, however, has not been trained to the kind of performance required in intelligence tests. There may therefore be some ground for skepticism as to the validity of these tests as measures of mental ability. Undoubtedly they test more accurately the effects of training than of native endowment. The same is probably true of the temperament-emotional tests, in that they test the effects of emotional experience, rather than native temperament, if there be such a thing.

Nevertheless, the tests do show very definitely that different training, different experiences, and different modes of living profoundly affect the intellectual, temperamental and physical characteristics of the individual. They have a considerable share in determining a given person's mental powers, his character, and his physique—in a word, his individuality. Hence we may conclude that training, social contacts, and living conditions, the main elements of the human environment, actually do have an important influence in determining what kind of person one becomes.

But it would be a great mistake to overemphasize the environment at the expense of heredity, for the degree of similarity that persists in all these twins, even though the environment has worked upon them in diverse ways, is even more remarkable than are the differences noted. Moreover, many of their characteristics, such as coloring, dentition, hair characters, features, general bodily peculiarities, voice, gestures, palm and finger patterns, ears and other traits too numerous to mention, are evidently almost purely hereditary in the sense that they are unmodified by existing differences in the environment. Some of their other character differ-

ences, such as those of body weight, muscular development, state of complexion, mannerisms, gait, and a few other traits, seem to be largely environmentally determined, in the sense that they are very plastic and easily modified by actual differences in the environment.

Mental characteristics seem to fall between these extremes. They are sometimes greatly modified in one respect, sometimes in another respect, sometimes in several respects. There is no general rule that applies to all the five cases of twins so far studied. One can, however, reach some estimate as to the relative potency of heredity and environment in moulding mental traits by comparing the averages taken in certain tests by fifty pairs of identical twins reared together, fifty pairs of fraternal twins reared together, and the four cases of this series of identical twins reared apart. Let us compare the data of these three groups with respect to the Stanford-Binet test, the Otis Self-Administering Test, and the Stanford Achievement Test.

In the *Stanford-Binet Test* the fifty pairs of *identical* twins reared together showed an average difference in "mental age" of 8.4 months, and an average difference in I. Q. of 5.3 points. The fifty pairs of *fraternal* twins reared together showed an average difference in "mental age" of 15.9 months and an average difference in I. Q. of 9.9 points. In each item the identical twins were only about twice as similar as the fraternal twins. This in itself showed that heredity has a large influence in determining mental status, for the environment was the same in both kinds of twins.

The differences in the four cases of identical twins reared apart are as follows:

	Mental Age Difference	*I. Q. Difference*
Case I	23 months	12 points
Case II	23 months	12 points
Case III	3 months	2 points
Case IV	34 months	17.7 points
Total	83 months	43.7 points
Average	20.7 months	10.9 points

This means that the average difference between *identical* twins *reared apart* is over two and a half times as great as the average difference of identical twins *reared together* for mental age, and over twice as great for I. Q.

More striking even than this is the fact that the average difference in mental age between *identical twins* reared apart is 5.2 months greater, or nearly 30 per cent greater, than the average difference of *fraternal twins* reared together.

In the *Otis Self-Administering Test,* the average differences in I. Q. in fifty pairs of *identical* twins reared together was 4.5 points; for fifty pairs of *fraternal* twins reared together 9.2 points. Thus identical twins are more than twice as similar as fraternal twins, again demonstrating the power of heredity.

The difference in the four cases of identical twins reared apart are as follows:

	Difference in I. Q.
Case I	18 points
Case II	15 points
Case III	10 points
Case IV	14 points
Total	57 points
Average	14.25 points

In this test the average difference between identical twins *reared apart* is over three times as great as that between identical twins *reared together*. Also the average difference of *identical* twins reared apart is over one and a half times as great as that of *fraternal* twins reared together.

In the *Stanford Achievement Test* the average difference in mental age between fifty pairs of identical twins reared apart are as follows:

	Difference in Educational Age
Case I	19 months
Case II	38 months
Case III	16 months
Case IV	34 months
Total	107 months
Average	26.75 months

Thus the average difference in educational age in identical twins *reared apart* is over four times as great as that of identical twins *reared together*. In the Stanford-Binet and the Otis Self-Administering Tests, it should be remembered, the differences between identical twins reared apart were only 2 and 2½ times greater, respectively, than the average of identical twins reared together. Hence there is much greater difference when achievement is *specifically* measured than when some attempt is made to eliminate achievement. It will also be noted that the greatest differences in educational age appear in Cases II and IV, where there was a very pronounced difference in the educational experience of the twins; the greatest difference in educational age being in Case II, where there was the widest divergence in educational experience.

Conclusions

It is still too early to attempt to draw general conclusions on the basis of only five cases of identical twins reared apart, but some facts confront us that cannot be dodged. Perhaps the most disconcerting fact that has been revealed by the data is that *fraternal twins reared together are on the average about one and a half times more similar in mental rating, as judged by intelligence tests, than are identical twins reared apart*. This seems to mean that when the heredity is different and the environment the same, twins tend to become more similar than when the environment is different and the heredity is the same. Extreme environmentalists may see in this situation evidence that environment is more powerful in determining mental capacity than is heredity. I do not believe that this is true, and for several reasons. First, if environment were more powerful than heredity, *identical twins* reared together should not be twice as similar as *fraternal twins* reared together. Again, fraternal twins differ as much in mental characters as they do in physical.

Second, all the tests used to determine mental ability, except possibly the International Test, measure largely the effects of training. This tends to mask or cover up hereditary resemblances and differences, increasing the differences in cases where educational experiences are widely divergent and decreasing the hereditary differences (in fraternal twins) where educational experiences are alike. Thus the tests used tend to exaggerate the potency of the environment and to minimize the potency of heredity.

In spite of these weaknesses in method, however, the data at least warrant the conclusion that both heredity and environment play important rôles in determining mental status. Insofar as a person's mental status is measurable in terms of ability to perform well or poorly on intelligence tests, it seems fair to conclude from our data that differences in heredity and differences in environment are about equally responsible for the state of the mind of an individual at the time the tests were given.

On the other hand, since a large part of what is measured by intelligence tests is merely achievement, and not mental power, some will say we are not really measuring mental status at all. There is doubtless some justification in this criticism of intelligence tests, yet I cannot help but believe that one's individual mind, his mental rating, is at any time the product of his hereditary make-up and his training. If this be true, it is only fair to rate him according to this product.

In spite of all this apparent evidence that hereditary differences and environmental differences are of about equal potency in determining mental status, I have a deep-seated conviction, which I am unable completely to justify, that differences in heredity are considerably more influential, perhaps twice as influential, in determining one's mental status, as are differences in environment. If this were not true, why should identical twins reared together be more than twice as similar, on the

average, as are fraternal twins reared together?

In conclusion, let me once more emphasize the importance of locating and studying further cases of identical twins reared apart. If any such cases be reported to the writer, every effort will be made to reach and to study them.

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TABLE I. Physical Characteristics of Mary and Mabel

	Mary	*Mabel*
Height	65¼ inches	66¾ inches
Weight	110¾ pounds	138½ pounds
Hair color	medium brown	a little lighter
Hair form	straight and soft	same
Iris color	medium brown	same
Skin color and texture	fair, but not so clear as Mabel's	fair and clear
Ear Shape	small, close to head	same
Handedness	right-handed in everything	same
Hair Whorl	on the right side of crown and clockwise	same
Features	nose long and narrow, mouth small, lips rather thin, chin prominent; features unusual	same except that face is fuller
Dentition	all teeth regular and in good condition for age	same
Alecranon to finger tip	L. 46.5 cm.; R. 46.0 cm.	L. 46.5 cm.; R. 46.3 cm.
Fibula	L. 37.8 cm.; R. 37.7 cm.	L. 38.6 cm.; R. 38.75 cm.
Circumference fore arm	L. 17.7 cm.; R. 17.65 cm.	L. 19.2 cm.; R. 19.1 cm.
Circumference calf	L. 31 cm.; R. 31.1 cm.	L. 34.1 cm.; R. 34 cm.
Foot length	L. 23.5 cm.; R. 23.4 cm.	L. 24.5 cm.; R. 24.2 cm.
Foot width	L. 7.8 cm; R. 7.7 cm.	L. 9.0 cm.; R. 8.8 cm.
Hand width	L. 7.4 cm.; R. 7.4 cm.	L. 7.7 cm.; R. 7.8 cm.
Eye defects	R. far-sighted, wears glasses	same, but glasses less strong
Use of eye in sighting	uses left eye	uses left eye

THE OPPORTUNITY OF SCIENCE

THE present economic debacle is a tragedy that in one way or another affects practically the entire human race. The economists and the "business surveys" are still, after two years, hailing each new "low" as the rock-bottom from which recovery springs. Each succeeding prophecy has an added note of plaintiveness, and it is evident that no mere cyclic variation is upon us, but a major economic and sociological disaster, that can only be met by brilliant and fundamental analysis and by drastic *and considered* action. Even palliatives are not being applied, and the fundamental problems are ignored or go unrecognized.

The disproportionally small Body Scientific should be the most promising source of the catalytic *ideas* which are our only hope of leavening the discouraging mass of tradition and inertia which holds us helpless in the present crisis. The following leading editorial appeared under the heading of "National Needs" in *Nature* (London) of November 14th. It is a remarkable discussion of some neglected aspects of the present situation. Its essentials transcend national boundaries, and with a few changes of names and places its message applies to every industrial nation today,—even to a nation over-stocked with wheat and cotton (but with millions underclothed and verging on starvation!).

While some of us may wonder just how scientific workers are to become "militant in every quarter" we must all agree that the need is there, and that it is generally not acknowledged. If a responsibility of this kind exists it applies especially to those whose discipline is heredity. All about us radical social and economic experiments are being suggested, nay demanded, and in practically every instance wholly without taking into account the quality of the people who are to form the basic element of these experiments. The failure to mention heredity in the recently promulgated "Children's Charter" is after all only a symptom of the almost universal attitude today. The Body Social either does not know of the demonstrated facts of heredity, or it dare not face them. It prefers to cling desperately to witch-doctor emotional formulations that are pathetically inadequate to exorcise the destructive tendencies with which we have been brought face to face by a machine age, and an essentially inhuman economic system.

Professor Armstrong's call to scientific thought and action is issued none too soon. Every scientific worker will appreciate the difficulties of heeding it, but this does not absolve us from a responsibility which must somehow be met. "The hungry sheep look up and are not fed." Perhaps science has no message for hungry sheep; if this is the case we may well ask whether science is really a technique for discovering and interpreting important facts, or has there been a fateful neglect of the field of human welfare?

If science is able to produce only machines, and if its generalizations deal mainly with such trivialities, from the practical point of view of racial welfare, as nebulae or protons, we may question whether science is really more than another epiphytic excrescence on a social organism that in many other respects is cooperative only in theory and dangerously parasitic in fact. If we have social insight we must deliver our message; if not in words of one syllable, at least in a form that on the average is less reminiscent of Taine's cynical remark about the use of language to conceal our thoughts. "Plan or Perish."—the alternatives are clear, and they appear to be tragically urgent. Can science, or rather scientists, show the way?—EDITOR.

▼

THE present sore trouble of the world, which has long been surely coming upon us owing to the inconsidered action of all nations, will not be without its compensations, if we be led to probe into the causes and to reorganise our operations upon considered lines.

The call is upon us all to turn our attention to affairs of State. Scientific workers especially have to recognize that they have been selfish in their too exclusive devotion to experimental study and the immediate development of their discoveries; that they have soared entirely above the heads of the community and in no way prepared the masses to understand the profound changes brought about in our life by the scientific forces at work in industry. Brains and brawn were never so far apart as now. Although this is the case, it was never so clear that a wise socialism is foreshadowed as the only possible way of avoiding the downfall of our civilization. In some way or other, the nations must come to work together. Competitive industry, as we have known it in the past, is ceasing to be possible: it pays no one and is but a disguised civil war. Mechanisation is bringing about its own defeat: the cost of displacing human labour is becoming prohibitive. Man is ever there; capital will not long be, at present rates of taxation: we shall soon be forced to return to *manu*factures.

The chief duty before us clearly is—to make all men work: a machine which is idle has no right to be. Nature itself ever works and necessarily dies if she cannot. Man alone of her works is ceasing to be natural and fast becoming a mere parasite.

The country is now in the hands of a Government elected upon a National ticket; the popular expectation is, that it will be Rational and rise superior to party. There is, however, an ominous preponderance of men of rigid outlook bearing political labels; few are known to profess a belief in government by scientific methods. Probably the Prime Minister alone can attach any real meaning to the term 'scientific.' He was long an attendant at Royal Institution lectures and grew up in a scientific atmosphere. Very remarkable, at the recent Faraday celebration in Queen's Hall, was the entirely sympathetic way in which he entered into the spirit of the occasion, outshining all other speakers in clearness of perception of the service rendered to the world by Faraday, especially the moral value of the example set by this great experimentalist. His father-in-law, we know, was the most sympathetic of Faraday's biographers. Probably Sir John Simon comes next to Mr. McDonald. He at least has breadth of outlook and a severely logical mind.

It is much to be feared that the men who have taken upon themselves to govern us cannot possibly fathom the depth of present-day world activities: the overwhelming complexity of the issues; the subtle problems they present; the hopeless difficulty of meeting the naturally selfish dictates of human nature. Earl Grey being all but out of action, there is no prophet in the field of politics. We need a Disraeli, a man of imagination, able to grasp our modern problems and be a constructive leader. If the recent elections have shown one thing more than another, it is that the country is prepared to follow a clear lead. In this particular. Mr. MacDonald and his small band of devoted followers have set a noble example.

Nothing could be more definitely painted upon the wall than the failure of our public schools to train the ruling classes in any proper way. The "science" introduced into them has been to no purpose—no effective teaching has been given in the logic of method. Consequently, the calculated practice of scientific argument—the habit of sound scientific thought—plays no part in our affairs; it has no considered place in our Civil Service. This is one of the matters of urgency, calling for fullest consideration, at the present moment. Unless and until our universities and schools are brought into harmony with the times, no real progress can well be made. Oxford most needs reformation: the present neglect of a broad culture by the University is a national danger. As to the schools, it should be deemed a criminal act to bring up a

boy or girl upon literary study alone, without training in the judicial method.

Our schools still teach little more than the worship of words and those largely words of the past. The men in charge of them, with few exceptions, have no feeling for the wide issues of practice and scientific leadership. We are a strange people: intensely practical in our general outlook, we place ourselves in the hands of those who cannot possibly become masters of the intricate situation which confronts us. Teachers as a class in no way grasp the responsibilities of their position, let alone the dangers of the times. They are too much of one school and, with rare exceptions, of an unadventurous type. Complaint of the inflexible nature of the material they produce is rampant everywhere.

Lafcadio Hearn, writing from Japan, in August, 1893, in an outspoken letter to Ellwood Hendrick, after remarking how terribly tragic modern life is becoming, directs attention to its hopeless contradictions. These, he says,

"can only be recognized and reconciled through a profound knowledge of social conditions, not in the abstract only but in the most complex operations. This is the theoretical recognition. The practical recognition requires special hereditary gifts—intuitions—instincts—powers. Mere education in business alone won't do. That only makes servants. Masters must be natural masters of men. Life is an intellectual battle but not a battle to be fought out by mere chess-combinations. It is also a battle of characters . . . . . I do not see much likelihood of moral development . . . . . morals have been at a standstill since the beginning of history: we have made no apparent progress in that. Then comes the question, Are we not developing immorally?"

There can be little doubt as to the answer. The Hollywood atmosphere in which the masses now live—the scorching, motor habit, developed as a mere means of escaping from the boredom of mental vacuity—the condition of the Press, which for the most part has no soul beyond sexuality and sport: all these things are proof sufficient that there is no real intellectual progress to be recorded. The masses have learnt to read—but "the hungry sheep look up and are not fed." The body scientific— those who hold the keys of knowledge—makes no calculated effort to provide them with food. The literary class, as a body, has no message; brought up upon words alone, with myopic uncultivated vision, it can only use words to describe the obvious.

Not so long ago, starting from Huxley and Herbert Spencer, a body of men representative of natural science, in its various branches, made a determined effort to galvanize the schools into practical scientific activity and make education an introduction to life: the attempt has been a failure. Scarce any of the band remain. Modern scientific workers—the professional researchers—no longer take interest in such matters. Occasionally someone kicks: Prof. Irvine Masson is the latest example. The bookmen hold the field: what public conscience there may be is stilled by free education and scholarship grants. We have little right to complain of politicians, so long as we make no effort to train them to their office by bringing about the revolution which every thinking man knows to be long overdue in our school system.

Meanwhile the industrial shoe pinches terribly. There isn't enough leather to make it a comfortable fit. We are so pampered into the belief that we can live on industrialism, that no real thought is given to the problem as a whole. The fetish of what is supposed to be cheapness is worshipped without any consideration of its consequences. No sense of the meaning and value of agriculture has been bred into the community.

The mathematicians can discuss the Unseen Universe with great popular effect. Yet they make no attempt to stem the ignorance of the money-changers or help them to devise a rational system of banking which will enable us to do without the gold we cannot have wherewith to back the counters issued as money—so that commerce may be controlled and its operations made facile, not the sport of spec-

ulators. The multitude obviously does not understand the meaning of the term *standard*: the professed economists have not the wide equipment that is needed for their task.

Men now need to be so trained that they learn to work together—without losing their souls. No trade today is free; some combine is at work which prevents each one from being fair. So long ago as 1894, Hearn could write to Hendrick prophetically:

"The tyranny of the future must be that of Organization: the monopoly, the trust, the combination, the associated company . . . . . much more powerful than the robber-baron . . . . .these are infinitely less human—having no souls."

Hearn spoke as an artist, of course. Still, his plaint is justified in a tyranny such as is being exercised over milk production, by the great combines now coming into action as universal purveyors. Water has long since passed into public control: milk some day perhaps will be seen to be of even greater consequence and cared for properly.

We have no food sense worth calling a sense: biology to this end is not taught in the schools; women decline to cook and are deluding themselves into a belief in canned foods. Our ignorance of all essential things is monstrous. Scared by a few bacteria, we systematically spoil our milk. We are even threatened with synthetic food. Machinery, using the sunshine of past ages, is to put the nose of our present sun out of joint and make the farmer a back number. Our only salvation is the natural horse sense that is in us.

* * * * *

In future, the scientific worker, to be worthy of the name, must justify himself through social service in the first instance. Our situation is so grave that he must be militant without delay and in every quarter.—H. E. A.

Nature: (London) Nov. 14, 1931.

The Science of Human Affairs the Most Difficult

There is another aspect of the relation of science to the community. The public conception of science is commonly limited to the mathematical, physical and chemical sciences, with the many departments of biology and their applications and in recent years, psychology. The great field of social studies is usually, at least in Europe, regarded as outside the field of science, with the single exception of economics. However, if prediction be the test of science, eonomics has not been, in the last few years, conspicuously successful.

It has been the aim of many thinkers to build up a science of human society, to embrace not only economics but all the activities of man. Sociology, to use the name coined originally by Comte, is the most difficult of the sciences, because it deals with phenomena of far greater complexity than any of the physisal or biologicol sciences. Few of its principles have yet been established, and it must still be regarded as a science in the making, but its importance is such as to deserve the attention of all students, in spite of the urgent claims made on their time by their chosen specialism. I believe that this fact is more generally recognized in America than in Europe, but nowhere is the treatment of social problems, such as those of politics and finance, scientific to the extent that would be desired by the sociologist, and it would seem that those who already employ scientific methods of reasoning with success in one field could render service in bringing about their extension to others.—Desch, Cecil H. Pure and Applied Science. *Science,* Vol. 74, No. 1925—Friday, Nov. 20, 1931. Page 500-1.

A PINK-FRUITED LEMON

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THE discovery of a striking fruit with color characteristics that are apparently new and that have a startling appearance is a matter of general interest. An illustration of this fact will be presented in this note which briefly describes a pink-fruited lemon and its origin so far as known.

A variegated limb variation was found by the writer in 1911 in an Eureka lemon tree located on the Chase plantation near Corona, California. The characteristics of this bud variation and progeny trees that were propagated from it were described in 1920.*

The variegated Eureka lemon parent-limb variation and its progeny trees have characteristics, both as regards foliage and fruits, similar to those of the normal Eureka limbs and trees excepting in the color of the bark, leaves and fruits. In the variegated bud sport the bark of the young branches is variegated with fine stripes of varying shades of green and straw color. The leaves are oval in shape, obtuse, of medium size, fairly abundant in number and variegated in color as shown in Figure 9. The white and green sections of the leaves occur in areas of variable size and shape, in some cases the leaves being almost or wholly white, as can be seen in the twig shown in the lower right side of the illustration, while in other instances they are nearly green.

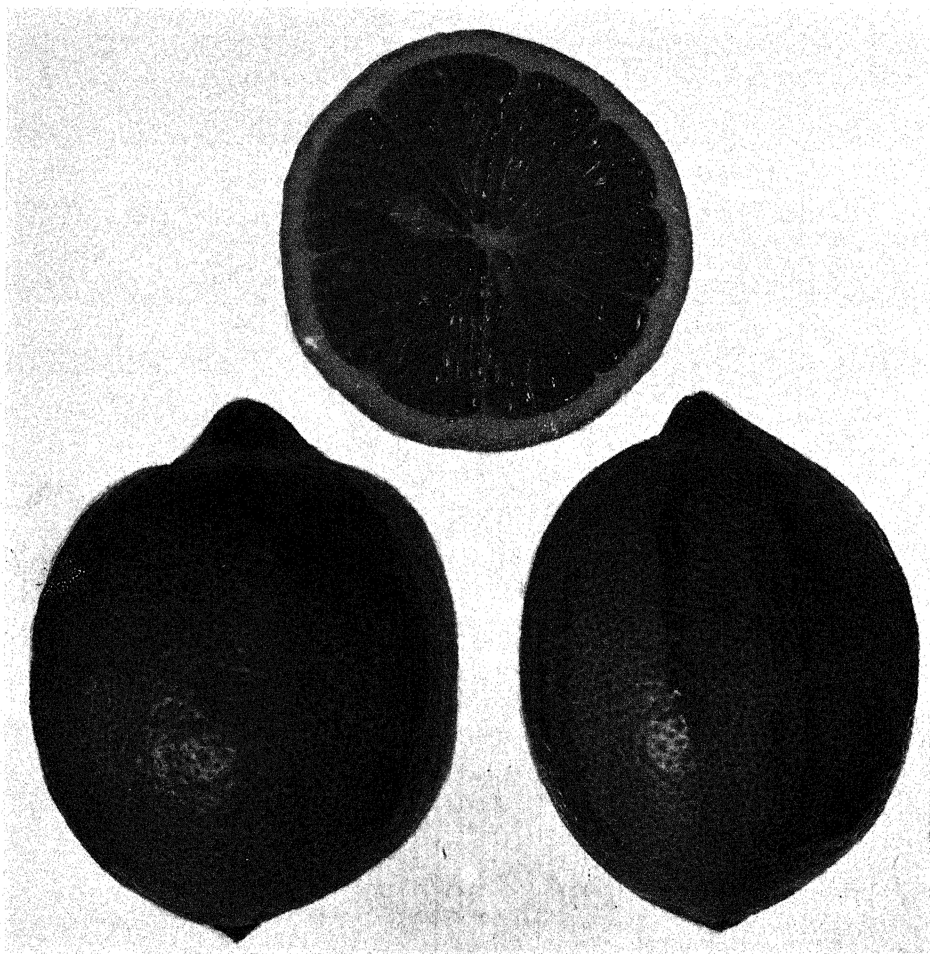
The fruits are oblong in shape, of medium size, rough or ridged in texture and have thin rinds. The rag is tender, and the juice is abundant but lacking somewhat in acidity. The seeds are similar to those of fruits of the Eureka strain. The lemons have a striped appearance, usually with alternate green and white areas. This appearance is correlated with a ridged condition, the ridges being usually green in color while the depressed spaces are white. The leaves, fruits and bark of the progeny trees of this strain originating as a bud sport have a similar variegated appearance. Trees of this strain are not as productive as those of the comparable normal Eureka strain. The very thin rinds and rough appearance of the fruits as well as the relatively low yields of the trees makes this strain of very doubtful commercial value but it is rather widely planted in some citrus growing areas of the Southwest for ornamental purposes. It has been found by the writer during the past twenty years occuring as individual fruit, limb and entire-tree variations of the Eureka lemon.

During the National Orange Show that was held at San Bernardino, California in February 1931, the writer noticed a typical variegated lemon among a collection of citrus fruit sports that was presented by the American Fruit Growers, Inc., as a feature of their general fruit exhibit. Upon examination this variegated lemon was found to have a pink-appearing rind and flesh that was not particularly apparent under the artificial light in the show building and it was largely unnoticed on account of the many strange specimens of citrus fruits with which it was grouped.

From information supplied by officials of American Fruit Growers, Inc., the parent tree from which this pink lemon was picked was located in a small planting of citrus trees in the grounds of the winter home of Mr.

*Shamel, A. D., et al., Citrus Fruit Improvement: A Study of Bud Variation in the Eureka Lemon, *U. S. D. A.* Bulletin 813, Washington, 1920.

LEAVES OF PINK-FRUITED LEMON
Figure 7
Showing range of variegation of leaves, from entirely green to completely white.



PINK LEMONS

Figure 8

Typical fruits of pink-fruited Eureka lemon from parent tre located at Burbank, Calif. The fruits, as they approach maturity have pink colored rinds, flesh and juice. This is the first pink-fruited variation of the Eureka lemon that has been reported. Photographed April 20, 1931.

D. W. Field at Burbank, California. This tree, the writer found on April 20, 1931, is identical in appearance and all characteristics observed with those of the Variegated Eureka lemon bud variation described in the foregoing paragraphs excepting that the fruits develop a decidedly pink color of rind, flesh and juice as they approach maturity (Fig. 8).

The pink-fruited Variegated Eureka lemon tree is about fifteen years old but it has not been possible to trace its origin definitely. It stands among neighboring orange and lemon trees of several varieties, some of which are obviously of the standard commercial varieties while others are unknown to the writer. It is a healthy, fine tree and apparently as productive as the Variegated Eureka lemon trees now under observation by the writer in

VARIEGATED LEAVES AND FRUITS

Figure 9

Variegated Eureka lemon limb variation in an otherwise normal Eureka lemon tree located on the Chase plantation, Corona, Calif. Such variegated sports are not uncommon in the Eureka variety, a number having been observed in the last twenty years. The pink-fruited form recently discovered resembles this more common variation except in color. Photographed August 21, 1918.

several Southern California lemon groves.

The pink color of the rind, flesh and juice was not observed in the young fruits but became increasingly evident in the older lemons on the tree until when the fruits had reached commercial size, this pink, reddish, or "blood" color is very conspicuous (Figure 8.)

In the pink-fruited tree several small branches were found that were apparently wholly green in color of bark, leaves and fruit but as it is very difficult to determine definitely this condition, the wholly green characteristics of these branches remain somewhat in doubt. Other small branches were observed where the stems and leaves were nearly if not wholly white but none of them bore fruits.

The pink color in the lemons is the only apparent characteristic that distinguishes this interesting variation from the Variegated Eureka lemons that have been under observation for years. From their study the writer believes that the pink-fruited tree is a bud sport of the Eureka lemon and is another illustration of the occurrence of striking bud variations in this variety of citrus fruit.

Budwood was obtained from the parent tree by the writer and inserted in sour orange seedlings at the Citrus Experiment Station of the University of California during the last week of April, 1931. It is planned to study the resulting progeny trees in connection with those of the Variegated Eureka lemon bud variation mentioned in the early paragraphs of this paper.

Books Received

BOOKS are acknowledged in this column as received, and such acknoweldgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

REPORT OF THE NINTH CONFERENCE OF THE INTERNATIONAL FEDERATION OF EUGENIC ORGANIZATIONS — Farnham, Dorset, England, September 11th to 15th, 1930. Pp. 100. Published by the I. F. E. O. Hon. Administ. Sec.: C. B. S. Hodson, 406 Fulham Road, London, England.

What's to do about eugenics in many corners of the globe, with papers on detailed problems and on specific traits: eye color; blood groups; racial psychology; race crossing, etc., etc.

LA THEORIE PHYSCIO-CHEHIQUE DE LA SEXUALITE, *by* PH. JOYET-LAVERGNE. Pp. 102. Five chapters. Price, 10 fr. G. Doin & Cie., Paris. 1932.

Some more sex-determination *theories.*

CHILDREN'S BEHAVIOR PROBLEMS, A Statistical Study Based upon 5,000 Children Examined Consecutively at the Illinois Institute for Juvenile Research, *by* LUTON ACKERSON, Research Psychologist, Institute of Juvenile Research and Behavior Research Fund, Chicago. Pp. 268. Fifteen Chapters. 57 Figures. Price, $4.00. University of Chicago Press, Chicago. 1931.

All the things that 4,592 kids could think of to do (one of them did nearly three pages in small type, from *bad table manners* to *vomiting spells* and *plenty* between), correlated with *IQ, CA* and what have you. The subhead of the book is "Incidence, Genetic and Intellectual Factors," but neither "genetic," "hereditary," "family," nor "inheritance" appears in the index. There are two pages of conclusions which we couldn't seem to get anywhere with, but the "study is being continued with a large number of possible causal factors," Better luck next time!

CHILDREN WHO RUN ON ALL FOURS, And Other Animal-like Behaviors in the Human Child, *by* ALES HRDLICKA, M.D., ScD., D.Sc.NAT., Curator, Division of Physical Anthropology, U. S. National Museum, Smithsonian Institution. Pp. 418. 13 Plates, 27 Figures. Price, $5.00. Whittlesey House, McGraw Hill Book Co., New York. 1931.

That's one thing those Chicago kids didn't think of. If they had there would have been another chapter. That our racial family history has a bearing on this way of getting around is a fruitful hypothesis, and a similar hunch, buttressed by a few thousand family histories might have proved more than all those graphs and statistics in the Chicago study.

POPULAR QUESTIONS ANSWERED, *by* George W. Stimson. Pp. 426. Price, $2.00. George Sully & Co., Inc., New York. 1930.

Debunking a few hundred superstitions and accounting for a few hundred queer practices. Quite a hodgepodge, but an interesting collection of the absurd in mental luggage with which we are wont to load ourselves. Among them many biological superstitions, on the average quite well handled.

ANNUAL REPORT OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION, Showing the Operations, Expenditures, and Condition of the Institution for the Year Ending June 30, 1930. Pp. 650. Price, $2.00. U. S. Government Printing Office, Washington. 1931.

Articles by Davenport on the Mechanism of Organic Evolution, and on Extra Chromosomes by Blakeslee are of especial interest genetically.

THE CRIMINAL, THE JUDGE, AND THE PUBLIC, A Psychological Analysis, *by* Franz Alexander, M. D., Visiting Professor of Psychoanalysis at the University of Chicago and Hugo Staub, Attorney at Law, Berlin. Translated from the German by Gregory Zilboorg, M. D., Bloomingdale Hospital, White Plains, N. Y. Pp. 238. 11 Parts, 16 Chapters. Price, $2.50. The Macmillan Co., New York. 1931.

Ever since Cain (the working out of whose Oedipus complex is made plain on P. 205), those of us who commit murder and whatnot appear to be behaving exactly according to Freud, and if we get adequately "psyched" in time we will be so busy with our subconscious we won't have time to be murdering folks.

THE DECLINING BIRTH RATE IN ROTTERDAM, A Statistical Analysis of the Drop in the Number of Children in 24,664 Rotterdam Families during the Last 50 Years, *by* J. Sanders, M. D. Pp. 180. 29 Charts. Price, 6 Guilders. Martinus Nijhoff, The Hague. 1931.

What nearly twenty-five thousand Dutch families have done in the way of perpetuating the race in the last half-century. The present incumbents are only 30 per cent as efficient as their forebears, and this is definitely not due to decreased biological fertility. A statistical study that *has been productive*, and of many extremely interesting conclusions.

THE ANTHROPOMETRY OF THE AMERICAN NEGRO, *by* Melville J. Herskovits. Pp. 283. Eight Chapters. 102 Tables, 34 Figures. Price, $4.00. Columbia University Press, New York. 1930.

Plenty of statistics, proving that more are necessary, or that in the absence of any special purpose just data don't get us very far.

A SHORT HISTORY OF THE AMERICAN NEGRO, *by* Benjamin Brawley. Pp. 311. Eighteen chapters. 3rd Revised Edition. Price, $2.00. The Macmillan Co., New York. 1931.

Up from slavery, and beyond. "It is also increasingly evident that the real leadership of the world is not a matter of race, or even of professed religion, but of principle."—Oh, yes?

Is Education Improving?

EDUCATION, CRIME, AND SOCIAL PROGRESS, *by* William C. Bagley. Pp. 150. 8 Chapters. Price $1.20. New York, The Macmillan Co. 1931.

It is the main thesis of the book that in spite of the vast number of young Americans subjected to the presumably beneficent attentions of today's so celebrated alchemist, the educator, the results in terms of good citizenship and social progress are very disappointing.

The measurement of our social limitations is made in terms of crime ratio comparisons with England, France, and even with puny and supposedly benighted nations, all of whom we greatly

excel in the amount and probably, too, in the variety of our crime. The author neatly forestalls any explanation of this in terms of the breakdown of formal religion by J. R. Miner's statistical demonstration of how the free thinking northern states are outdone, in a general breaking of the Commandments, and especially the tenth, by the God-fearing South. For good measure the author adds divorce rate comparisons in which the two great political unions (U. S. and U. S. S. R.) easily lead the field in the granting of legal dissolutions of marital ties.

While agreeing that we must look for the ultimate cause of crime and divorce in the individualistic selfishness and hedonism of the modern American, the author's contention is that our educators have been recreant in their duty of instilling a sense of personal obligation and an appreciation of the necessities of order, discipline, and effort in the young. Rather there has been a mad pursuit of new methods of teaching, and new curricula, full of promises of intellectual mastery with minimum effort; so many roads that wind and wind and lead nowhither. It is heartening, to the reviewer, to have an educator thus expose the twaddle which so often passes for modern educational progress.

The other matter discussed, second in terms of textual space although it is the first introduced to the reader, is that of technological unemployment. Professor Bagley feels that education should better fit people for proper utilization of leisure and for different sorts of employment. Here, again, mastery of basic subjects rather than a superficial knowledge of soon-to-be-obsolete specialties is the desideratum.

The statistical facts presented in the book cannot, of course, be denied. Most people will agree, at least in principle, with the inferences which Professor Bagley has made. It is, I believe, at this juncture that one little mouse asks: "And who is going to bell the cat?" The method by which Professor Bagley's ideals are to be attained is not very obvious at present, in spite of the commendable analysis of the problem with which he confronts us.

R. R. Huestis,
University of Oregon.

Inspirational Heredity

LIKE BREEDS LIKE, A Non-Technical Treatise Covering Heredity, Live Stock Breeding and Eugenics, *by* Harry H. Cook. Pp. 383. 57 Chapters. 77 Illustrations, 21 Tables. Price, $3.50. Research Department, Sans Aloi's Jersey Farm, Ontario, California. 1931.

THREE hundred and eighty-three pages of unbridled enthusiasm for the breeder's art and of reiterated expressions of confidence in the importance of heredity. Non-critical throughout and full of an excess of superlatives almost to the point of incoherence in places, the book seems to be a lineal descendant of Wiggam's *"Fruit of the Family Tree,"* not one whit diminished in enthusiasm or desire to evangelize nor one bit increased in the scrutiny and verification given to each statement. The biblical allusions are abundant and may materially enliven an otherwise idle hour for one to whom this book is handy. It is delightful to see a man devote one chapter to paleolithic and neolithic man and the next chapter to tracing the descent of the present races of man from Noah and his three sons. The author is somewhat puzzled at how to derive from a single family such diverse races of mankind as now exist and partially satisfies himself with the conclusion that Ham in order to have been the progenitor of such races as the negroes and the primitive Australians (!) must have been from a dark skinned mother and therefore was only a half brother to Shem and Japheth. One of the interesting features of the book is the pedigree of Moses showing 6.5% inbreeding and tracing in twelve lines to

Terah. Moses had 8.2% of the blood of Terah although the latter was a great, great, great, great grandfather in the nearest line. (These calculations of course rest upon accepting at face value the story of Pharaoh's daughter as to how she came in possession of Moses!)

More than half of the book is devoted to sketchy histories and descriptions of common breeds of livestock. There are many typographical errors such as zebra where zebu was obviously intended, a reference to the Clydesdale stallion, Boron of Buchlyvie, and the statement on page 259 that "—sperm from the semen of the male ignites with the ovum of the female—."

At any rate the author, who is a successful breeder of jersey cattle, believes in the power and majesty of the law of heredity (although he never does just exactly define the law!) and the book will appeal to some as easier to read than a more coherent one, technically accurate in details would have been.

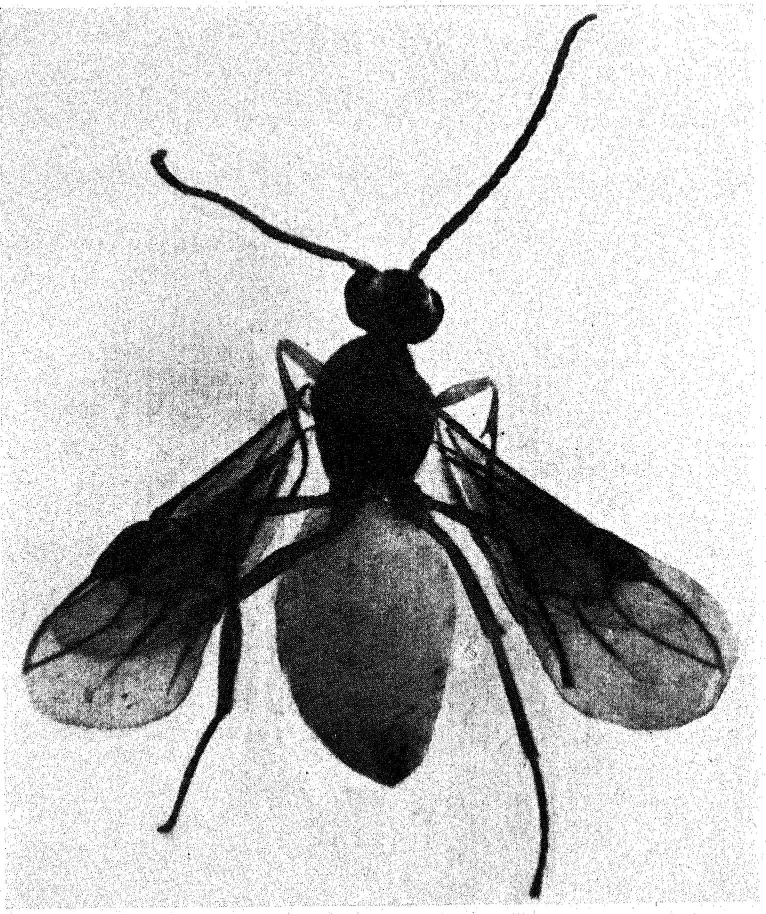
JAY L. LUSH,
Iowa State College.

Heredity and Diabetes—A Medical View

Prof. Marcel Labbé, in an address delivered before the Academy of Medicine, Paris, discussed the hereditability of diabetes, in which field he has had a long experience. According to the majority of writers, such hereditability, long since recognized, exists in 25 per cent of the cases. A distinction is made between hereditary and familial diabetes, but in reality they have the same significance. Diabetes comes under an hereditary system that has functioned for centuries according to the laws of Mendel. Labbé distinguishes benign diabetes without denutrition, and grave diabetes with denutrition. The former is usually acquired as a result of prolonged superalimentation; the latter without known cause. Under the various forms of familial diabetes may be distinguished diabetes due to an hereditary influence and diabetes due to wrong familial habits of eating, which lead to superalimentation. In a hundred cases, Labbé and Landau found twenty-three cases of hereditary diabetes. Evidences of heredity are found in 35 per cent of the grave cases of diabetes and in 20 per cent of the benign cases. The rôle of paternal heredity is much greater than that of maternal heredity, for the reason that a diabetic woman rarely has children. The hereditary influence is, however, sometimes overcome, to a certain extent. Labbé stated that he had seen six children born of mothers with grave diabetes. While these children have not yet shown diabetic symptoms, it is possible that the disease will appear later. The hereditability of diabetes now is conceived of as the transmission of a functional insufficiency of the islands of Langerhans. It is difficult to understand why the insufficiency of the islands becomes apparent at different ages and remains latent for varying lengths of time. Hereditary transmission entails certain deductions with regard to the marriage of persons with diabetes. Some physicians prohibit marriage. In Laddé's opinion, in the majority of cases, diabetic persons who take good care of themselves may marry. Young diabetic women should, however, avoid pregnancy, as it is always dangerous for them.—*Jour. Amer. Med. Assoc.*, Oct. 10, 1931.

FERTILE GYNANDROMORPHS IN HABROBRACON

P. W. WHITING and EDWARD J. WENSTRUP*



A MOSIAC WASP

Figure 10

A mosaic male produced by a virgin mother heterozygous for two factors, one causing the antennae to be shortened and tapering, the other causing the basal segments of the antennae to be yellow. The right antenna is shortened and yellow at base, the left is normal. Such mosaics are due to the taking part in development of the polar body, in which one set of chromosomes is ordinarily discarded in the formation of an egg cell. In a case such as this, one-half the wasp is the genetic complement of the other half, in all genes for which the mother was heterozygous. × 21.

IN the parasitic wasp, *Habrobracon*, unfertilized eggs develop into males which are haploid in chromosome constitution and resemble their mothers in hereditary traits. Fertilized eggs usually produce females which are diploid and of biparental inheritance. Virgin females, heterozygous for any

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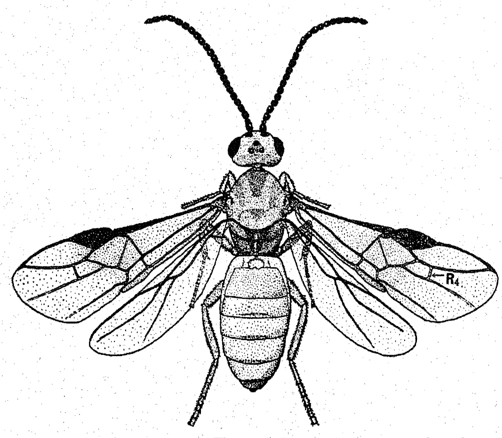


Figure 11

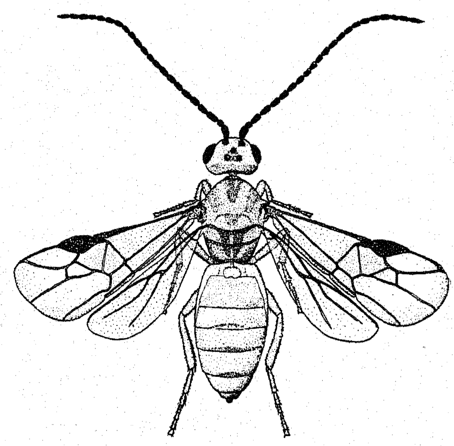


Figure 12

NORMAL AND MUTANT WASPS

At left the normal form; at right the mutant form called *long*. The factor long causes lengthening of antennal segments. The wings are curved ventrally toward the tip and also shortened distally as may be seen in the radial cell above vein R_4. $\times$ 10.

one of the numerous mutant factors which have been found, ordinarily produce males of the two expected types in approximately equal numbers.

Unfertilized eggs from heterozygous mothers occasionally develop into mosaic males. A male mosaic shows in different parts of his body, diverse traits inherited from his mother. We have abundant genetic evidence that these males arise from binucleate eggs differing in allelomorphic factors. One of the nuclei corresponds with the second polar body, the other with the reduced egg nucleus. An understanding of the origin of these males is essential to an understanding of the nature of gynandromorphs. The frequency of their occurrence is about one in 500 to one in 5,000 males bred from heterozygotes, varying according to stocks involved and other conditions.

Figure 10 illustrates such a mosaic (No. 516) from a mother heterozygous for a recessive factor causing shortening and tapering of antennae, *ta*, and a dominant causing the basal antennal segments to be yellow, *My*. One nucleus of the binucleate egg giving rise to this male contained factors *Ta* and *my* for normal antenna, the others *ta* and *My* for tapering, yellow antenna. Both nuclei took part in parthenogenetic cleavage, the former in this case producing the left half of the body, the latter the right.

Occasionally a sex-mosaic or gynandromorph is found among the progeny of a mated female. The frequency of occurrence of gynandromorphs among females is comparable with that of male mosaics among males. Male parts of gynandromorphs bear the hereditary characters of the mother while female parts show the dominant traits of both parents. It may then be said that these female parts have, like females, two parents, while the male parts have, like males, a mother but no father. Evidence shows that gynandromorphs arise when one of the nuclei of a binucleate egg is fertilized.

In comparison with females, males have longer antennae, readily noted with the unaided eye; larger ocelli and smaller abdominal sternites. Genitalia of the female include a pair of sensory appendages and a sting. Males average somewhat darker in general body pigment than females and have smaller legs and wings but these more

variable characters cannot ordinarily be used to distinguish sex.

Figure 11 shows a normal male and Figure 12 a male of the mutant form long, *l*, so called because the antennal segments are considerably lengthened. This factor also has the effect of shortening the wings distally and causing them to curve ventrally rather than being flat. The shortening is especially noticeable in the radial cell above vein R_4.

Figure 13*A* shows a gynandromorph (No. 348) with male head and female abdomen. The mother was homozygous for long, *l*, while the father was non-long, *L*. Antennae are male and haploid with segments lengthened due to factor *l*. The wings, however, show no shortening nor ventral curvature and the radial cell is of normal length. Influence of factor *l* is therefore obscured in the wings by the dominant allelomorph, *L*, brought in by the sperm and the wings may be regarded as female like the abdomen. Both parents had ivory eyes and consequently the gynandromorph shows this trait. Gynandromorphs with male heads and female abdomens have been found more frequently than other types.

Figure 13*B* shows a gynandromorph (No. 514) found by C. H. Bostian. This insect has female head and predominantly male abdomen. There are, however, female islands in the abdomen as may be seen in the photograph; the second right sternite and the first and third left are large and unmistakably female. The mother had cantaloup, *c*, eyes while the father was orange, *o*. Cantaloup and orange are both light, reddish and somewhat variable and stand in marked contrast to the jet black type reconstituted in the heterozygous, *OoCc*, female eyes and ocelli of the gynandromorph.

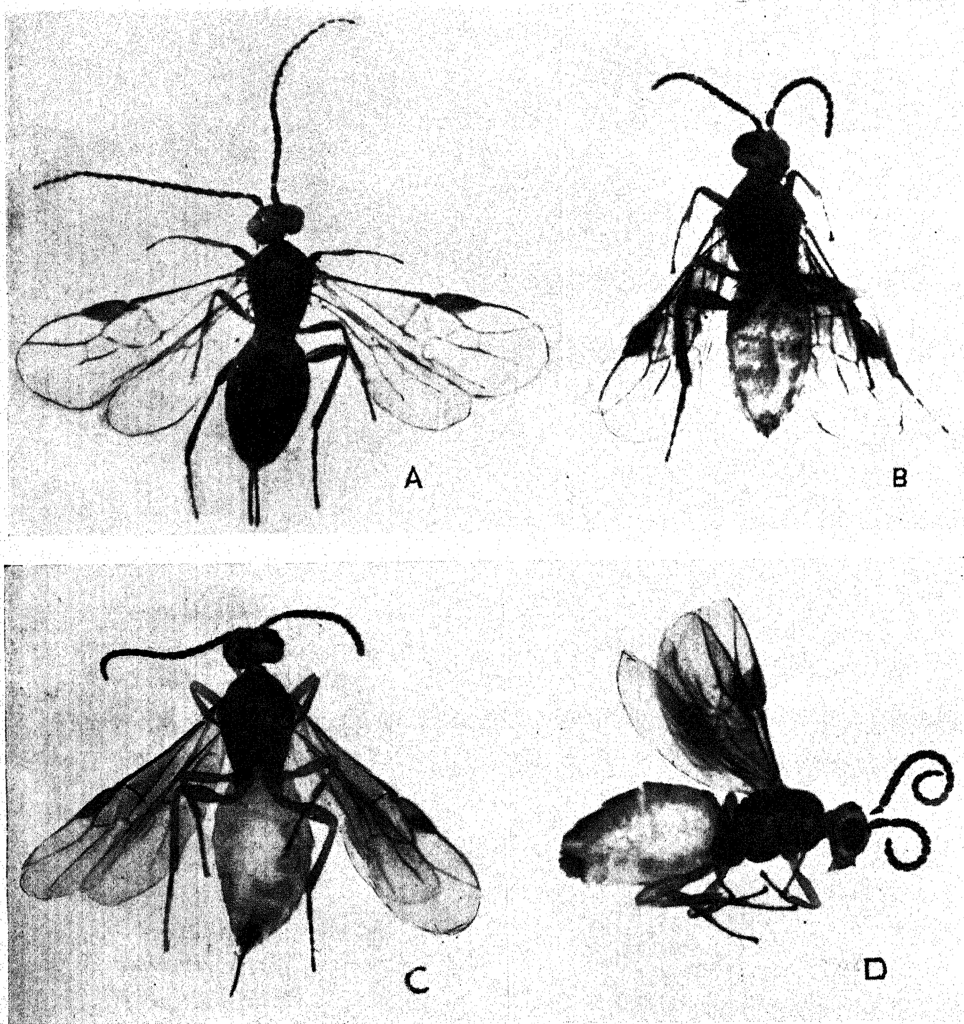
A third type of gynandromorph (No. 446) is shown in Figure 13*C*. The right side of the body is male, the left female, as may be seen in antennal length, in wing size, in abdominal sternites and in genitalia (Figure 14). This individual came from a cross of orange female with defective wing vein R_4 (Figure 11) by type male with black eyes and normal R_4 vein. The right eye is orange, the left black; ocelli are orange and male. Left primary wing has slight break in R_4 and is somewhat larger than right. It is therefore probably diploid, female, and heterozygous, *Dd*, while right primary with its greater break in R_4 is haploid, male, and defective, *d*, genetically. Secondary wings are symmetrical. The sperm brought in the dominant allelomorphs to orange and to defective and consequently the female side is *OoDd* while the male side is derived from the unfertilized nucleus *od*.

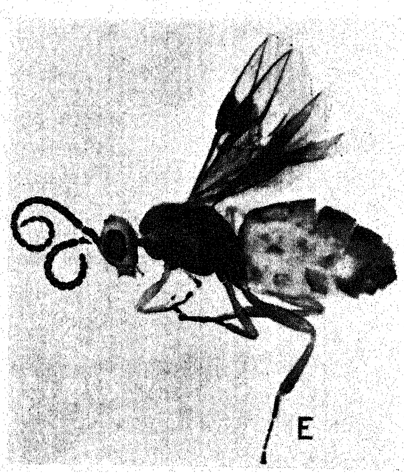
Instincts of Gynandromorphs Governed by the Head

Reproductive instincts of gynandromorphs are governed largely by the head and consequently it has been impossible to secure offspring from most of them. If the head is male, the insect with a female abdomen will attempt vainly to mate with females but shows no interest in caterpillars, will not sting them, feed on them or lay eggs. In fact the eggs of such an insect degenerate for want of the proper nourishment, the juice of the host caterpillar. A gynandromorph with female head will, on the other hand, attempt to sting caterpillars, despite its lack of a sting, and to lay eggs, despite its male abdomen. Of the three gynandromorphs described, the second only (No. 514) was tested. This specimen was female in instinct, indifferent to females and reacting to caterpillars.

In all of the 52 gynandromorphs tested only six have been found with instincts and genitalia of the same sex. These will now be discussed in some detail.

A gynandromorph (No. 425) found

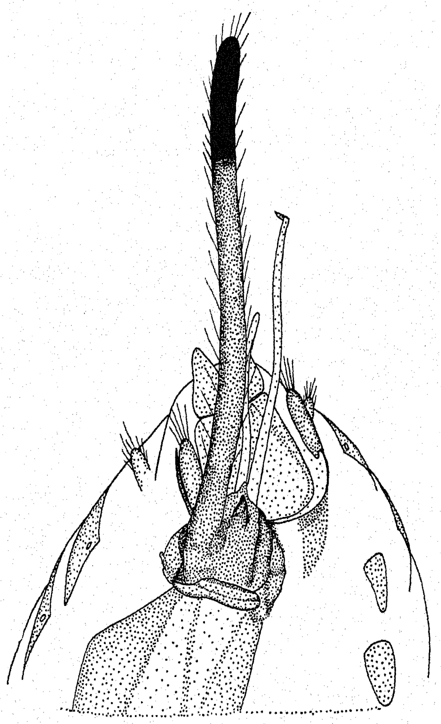




MIXED MALE AND FEMALE

Figure 13

Gynandromorphs showing various combinations of male and female components. Detailed descriptions of each of these will be found on the opposite page. The sex of the head determines the complicated instinctive reactions of these individuals regardless of the sex-constitution of the rest of the body. $\times$ 14.



STING AND CLASPERS

Figure 14

External genitalia of gynandromorph shown in Figure 13*C*. There is a complete set of male organs including first and second pairs of claspers and penis. Female organs are those of the left side only including the elongate sensory appendage and two elements of the sting. × 100.

by M. M. Torvik has left side (Figure 13*E*) male anteriorly with orange eye and female posteriorly with large sternites. The right side (Figure 13*D*) is female anteriorly with eye mostly black and male posteriorly. Ocelli are male and orange as is also a small patch in the anterior portion of the right eye. External genitalia are male.

The mother was orange, *o*, the father cantaloup, *c*. Male parts are therefore haploid and genetically orange, *o*, as shown in eyes and ocelli, while female parts are diploid and heterozygous, *OoCc*, showing as reconstituted black in the eyes.

Thorough tests indicated that the specimen was in general negative to caterpillars, although at one time it gave a slight positive reaction. It attempted vigorously to mate with females but was unable to do so.

A second gynandromorph (No. 335) found by M. M. Torvik came from a cross of an orange-eyed female with short wings, *o sh*, by a type male, *O, Sh*. Left antenna is female, right, male. Eyes and ocelli are all black but the right ocellus is slightly larger than the left, due either to mixed origin or to modification by adjacent male tissue. The right wings are both much shorter than the corresponding left due not only to sex difference but also to

DESCRIPTION OF GYNANDROMORPHS SHOWN IN FIGURE 13

A

A gynandromorph with male head and female abdomen and wings. The mother had the factor long, the father had the dominant allelomorph. Antennae are haploid and show the matroclinous trait; wings show the dominant character brought in by the sperm to the diploid female parts. Compare figures 11 and 12. The specimen has ivory eyes, characteristic of its parents. × 14.

B

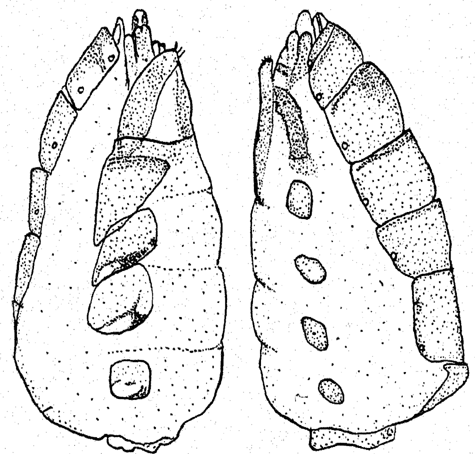
A gynandromorph with female head and predominantly male abdomen. The large abdominal sternites indicate islands of female tissue. The mother had light red eyes called cantaloup, *c;* the fatherhad orange eyes, *o*. The jet black eyes of the gynandromorph are due to the fact that the head is female, diploid, and heterozygous for both factors, *OoCc*. If this speciment had bred it might have produced cantaloup offspring, for its male tissues should be matroclinous. × 14.

C

A gynandromorph with left side female, right male. Note the shorter antenna, black eye, larger wings, more complete R_4 vein and larger abdominal sternites on the female side. The mother was orange with factor for defective R_4 vein. The father was type (black-eyed, normal veined). The female side shows the dominant paternal characters, while the male side is matroclinous. × 14.

D

This gynandromorph has the head mixed, male on the left side as may be determined by length of antenna. The orange eye and ocelli are of maternal origin. The black color of the right eye is female, a color reconstituted by combination of the dominant allelomorphs to orange and cantaloup, *OoCc*. The abdomen is opposite to the head, female on left, male on right. The instincts were male but the animal was unable to mate. × 14. *E* shows the other side of this same individual.



SEX DIFFERENCES OF TWO SIDES
Figure 15

The left, female side, and the right, male side, of a gynandromorph showing difference in sternites. × 40.

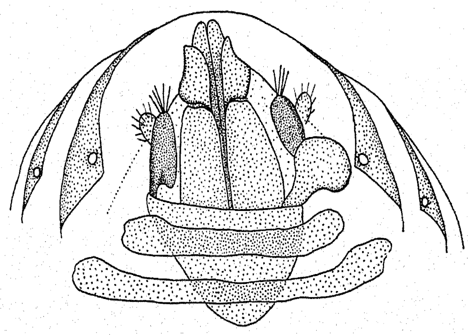
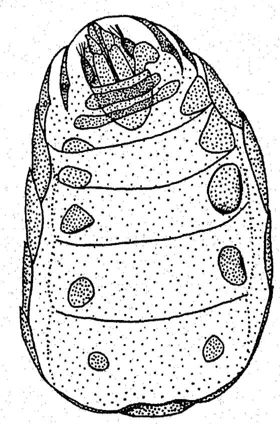


Figure 16

Male genitalia of a fertile gynandromorph. There is an extra clasper on the right side. × 100.



ANATOMICAL DETAILS OF FERTILE GYNANDROMORPH
Figure 17

Ventral view of abdomen of the fertile gynandromorph shown in Figure 16. × 40.

the factor *sh* received from the mother. Abdominal sternites are female on left side, male on right (Figure 15). External genitalia are normal for male.

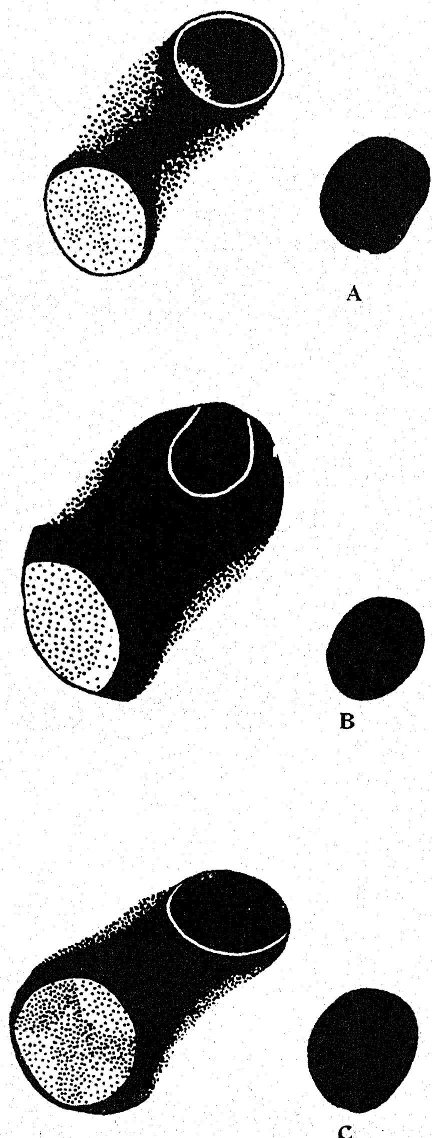
The reproductive instincts of this insect were male like its genitalia for it attempted many times to mate with females. It was unable to do so since it always thrust its abdomen to right of female's body and far forward.

The junior author has carried on extensive breeding tests of gynandromorphs found in a series of experiments in which females homozygous for orange eyes, *o,* and for defective venation, *d,* were crossed with type males. Success was attained in only four cases which follow.

Number 395 has left antenna male, right female. Left eye is orange, right black. Ocelli are orange and male. Primary wings are of same size and each has R_4 completely lacking. Left secondary wing is much smaller and therefore male. Back of head is darker on the left (male) side. Abdomen is in general female on left side, male on right, with male genitalia (Figure 16) which are normal except for what appears to be an extra clasper on the right. Sternites (Figure 17) are small (male) on left side, larger (female) on right.

This gynandromorph functioned normally as a male and was crossed with an ivory-eyed female. Ivory, o^i, is an allelomorph to orange, *o,* and hence the appearance of 30 orange daughters, oo^i, besides the 36 ivory, o^i, sons, indicated that testes were of maternal origin as expected. No record was taken of venation.

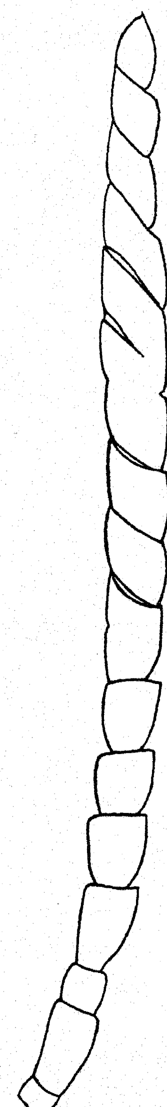
Number 475.5 has left antenna female, right male. Left eye is black, right is black with an orange spot ventrally. Left ocellus (Figure 18*A*) is orange and male with area about it black; median is mixed, black for the



DIFFERENCES IN EYE COLOR

Figure 18

Ocellar areas of three fertile gynandromorphs, Nos. 475.5, 448, and 503, respectively. In each case the left side is male with darker integument, but the large male ocellus of this side is lighter than the small female ocellus on the right. Male parts of eyes show the maternal orange color while female parts show the dominant paternal black. × 300.



FUSION OF SEGMENTS

Figure 19

Left male antenna of gynandromorph showing spiral fusion of segments. Irregularities of this type seem rather frequent in gynandromorphs. × 100.

most part but with light spot on left side; right is black and female with area about it yellow. Wings are symmetrical with R_4 lacking from both and hence to be regarded as haploid, male, and genetically defective, *d*. Abdomen is completely male. There is no asymmetry in body pigment.

This gynandromorph also functioned normally as a male and was mated to an orange defective female. There were produced 23 orange defective daughters in addition to 23 orange defective sons. The fact that the females were defective like their brothers, shows that the gynandromorph bred as might be expected if testes were of maternal origin.

Number 448 has left antenna male, right female. Left eye is black ventrally, orange dorsally. Right eye is black with small orange spot dorsally. Left ocellus (Figure 18*B*) is orange and male; right and median are black and female. Ocellar area is blacker on left, male, side. Wings are symmetrical with normal venation and hence to be regarded as diploid, heterozygous, *Dd*, and female. Abdomen is female throughout.

This gynandromorph functioned as a female, stung caterpillars and laid eggs, producing five males with black eyes and four with orange. She therefore bred as a heterozygote *Oo*. No record was made of venation of offspring.

Number 503 has left antenna male (Figure 19) with more or less spirally fused segments, right antenna female and normal. Left eye is orange with black region anteriorly; right is black. Left ocellus (Figure 18*C*) is large with light brown pigment, probably male and orange genetically, or mixed; median and right are black and female. Area between median and left is black (male), between median and right yellow (female). Left wings are smaller than right and therefore probably male. Left primary wing has R_4 missing; right primary has R_4 with a slight break. Abdomen is entirely female.

This gynandromorph functioned as a female, stung caterpillars and laid eggs from which developed males: type 1, defective 1, orange 1, and orange defective 2, and females: type 3, defective 4, orange 4, and orange defective 5. The gonads were diheterozygous, *OoDd*, as expected. The gynandromorph had mated with one of its orange defective brothers.

Summary

Despite the fact that reproductive instincts of gynandromorphs are usually opposite to the sex of the gonads, there have been found six in which there was agreement. Two of these with male instincts and genitalia were unable to mate, two functioned as males, and two as females.

Origin of male parts of gynandromorphs has hitherto been assumed to be maternal on the basis of visible recessive traits only. Breeding tests from the two specimens functioning as male shows that male gonads are of maternal origin.

Female parts of gynandromorphs have hitherto been assumed to be biparental, because dominant traits possessed by either or both parents appeared in these parts. Breeding tests from the two specimens functioning as female show that ovaries are actually heterozygous transmitting recessive maternal traits as well as dominant factors received from their fathers.*

*Work reported in this paper has been aided in part by a grant from the National Research Council, Committee on Effects of Radiation on Living Organisms. The project outlined under the grant is "Genetic Study of the Parasitic Wasp, Habrobracon, with the Purpose of Throwing Further Light on Such Problems as Sex-Determination, Parthenogenesis, and Gynandromorphism."

HEREDITARY VARIATION OF THE CHINCHILLA RABBIT

In Coat and Eye Color

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IT is well known that the coat of certain fur bearing mammals has a tendency to fade. Especially is this found among animals in which black is the predominating pigment of the coat. This characteristic has been variously attributed to diet, environment and breeding and it is not improbable that each may have its effect under certain circumstances. Recent experiments have brought to light some interesting facts regarding the color phases of the chinchilla rabbit and their inheritance which may be of interest to rabbit breeders and perhaps to breeders of other fur-bearing animals in so far as they bear upon the general problem of permanency of coat color.

Chinchilla coat color has been found in mice and in guinea pigs as well as in rabbits and it is not improbable that it may be found among many other animals. As compared with the ordinary gray type, it is characterized primarily by absence of yellow pigment from the coat, while black is either undiminished or but slightly reduced. It is inherited as an allelomorph of full color and of albinism.

In order to explain briefly the meaning of this last statement I should like to call attention to the fact that according to the modern conception of Mendelian heredity, all inherited characters depend for their production upon genes (genetic factors) which are transmitted from generation to generation in the nucleus of the reproductive cells. Union of the ovum or female gamete or reproductive cell with the sperm or male gamete results in a duplication of these genes in the fertilized egg which is to produce the new individual. Every cell which thereafter results by division and growth of this fertilized egg normally contains all of the genes in this paired condition, until the maturation of the reproductive cells occurs (forming eggs or sperm) at which time each of these pairs is again separated.

The genes have been proven each to occupy a definite position in one of the deep staining bodies called chromosomes, which are visible parts of the cell nucleus, to be seen under high magnification at certain stages in cell division. In rabbits, the number of these pairs of chromosomes has been found to be 22. The members of each pair of chromosomes are visibly alike and are homologs but they differ in size and shape from each of the other twenty-one pairs. Genes which occupy identical loci or spots of homologous chromosomes are known as allelomorphs. In the normal wild type rabbit each gene is like its mate (allelomorph) and little or no variation is found in the stock from generation to generation. Occasionally, however, the internal structure of some one gene becomes altered, the cause of which may as yet be only speculated upon, and a new variation, mutant or sport, as such products have been variously termed, results.

Allelomorphs of Albinism

Sometimes a gene will be found to have mutated several times producing each time a different mutant type. Since they are but alterations of the same gene they occupy the same loci of homologous chromosomes and behave as allelomorphs in inheritance. Since never more than two chromosomes of any one kind are normally

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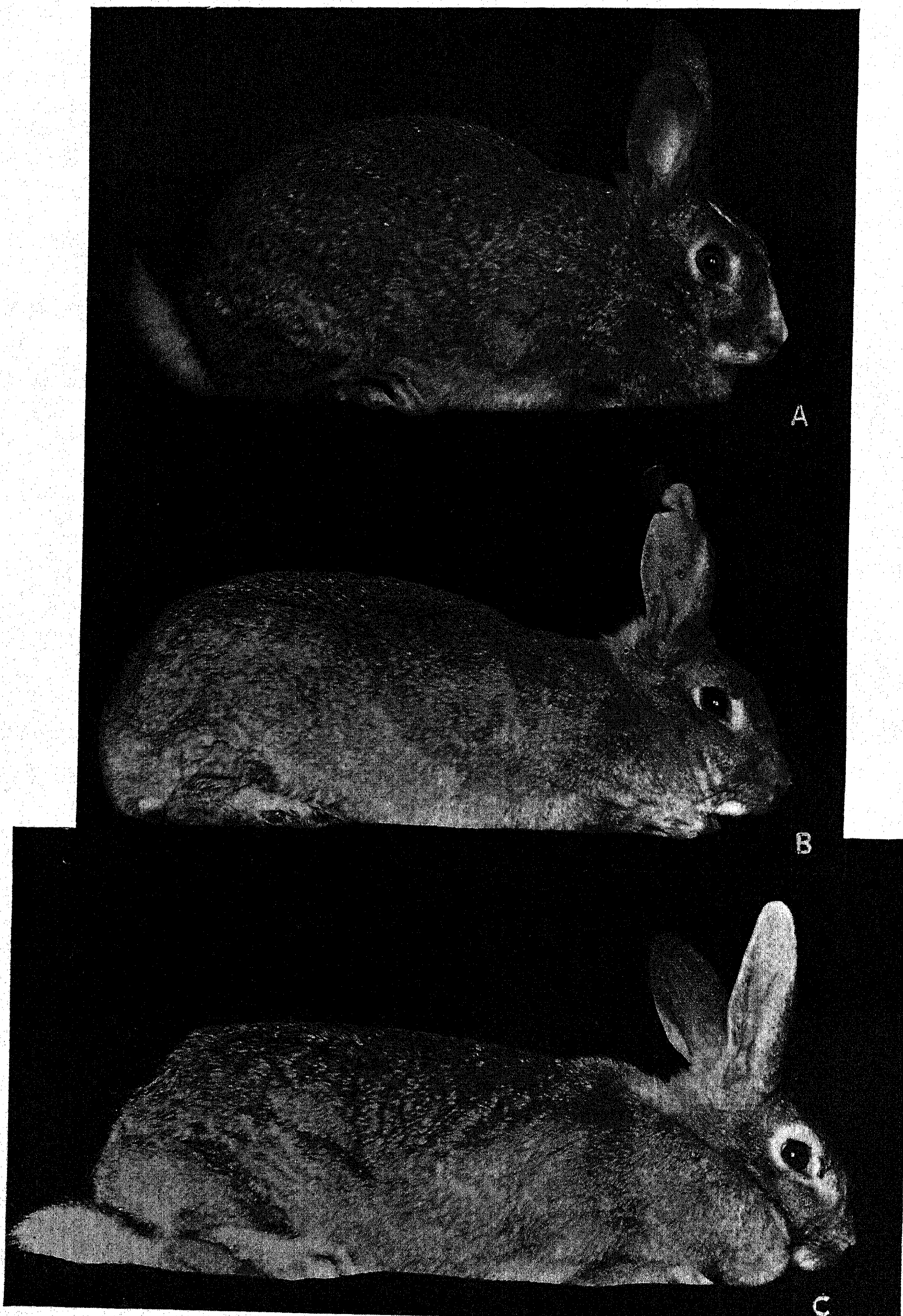


CHINCHILLA COMBINATIONS

Figure 20

A—Blue-eyed dark chinchilla (*ch*^{3}); *B*—Pale chinchilla (*ch*^{1}). Note the less intense black pigment which is especially evident on the sides; also the brown eyes as compared with *A*; *C*—Dark chinchilla (*ch*^{3}) with gray (modified blue) eye.

found in any one individual it is never possible to combine more than two of these mutant types in any one individual.

One of the most common of such mutants which occurs in the wild is albinism. Two different albino mutants are known among mammals. One of these has been designated the complete albino since it appears to be incapable of developing any pigment whatever. The hair, skin and iris of the eye are white and the pupil of the eye is pink or red. The second type is exemplified in the Himalayan race of rabbits in which the extremities (ears, nose, feet and tail) develop some pigment. The albino guinea pig is of this type.

Associated with the Himalayan gene is the interesting phenomenon of temperature sensitivity of the pigment producing mechanism. It is a well known fact among those who have had experience with the Himalayan rabbit that the young are born as devoid of pigment as are the young of their true albino cousins. With the moult of the juvenile pelage appear the characteristic markings of the race. Schultz[1], a German investigator, and others, found that the plucking of the fur from spots on the back of these rabbits during seasons of low temperature resulted in the production of new fur upon these areas which was pigmented. This indicated that the slightly reduced temperature of the spots from which the fur had been plucked was responsible for pigment production and that the normal markings of the extremities were also due to a slightly reduced temperature of those regions of the body. By proper manipulation of eye tissues Schultz has since been able to demonstrate that they also are capable of producing pigment outside of the body at reduced temperatures.[2] The newborn Himalayan is non-pigmented because the tips of the hairs, already developed at birth, have been formed within the body of the mother at a temperature too high for pigment production. But hair grown after birth becomes pigmented in those regions of the body where the temperature is sufficiently low.

Allelomorphs of Chinchilla

In the ordinary wild type rabbit which is a gray or agouti like that of the Gray Flemish, both black and yellow are found to be well developed. For this the color gene (C), commonly referred to as full color, is responsible. Complete albinism (c) may be considered as due to an alteration in the internal structure of the full color gene (C) such that at normal temperatures it is incapable of pigment production. The Himalayan gene (c^H) is a similar alteration which is incapable of pigment production at the usual body temperature but may produce it at average atmospheric temperatures which the skin of the extremities of the animal approaches.

If the (C) gene and its variants (c) and (c^H) are carried only in pairs as indicated above, it follows that a single individual may never carry more than two of these variations. The possible combinations are CC, Cc^H, Cc, c^Hc^H, c^Hc, and cc. Crosses readily demonstrate the fact that full color (C) is completely dominant to Himalayan (c^H) and also to (c). One dose of (C) is equivalent to two in so far as the general appearance of the animal is concerned. Known CC ,Cc^H, and Cc types are indistinguishable from each other. Himalayan albinism (c^H) is also found to be dominant to true albinism but as indicated above is recessive to full color. This relationship which is known as allelomorphism was known as early as 1906. Castle[3] who was the first to record it, was also the first to point out that the chinchilla rabbit which was discovered during the World War behaves as an allelomorph of albinism and full color and therefore is due to still another modification of the full-color gene.[4]

Since that time two other types of chinchilla have been recognized,[5, 6, 7] making a total of six allelomorphs in this series. Arranged in the order of

BLUE-EYED DARK CHINCHILLA[3] IN COMBINATION WITH NON-AGOUTI ***(aach³ch³)***

Figure 21

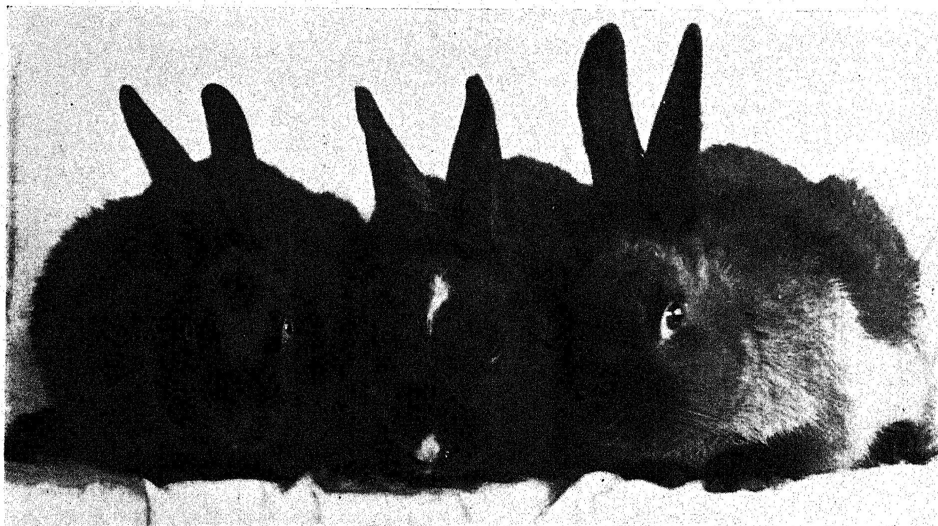
Note the intense black pigmentation of the fur. The white hairs are a form of silvering due to hereditary factors apart from chinchilla or agouti.

dominance, they are full color (C)* dark chinchilla (ch^3), light chinchilla (ch^2), pale chinchilla (ch^1), Himalayan albinism (c^H) and true albinism (c). The possible combinations including the six homozygous or true breeding types total twenty-one and the possible chinchilla types alone total twelve.

Pale chinchilla (ch^1) (Fig. 20*B*), develops the least pigment of the three chinchilla types and is characterized not only by its brownish black pigment but also by being lighter on the sides and belly, while the extremities, especially in cold weather and in the new coat, are darker than the rest of the body, as in the Himalayan. It is only partially dominant to the two albino forms, the hybrid or heterozygous combinations ch^1c and ch^1c^H being much lighter in color than the homozygous ch^1ch^1 true breeding type, especially as regards the color of the sides. The agouti combination ($AAch^1ch^1$) is the undesirable brown-sided chinchilla while the non-agouti combination ($aach^1ch^1$) is the Sable, most prized by the fancier in the heterozygous combination with albinism ($aach^1c$) although it will not breed true, since it produces both the homozygous dark type and the albino as wasters. (See Figure 22.)

Light chinchilla (ch^2) develops a greater amount of pigment than pale chinchilla (ch^1), when either heterozygous for albinism or in the homozygous condition. In combination with albinism both non-agouti ($aach^2c$) and agouti ($AAch^2c$) are almost indistinguishable from respectively similar types of true breeding sable ($aach^1ch^1$) and pale chinchilla ($AAch^1ch^1$). The only difference between ch^2 and ch^1 types is found in the uniformity of the former in contrast to pale chinchilla types which are lighter in color on the sides. (See Figure 22.) The combinations of pale with light chinchilla, either agouti ($AAch^2ch^1$) or non-agouti

*The reader will find the symbols here used similar to those of Castle, W. E., "The Genetics of Domestic Rabbits," Harvard University Press, Cambridge, Mass. As there are now three chinchilla types numbered 1, 2 and 3 or according to Castle's terminology c^{ch1}, c^{ch2}, and c^{ch3} for simplicity I have designated them merely (ch^1), (ch^2) and (ch^3).



NON-AGOUTI-CHINCHILLA COMBINATIONS

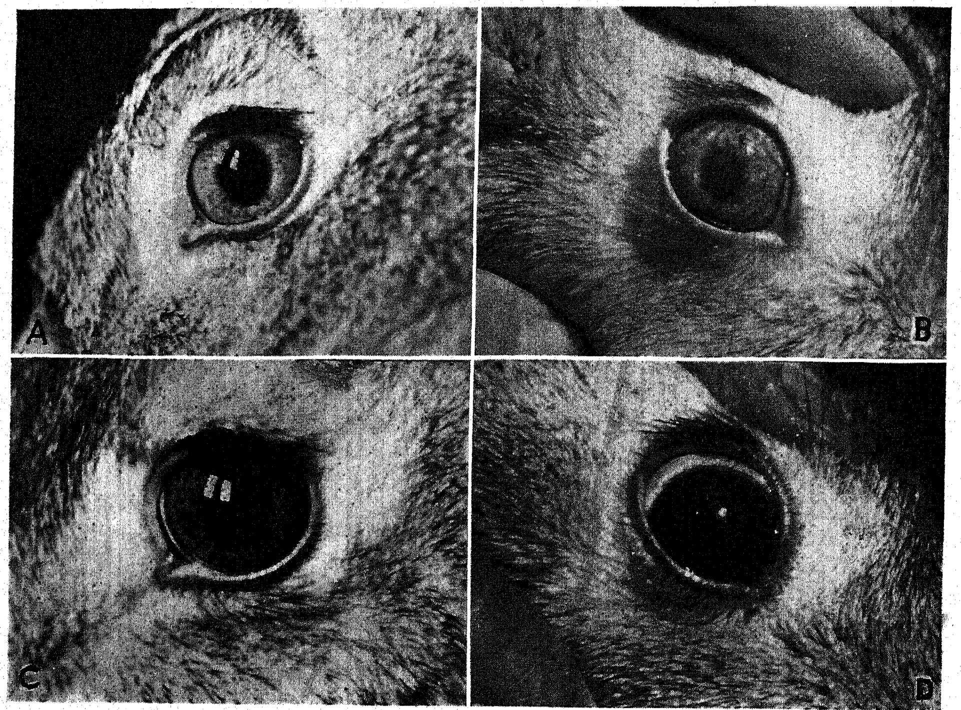
Figure 22

Three-months-old individuals in which non-agouti is combined with each of the three types of chinchilla. Note the different intensity of pigment in the coat of each. The individual at the left is dark chinchilla. In the center is light chinchilla and at the right is pale chinchilla or sable. Each of these carries albinism.

($aach^2ch^1$), are perceptibly darker than the homozygous pale (ch^1ch^1).

Dark chinchilla (ch^3, Fig. 20*A*) is the type which in the agouti combination appears to be necessary for the production of the non-fading dark coat of the fancier's preferred type of chinchilla. Unfortunately the characters blue and gray eyes considered by fanciers as undesirable are closely associated with if not a part of this allelomorph. Blue-eyed dark chinchilla is completely dominant to the albino forms, the heterozygous (ch^3c) or (ch^3c^H) being indistinguishable from the homozygous (ch^3ch^3) individual or true breeding blue-eyed dark chinchilla. In combinations of dark chinchilla with either light chinchilla (ch^3ch^2) or pale chinchilla (ch^3ch^1) the eye however becomes brown. Dark chinchilla produces the desired coat color but light or pale chinchilla is necessary for the desired brown eye color. The mating of such heterozygous individuals together, however, results in 50% wasters since one-fourth of the offspring are blue-eyed dark chinchillas and one-fourth are light or pale chinchillas as the case may be. Under such conditions the most economical procedure is the maintenance of both true breeding strains, of dark chinchilla which is blue-eyed and light or pale chinchilla which is brown-eyed, and the crossing of these two for the production of show animals or market pelts. Such a cross produces 100% of the desired type. The non-agouti dark chinchilla ($aach^3ch^3$) is black and is indistinguishable except by eye color from any other black rabbit. (See Fig. 21).

Genetical analysis of the dark chinchilla has shown that the blue eye is also affected by genes known as modifiers because of their secondary influence upon the principal factor blue-eye. Evidence has been obtained that these are at least several in number. In their absence (or recessive condition) an eye is produced of very nearly as clear a blue as that found in the Vienna White rabbit, but the cumulative effect of several active (or dominant) modifying genes produces an increase of pigment in the front wall of the iris resulting in various degrees of gray or mottled eye (see Figures 20, 21 and 23). A maxi-

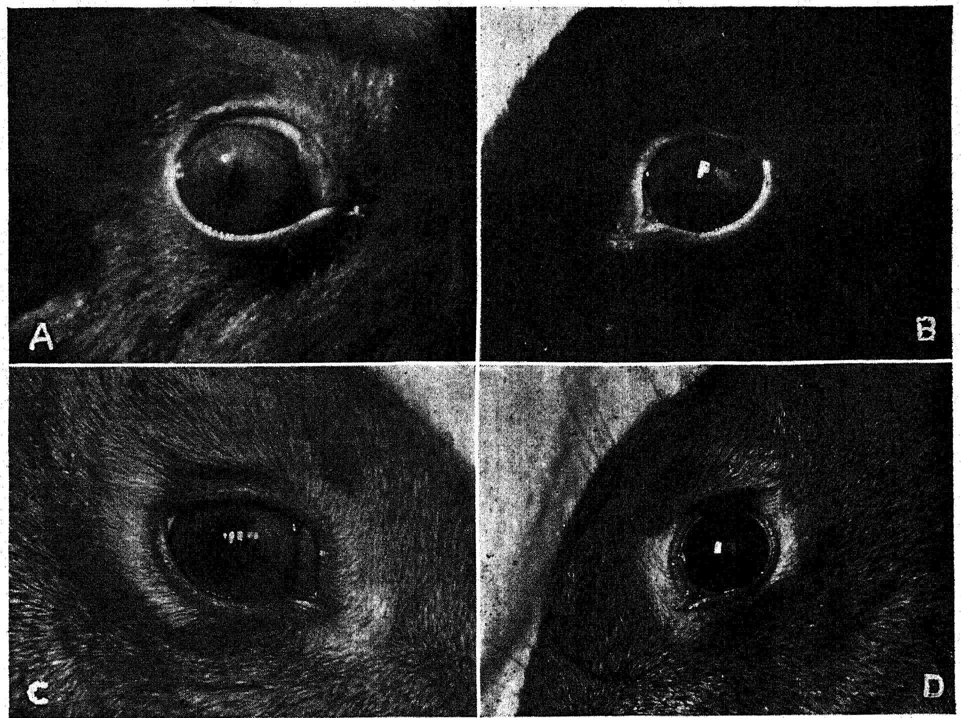


VARIATION OF EYE-COLOR IN DARK CHINCHILLA RABBITS
Figure 23

A—Typical blue eye with a minimum of pigment on the anterior wall of the iris. *B* and *C*—Gray eyes, also genetically blue but with intermediate amounts of modification. *D*—Pseudo-brown eye, genetically a blue but with a maximum of genetic modifiers producing anterior iris pigmentation.

mum condition is reached in a pseudo-brown (Figure 23*D*), which has been observed to be more intensely brown than many of the brown-eyed dark chinchilla (ch^3ch^2) or (ch^3ch^1) heterzygotes. Pseudo-brown eyed individuals may probably be true-breeding but up to this time they have not been sufficiently investigated. Gray-eyed types characterized by minute blue-spots in the iris which brown pigment has failed to cover may produce off-spring with blue, gray or pseudo-brown eyes. Gray and pseudo-brown eyes are always very bluish at weaning age and become darker as the animal grows older.

Two genetic agencies then are involved in producing the eye color of the chinchilla. One (blue eye) is closely associated with dark chinchilla. Its allelomorph (brown eye) is associated only with full color, light chinchilla or pale chinchilla. If this were the only agency involved, brown-eyed dark chinchillas would never be found to be true breeding. The second agency is a group of modifying genes which influence the pigmentation of the front surface of the iris. These make possible a true-breeding type. They are independent of the albino series since corresponding variations in the intensity of pigmentation of the iris (varying shades of brown) have been found among the full color, light and pale chinchilla segregates, as well as varying shades of blue, gray and pseudo-brown among dark chinchilla segregates from crosses designed to test the inheritance of this particular character. The eyes of full colored, light and pale chinchillas which lack these modifying genes are bluish at weaning age but by the time sexual



VARIATION OF EYE-COLOR IN LIGHT CHINCHILLA (ch^2) AND IN FULL COLORED RABBITS.

Figure 24

A—Light brown eye of light chinchilla without modifiers. Compare with Fig. 23*A*. *B*—Dark brown eye of light chinchilla containing modifiers. Compare Fig. 23*D*. *C*—Light brown eye of full colored rabbit lacking modifiers. Compare Figs. 23*A* and 24*A*. *D*—Dark brown eye of full colored rabbit containing modifiers. Compare Figs. 23*D* and 24*B*.

maturity is attained they are a very light brown in color (Figures 24*A* and 24*C*). The maximum effect of these iris modifiers in full colored or in any of the three chinchilla types is the production of an intense brown-black iris (Figure 24 *B* and *D*).

Production of Brown-eyed Chinchillas

From the above facts it is obvious that two methods of producing brown-eyed chinchilla rabbits are possible. The first and most certain as already indicated is to cross light or pale chinchillas (which are brown-eyed) with blue-eyed dark chinchillas, which contribute the desired coat color. Brown-eyed dark chinchillas produced by this method are, however, not true breeding.

The second method is the selection for a number of generations of only the darkest gray-eyed chinchillas for breeding, preferably those of the pseudo-brown type. Obviously if a true-breeding race can be established by this method it is more desirable than a type not true-breeding. The nature of these two genetic complexes is such that the two methods may be readily combined, so that while practising the first method the breeder may also by selection of chinchillas with the darkest eyes (either brown or gray) build up in a few generations the desired race breeding true for eye color. After this has been secured, the pale or light chinchilla waster classes may be permanently eliminated by back-cross breeding tests which will identify the desired homozygous brown-eyed dark chinchil-

las, those which will breed true for the desired characters of both coat and eye color.

Conscientious breeders undoubtedly have indirectly been practising this combination method by mere selection of the darkest eyed chinchillas. This attains the same end but may produce more wasters during the process.

Two facts are of value to those interested in the promulgation of breed standards. First, there appears to be no good reason for adopting the brown eye other than personal preference based upon aesthetic appeal. From the genetic standpoint it appears much easier to obtain a condition of true breeding to the desired coat color in the blue-eyed chinchilla. But secondly, if brown eye is to be given preference, the genetic evidence is that the most intensely pigmented brown-eyed individual is most likely to breed true.

The pigment of light and pale chinchillas is subject to temperature influences much as is that of the Himalayan rabbit and the Siamese cat. This is most pronounced in the Sable or non-agouti pale chinchilla which has been studied in some detail by Kosswig. In this rabbit the extremities are always dark as in the Himalayan. At birth both pale and light chinchillas (and their non-agouti combinations) are a uniform color over the entire body but a little later the tips of the guard hairs which had developed in the warmer temperature of the mother's body before birth will be found to be lighter in color than the parts formed after birth. If the animal is born at a cold season of the year the contrast between these light tipped guard hairs and a very dark under fur developed in the cold, is such as to give the animal a frosted appearance. Dark chinchilla thus far has given no indication of being affected by temperature differences.

Pale and light chinchilla are also extremely subject to fading. The new fur always develops a much darker color in contrast to the old pelage which has faded. Dark chinchilla also fades but to such a slight degree that it requires close examination under good light to detect the difference between old and new fur in a moulting non-agouti or black animal and in the agouti combination which is the normal dark chinchilla it is still more difficult.

The cause of fading I believe is not clearly understood. Sunlight, lack of humidity and other environmental influences are under suspicion. Breeding has also come in for its share of blame. In the three chinchilla allelomorphs in the rabbit we have a clear cut example of hereditary color factors which exercise an influence on ability to withstand whatever environmental influences are responsible for the fading phenomenon. This indicates that breeders of fur bearing animals whose stock has a tendency to fade might profitably give some thought to the breeding of their animals in this respect.

Details of the breeding experiments here described which have been carried on at the Bussey Institution of Harvard University, Boston, Mass., are to be published elsewhere.

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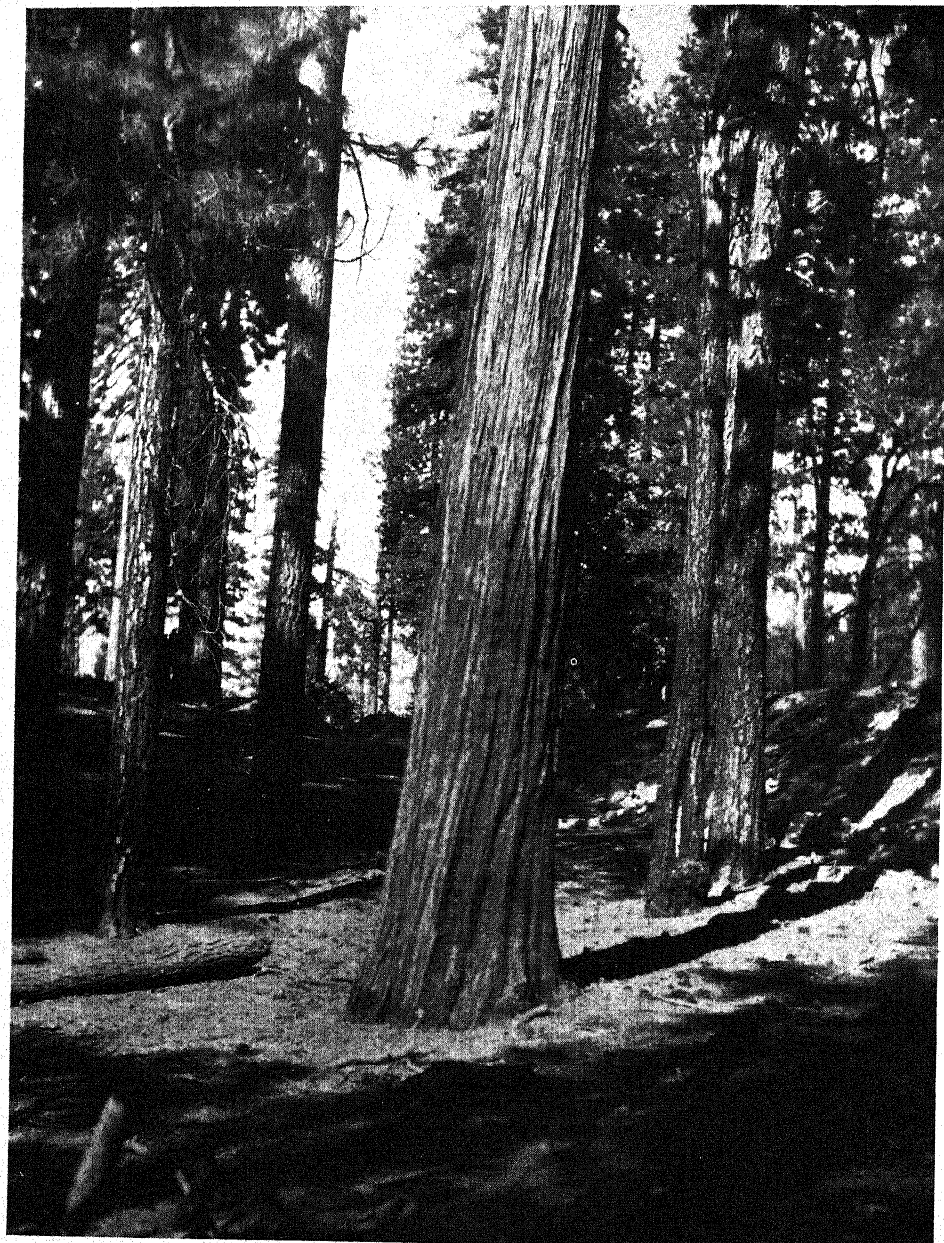


IS THE TWISTED TRUNK CAUSED BY THE WIND?

Frontispiece

A number of causes have been suggested to account for trees having twisted trunks. While in some situations solar action or wind action might appear to be the cause, in other instances it does not account satisfactorily for the phenomenon. In the same species trunks of twisted trees can be found with a clockwise twist or with a counter clockwise twist. This tree of California White Cedar (*Libocedrus decurrens*) has a pronounced clockwise twist, as viewed from the base.

WHAT CAUSES TWISTED TREES?

F. Knorr

Modesto Junior College, California

In the summer of 1925 our attention was attracted to a large number of twisted trees located near the summit of the Sonora Pass in Alpine County, California. At an elevation of 8,398 feet there was an unusually large group of Jack Pines that were twisted counter-clockwise. Why such a large group should be twisted in that direction with but a very few exceptions, naturally excited our curiosity.

Consulting with the forest ranger of that area regarding the possible causes contributing to this phenomenon, we were surprised to learn that he had never noticed this twisting, and had never given any thought to this condition and remarked: "no doubt it is due to the wind."

During the remainder of the summer we had an opportunity to question a number of sheep and cattle herders relative to twisted trees; we found a number of individuals interested in the problem and practically all attributed the cause to wind action or the "pull of the sun."

The theory of the sun action being that the young tree follows the sun after the same manner as the sun flower, facing the sun in the morning and again facing it in the evening. The theory did not seem feasible. Even though the plant made a half turn during the day it did not seem possible that it would continue to twist and make a full turn to face the sun in the morning. Flowers following the sun swing back the same way they turned during the day.

The wind theory seemed more plausable. In an effort to verify this, observations were made the following summer in such canyons where the winds were constant or had an opportunity to blow at the trees from a definite angle and would, therefore, exert the same force on all of the trees in that particular area. At the end of the season of 1926 we found that out of 486 twisted trees that we checked, 56 per cent were twisted counter clockwise and 44 per cent were twisted clockwise. These data did not impart any special information except that the chances were about even that the wind would blow as many trees in one direction as the other. Like the instances brought forward by Cooper[1] and Wentworth,[2] these observations did not offer an altogether satisfactory solution of the problem.

One Quarter of Trees Twisted

In the early part of the summer of 1927 we ranged in the big timber, the *Sequoia gigantia*. At this time we began to check the straight as well as the twisted trees in certain areas. Later in the same season we made an extensive count of the Western Yellow pine, the Jeffry pine, Sugar pine, White fir and the Incense Cedar.

The check was made regardless of species; of 822 trees counted it was found that 73 per cent were straight and 27 per cent twisted. Of the twisted trees 52 per cent were twisted clockwise and 48 per cent counter clockwise.

There were some errors in this count due to the fact that all trees do not give an exterior indication of the nature of their growth; the bark is of such a nature that it does not always twist as does the wood. This is especially true of the Firs and the Sugar pines.

Again our work seemed inconclusive, except insofar as we were convinced that the wind was not a causative factor, several exposed areas where wind action was constant showed as low as two per cent of

TWISTED TRUNKS OF WESTERN YELLOW PINE

Figure 1

The trunks of these pine trees have both a clockwise twist. The one to the right is much more twisted than the other. If these twisted trunks are produced by genetic causes, the lumbermen's custom of leaving twisted trees as seed-trees, would be expected to increase the number of twisted trees rather rapidly in later generations.

twisted trees. Again, if wind, direct or indirect, is the cause of twisting, then by far the larger percentage of trees should show a decided twist. After the year's work we concluded that certain species in some areas twisted more than other species.

In the summer of 1928 the method of procedure was again changed. An improvised chart was made of tree groups, on this the trees were plotted, showing the relative locations of the species and the nature of growth. This procedure made observation very laborious. Much data were collected during the season of 1928 and 1929. This material was most interesting but proved to be difficult of interpretation. It was found that in a certain area where three or four species of trees abounded only one species would show twisting; in other sectors two or three species would show twisting. There was absolutely no relationship in the number of twisted trees in one area as compared with those in another location.

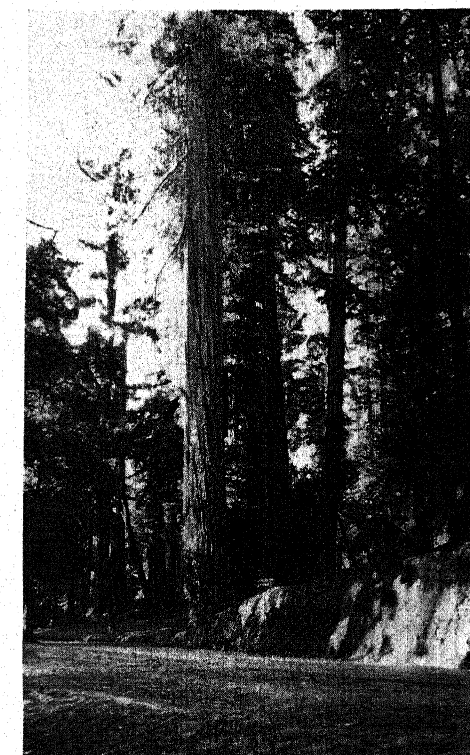
Is Twisting Inherited?

We were now fully convinced that neither the wind nor the sun were the contributing factors that cause twisting. The theory that twisting is due to impoverished soil or hard subsoil was not substantiated as we found many well grown, thrifty but twisted trees growing in deep alluvial soil; the gnarling of trees may be due to slow growth in hard semi-sterile soil but not twisting. In 1930 we came upon some old Junipers near the timber line, an old tree that was all but

destroyed had sent up three sturdy young trees from the base, these three trees were twisted in the same direction as the parent tree; we grasped the idea, twisting is inherent in the roots. A trip into the *Sequoia sempervires* forests of California verified our conclusions. These trees, as is well known, send up new growth from the roots of the fallen trees or from the fallen trees themselves. Here we found that these "rootlings" always produced the same type of growth as found in the original tree.

Secondary growth of some deciduous trees was studied, again it was found that such growth was of the same type and nature as the original. We were now fully convinced that climatic conditions did not influence type of growth.

Late in the fall of 1930 we felt convinced that twisting must be an inherent condition. A study of the charts made a few years previously confirmed this belief, and with this in mind a study was made of the young growth. The trees used were taken from dense growth areas where thinning was necessary and in one case where a clearing was made for agricultural purposes. The trees were from four to ten feet high; it was necessary to peel the bark in order to determine the nature of the wood growth. In this group of trees composed of four species a very small percentage of Sugar pines showed twisting.



A TWISTED SEQUOIA

Figure 2

A potential giant of the California forests (*Sequoia sempervirens*) with a counter clockwise twist. Secondary growths and "rootlings" from twisted trees are found to produce trees twisted in the same direction as the original tree. This is a strong indication that the twisting is not due to environmental causes.

Twisting of Seedlings

In 1931 we had access to a large number of seedling conifers ranging from two years of age to about six years. The trees were pulled up by the roots, the bark was immediately stripped from the tree and roots, this was easily accomplished while the plant was fresh.

Of the 261 conifer seedlings thus studied it was found that twisting is apparent as soon as the woody tissue is formed and that it carries the same intensity of twisting below the ground as above the ground. Carrying these findings to older groups of trees we find that the branches and twigs all show the same characteristic twisting.

One extremely interesting study was made of a freshly felled White fir that had a very decided twist; we wished to determine if the intensity of twist is equal in all parts of the tree. On this tree it was found that the intensity of twist varies slightly even on the same branch.

A further study of the twisting characteristic of lumber-yielding trees would be interesting and of economic

value. Twisted trees produce lumber in which the grain "runs out," does not split straight and is therefore much weaker than straight lumber. Some of our work leads us to believe that twisted trees grow slightly slower than straight trees and are more subject to "wind falls" (i.e., to being broken off by high winds).

We have found several areas where the lumbermen permitted twisted trees to stand as "seed trees" in order to reseed a cut-over area, these being the most worthless trees from a lumber or economic standpoint. If twisting is an inherited characteristic the folly of such a course is obvious. We tried to determine the intensity of inheritance of the twisted seedlings surrounding such twisted trees but had to abandon that phase of the work because of the inaccuracy that is inevitable under uncontrolled conditions.

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Evolution and Genetics

MENDELISM AND EVOLUTION, *by* E. B. FORD. Pp. xii + 116. $1.50. Lincoln MacVeagh (The Dial Press), New York. 1931.

THIS little volume in the "Dial Press Monographs on Biological Subjects" will prove to be of real service to genetics because it presents in untechnical language the evidence which recent discoveries offer toward a solution of the general problem of organic evolution. And still more important, the evidence is so rationally interpreted and the conclusions so logically presented that the intelligent reader, even with no previous knowledge of the subject, can hardly fail to see that the processes taking place in the laboratory and in nature today must be similar to those that have produced the multiplicity of organisms of the past and present.

The author quite properly stresses the importance of the internal as well as the external environment in controlling the effects of individual hereditary factors. This internal environment, which depends upon the interaction of the entire hereditary complex, is too frequently neglected in interpreting the effects of particular genes, leading to erroneous conceptions as to the potency of the gene itself. Although it is evident that all genetic factors have their origin in spontaneous mutations, it is unnecessary to assume that any particular mutation was immediately of survival value, for later changes either in external or internal environment may cause even a previously disadvantageous factor to become essential to the individual. The chances for survival, however, will always be in favor of a mutation which is immediately advantageous.

Each of the five chapters into which the book is divided, namely, particulate theory of inheritance, heredity and environment, experimental genetics and its bearing on evolution, the application of the mendelian theory to evolutionary problems in nature, and special problems of evolution, is followed by a brief summary giving in a few words the essential conclusions from the evidence presented. It is a surprise to find that the evolutionary picture can be adequately sketched in so small a compass and without the omission of any of its essential features.

The book deservies the highest praise and can be recommended to anyone who desires to know to what extent the evolutionary problems have now been solved. There is a helpful list of references and a glossary of the few technical terms employed.

WESLEY R. COE.

IDENTICAL TWINS REARED TOGETHER[1]

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THE few detailed studies[3, 5, 6] of identical twins reared apart have contributed very interesting data concerning the effects of nature and nurture on mental traits. Some of these pairs have shown pronounced similarities but others have shown consistent differences. The results have no clear-cut interpretation, partly for the reason that the numbers of cases reported are as yet rather small.

H. J. Muller[5] has reported a case of identical twin women about 35 years of age. They were brought up in different homes, and one had considerably more schooling than the other, but their home environments were roughly comparable. They were very similar in performance on intelligence tests, and somewhat different in non-intellectual traits. Newman has reported[6] a study of another pair of twin women, aged 27, who had been brought up in different homes. Like Muller's subjects, they had similar home environments and one had much more schooling than the other. However, in this case the more schooled twin was slightly superior in all intellectual preformances (the I. Q. difference was 12 points), but the two were remarkably similar in all tests of emotional traits and temperament.

Newman[6] reports two other very interesting cases. A pair of twin girls aged 17 were brought up separately in different home environments but each received about the same amount of schooling. They were very similar in temperament and emotional makeup, but the one with the superior home environment was consistently somewhat superior in all intelligence tests (the I. Q. difference was 12 points). In another case study a pair of identical twin men are described, who had a similar amount of school training and whose environments were very different, one spending most of his time in cities while the other had an essentially rural environment. Mental tests revealed close similarity (the I. Q. difference was two points) but the twins were very different in non-intellectual traits.

There is no consistency of results in the several cases; where the home environments have been different, the twins differ in intellect and are alike in personality traits in one case, and the reverse is true in both respects in the other case; where the schooling has been different, the twins differ in intellect and are alike in personality traits in one case, and in the other case the reverse is true for both traits. No explanation in terms of environmental factors affecting intellect and personality is consistent with all the results obtained.

To aid in the interpretation of the results, Newman furnishes statistical data obtained on 50 pairs of identical twins reared together. By this means he is able to show which differences are significant. He notes that in five of the cases of identical twins reared together the I. Q. differences were zero; also, in five other cases the I. Q. differences were as large as those obtained in the two cases of identical twins reared apart where an I. Q. difference of twelve points was found. However, the differences shown by several tests were consistent for the pairs reared apart, while for those reared together

* This work was done by the writer as part of a more extensive investigation of twins which he is carrying out at Stanford University under the direction of Dr. Lewis M. Terman. These studies were made possible by a grant from the Social Science Research Council. The complete data will be published later.

FINGER PRINTS OF TWINS B.E. AND L.E.

Figure 3

These twin boys of fourteen years of age are so much alike that they are frequently mistaken for each other by friends and relatives, and even by their father. While *B* tends to be left-handed, the fingerprints are of the type showing remarkable same-sided similarity. In fact they are much more similar than are the fingerprints of many other pairs of identical twins. In spite of a similar environment all their lives the twins record nearly a year's difference in educational age, though only four points difference in I. Q., on other tests.

data obtained by use of different tests did not by any means reveal such consistency. This of course creates the impression that the differences found for the twins reared together were random differences. The really important finding is not the size of the differences, apparently, but the fact that different tests consistently show a difference in the same direction for the twins reared apart.

Statistical studies citing average differences show that pairs of identical twins will exhibit great similarity of ability on the average. In such studies it is always possible to interpret even differences of considerable magnitude between members of a particular pair as the expected result of the operation of chance factors. The data reported here, however, suggest that such differences may not always be random fluctuations, but may be representative of stable differences. The writer believes that intensive studies of identical twins reared together are needed as controls for studies of identical twins reared apart. A fundamental truth is stated in Newman's assertion that each case of twins reared apart is a special problem; it also appears that each case of identical twins reared together also constitutes a special problem.

This paper summarizes the results of studies of identical twins brought up in environments essentially similar for the members of each pair. In each case the diagnosis of identity was based on the techniques outlined by Bonnevie[1], Dahlberg[2], Siemens[8], Newman[7], Muller[5], and others. The findings are merely summarized. Differences and similarities of environment are briefly described. The results of personality trait measurement are given without any attempt at exact evaluation of scores; it is assumed that those results give some insight into the situation, but careful evaluation must be based upon statistical treatment of larger numbers of cases. In view of the unreliability of instruments for measuring personality traits, the results of ability tests are given slightly greater emphasis. Only four cases are considered here, but others will be reported as the investigation continues. The statistical evaluation of the differences is left for later more detailed treatment. The argument here rests upon the consistency with which the results are obtained.

Case I. B.E. and L.E.*

Twins *B* and *L* are boys fourteen years of age. They belong to the extremely similar class of identical twins. Examination by the above-mentioned accepted techniques clearly demonstrated that they are monozygotic.

Their environments have always been very similar; they have the same home, the same school training, the same friends; they spend nearly all of the 24 hours of each day together, and have never been separated even as long as over night except in case of illness. They have had the same illnesses at the same times, excepting that *B* has had pneumonia and *L* has not.

The Strong Vocational Interest Test was scored for interests in 23 occupations, and great similarity of results obtained. The Bernreuter Personality Inventory was given twice, and in both cases *B* was clearly more introverted, less self-sufficient, and less dominant. An 80-item information test showed that *L* is more inclined to overstate his abilities. The difference in scores on the Masculinity-Femininity Test indicated that *B* has more of the mental traits associated with masculinity. *L*'s score on the Meier-Seashore Art Judgment Test was slightly higher, but neither twin shows any particular aptitude for art.

Direct questioning of the mother resulted in statements that the twins were of equal ability, but answers to specific diagnostic questions scattered about in a questionnaire indicated that she considers *B* brighter. She reports that *B*

* A preliminary account of the detailed results on this case was given in a report at the meetings of the Western Psychological Association, at Eugene, Oregon, June 13, 1931.

FINGER PRINTS OF TWINS D.C. AND T.C.

Figure 4

The finger prints show same-sided symmetry and the twins are both right-handed. In spite of physical and mental differences at 38, the fingerprint patterns alone are almost conclusive evidence that they are identical twins. Their environment was very similar until they were eighteen, and has differed somewhat since that time. At the present time the twins differ twelve points in I. Q., and thirty pounds in weight. This difference is nearly as large as the greatest difference found by Newman in twins reared apart since early infancy.

reads more. Their home-room teacher states that *B* is brighter than *L*, but another teacher failed to notice any differences.

The differences here are not as large as those found by Newman for two cases of identical twins reared apart, but they are larger than those found for the two other cases of identical twins reared apart. It is thought that such consistent differences between identical twins are comparatively rare, but the findings require explanation. Is *B* superior because he reads more, or does he read more because he is superior? Is he a better scholar because he is more introverted, or is he more introverted because he is a better scholar? These questions can better be answered when data are available for comparison of a number of cases of this type.

Case II. D.C. and T.C.

In determining the difference between identical twins reared together, it is perhaps desirable to select cases representing different age groups, so as to exclude the possibility of the differences being due merely to variations in rate of maturation. Here we must take strict account of any environmental differences, and view the situation as a whole in the attempt to see some significant trend in the data.

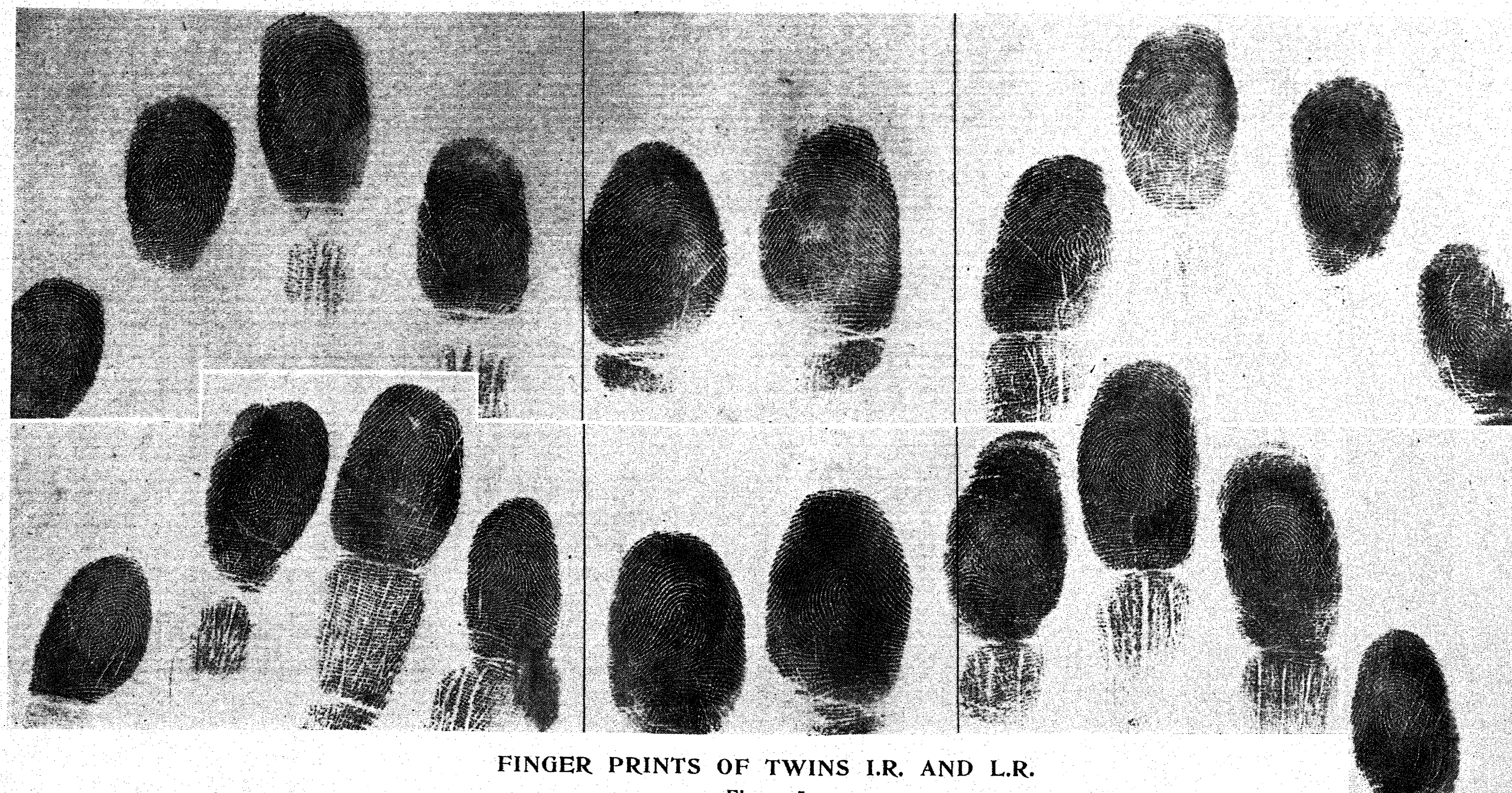
D and *T* are twin sisters aged 38. At present they are superficially different in appearance, as *D* is 30 pounds heavier, much stronger, and is tanned by exposure to the sun, occasioned by gardening. However, there is great similarity of stable features such as height, form of face, fingerprint patterns, color of hair and eyes, etc. The data are such as to justify a diagnosis of monozygosity on the basis of the criteria usually employed.

Their environments have been extremely similar in early life, but increasingly different since age 18. *D* had two years of highschool training, and *T* left after one year. *T* spent 15 months in business college, and *D* entered later and completed the same course in 9 months. During the last decade, *T* has had an environment which offers more social contacts, and more opportunity for cultural development, intellectual advancement, and the pursuit of knowledge. Her husband having died seven years after their marriage, she has worked in the Y. W. C. A. for nine years, as General Secretary. She has only three children, while *D* has seven; when the children were young, *D* took care of all of them

TABLE I. Results of Tests of Intellect and Achievement—Case I.

	B's Score	L's Score
Terman Group Test, at age 14-2		
Raw score	37	27
I. Q. Equivalent	81	77
Vocabulary tests*		
Test A	38	26
Test B	31	20
Test M (Very difficult)	29	25
Stanford Achievement Test		
Total Score	778	688
Educational Age	12-3	11-5
Grade Level	6.4	5.6
Willoughby battery of 11 tests		
Total score	208	182
Stanford-Binet Intelligence Test		
Mental Age	12-3	11-10
I. Q.	83	80

* Vocabulary tests taken from the University of Minnesota College Aptitude Test. Tests A and B combined have a reliability coefficient of .92 for adults, or for children in homogeneous age groups. Each test takes 15 minutes.



FINGER PRINTS OF TWINS I.R. AND L.R.

Figure 5

The twins were sixty years of age when the tests were made. *I* is strongly right-handed, but *L* shows a tendency to be left-handed. The finger prints show decided same-sided symmetry. *I* and *L* have had a remarkably similar environment, even for twins, in spite of which very definite differences were noted, especially in their personalities.

while *T* worked at the Y. W. C. A. Housework occupies all of *D*'s time. They have very similar interests, as determined by the Strong Vocational Interest Test. *D* mentioned gardening as a hobby, while *T* stated that she is interested in the problems of young people and in work of the kind she does in the Y. W. C. A.

On the Bernreuter Personality Inventory, *D* was found to be more self-sufficient, more dominant, and more extroverted. An 80-item test of information showed that *T* has a greater tendency to overstate her knowledge, and indications obtained in an interest rating scale also showed that she is more inclined to overstate. Scores on the Masculinity-Femininity Test indicated that *T* has considerably more of the mental traits of femininity. *T* has 17 psychoneurotic symptoms and *D* has 12, as determined by answers to the Woodworth-Cady Questionnaire; in other words, *D* is more stable, although the difference is slight. Ratings by their daughters and their nephew agree that *T* is more excitable, obstinate, and unruly, and has a greater liking for responsibility. These raters agree that *D* is easier to get along with, more practical and sensible, and more good-natured.

D's consistent though slight superiority on the vocabulary tests is surprising in view of the fact that the twins and their daughters and nephew unanimously report that *T* reads more. Their vocabulary scores approach the mean of 91 for college freshmen on tests *A* and *B*.

T reports that *D* is brighter than she herself is, and always did better work in school. The differences of course are very slight, but their consistency indicates that they are not mere random fluctuations. Whatever the cause, the differences are stable, and indicative of something in the fundamental make-up of the twins. We believe that the environmental factors have been such as to favor a difference in the other direction, and the findings are therefore all the more significant as an indication of mental superiority on the part of *D*. The greatest difference is not in achievement test score, but instead it is found in the scores on the Terman Group Test, where the I. Q. difference is twelve points.

In this case it is the superior twin who is more extroverted, more self-sufficient, and more dominant, while in Case I the opposite is true. Here the superior twin reads less, while in Case I the superior twin reads more. It is interesting to note that of all the cases reported in this paper, the environments have been most different in Case II, but the differences, with the exception of the Terman Group Test results, have been smallest; furthermore, all these differences are in the direction opposite to that in which the environmental differences would lead us to expect them.

TABLE II. Results of Tests of Intellect and Achievement—Case II.

	D's Score	T's Score
Vocabulary Tests		
Test A	88	84
Test B	91	83
Test M (very difficult)	72	67
Stanford Achievement Test		
Total Score	1097.5	1067.0
Educational Age	17-0	16-6
Total Score	393	388
Willoughby battery of 11 tests		
Terman Group Test Form A		
Total Score	185	146
M. A. Equivalent	18-4	16-5
I. Q.	114.6	102.6

Case III. Twins I.R. and L.R.

Twins *I* and *L* are men in the sixties. Careful examination reveals a great physical similarity. Their body builds are very similar, and unlike those of their siblings; their fingerprints show the close resemblance peculiar to identical twins, and bodily measurements are strikingly alike. There was no difficulty in arriving at a diagnosis of monozygosity.

Their environments have been very similar in most respects. They have always spent a great deal of time together; they have had the same amount of school training, and have taken the same courses, with a few exceptions; they have been doing the same kind of professional work all their lives. Each has a wife and a daughter. The main difference in environment is the fact (reported) that *L* has a rather unsympathetic wife, and his home life does not give him the companionship he needs. *I* is happily married, and his home life is ideal. Their personality traits have perhaps resulted in some environmental differences, as *I* is more socially inclined, and has more friends.

Their disease histories have been similar, both having had the same illnesses, and usually at the same time. At present *I* has heart trouble and must avoid violent exertion, and *L* has not been very well for the past year or two. It is interesting to note that *L*, the first-born, precedes *I* in reaching various stages of development; he was the first to require glasses, and the first to need false teeth; at present he seems to suffer from fatigue and absentmindedness, and exhibits symptoms of feebleness and old age far more than does *I*.

In personality they are very different, although both are sensitive, anxious to excel, modest, sympathetic, very conscientious, and persevering. They used to be very alert intellectually, but at present this is not true of *L*. A brother reports that *I* is more socially inclined, and more like their father, while *L* is more like their mother. *I* is more impulsive and quick-tempered; *L* is inclined to be solitary. An observer always feels comfortable and at home in the presence of *I*; any visitor, however welcome, finds it something of a strain to be with *L*.

The Strong Vocational Interest Test showed that they have many interests in common. In eight occupations both received scores above C. In addition to these common interests, *L* had four interests which *I* did not have, and *I* had two which *L* did not have. Their interests differ in specific ways, but the total picture is one of similarity rather than difference.

L is more self-sufficient, more extroverted, and more dominant according to results of the Bernreuter Personality Inventory. The difference in introversion was not great, but was something of a surprise, because *I* is the more socially inclined, and *L* appears to worry and daydream more. The desirability of a retest is evident.

By means of an information test it was shown that neither has much tendency to overstate, but *L* overstated more than *I*. The Masculinity-Femininity Test showed that *I* has considerably more of the mental traits of masculinity. The Watson Test of Fair-mindedness indicated that both have many more prejudices than the younger twins tested, but *L* has nearly twice as many as *I*.

The Meier-Seashore Art Judgment Test scores indicate that neither has any aptitude for art, but *I* was better than *L*.

Since the Stanford Achievement Test measures facts both must have learned (detailed information on the case makes this conclusion unavoidable), the great difference in score must mean that *L* has a gross memory defect. He was allowed to try all items in order to eliminate differences due to speed. Any one of the differences found above is sufficiently large to be significant taken alone. The general trend of results indicates that *L* has poorer powers of observation, less

speed of reaction, a poorer memory, less information, a much more limited vocabulary, and less resistance to fatigue.

Early home life and school training have been very similar, and later environments have been far more similar than one would expect, even for twins. Yet *I* is in all respects definitely superior to *L*. The best conclusion the writer is able to draw is that *L* is more rapidly aging, and has reached a rather advanced stage of (normal) senescence while *I* is still carrying on very capably. It remains for further observation to determine whether the differences have always been present to some extent, and are merely increased by the onset of old age.

Case IV. Twins D. and V.

D and *V* are the Siamese girl twins investigated by by Dr. Helen L. Koch[4]. They were fourteen years of age at the time of the examinations.* Of course they must be monozygotic, and their physical traits were very similar, although not more so than the inheritable physical characteristics of the above-described pairs. *D* is the right member of the pair, and is right-handed; *V* is left-handed. They have very similar developmental histories, but *D* menstruated one month in advance of her twin, and cut her teeth three weeks earlier. She is slightly larger than *V*.

The twins were very similar as regards results of the Downey tests, but the Kent-Rosanoff Free Association Test revealed considerable differences, *V* having over three times as many individual responses, and greatly exceeding the norms. The Pressey XO tests gave very different results for the two, indicating an emotional difference. In tests of speed of movement, in tapping and in card sorting, *D* was superior.

D was more talkative, and was consistently superior in all tests of intelligence and scholastic achievement.

In addition to the above, a lengthy series of tests of vocabulary, composition, and reading comprehension was used, and in these *D* was consistently superior. The differences in intellect and achievement were less than are found in most twins, but practically all favor *D* For details, see the original article.

All deviations from the norms were in the minus direction. As in our Cases I and II above, there is an essential similarity of profile and of achievement level with reference to random pairs of comparable age, but there can be no doubt that one of these girls is very consistently superior to the other. This case certainly supports our contention that those differences are hard to explain in terms of environmental factors.

Summary and Discussion

Each of these four cases of identical twins reared together exhibits a consistent intra-pair difference in ability as measured by tests of intellect and achievement. The high reliability of such long batteries of tests, and the regularity with which the results point in one direction justify the conclusion

TABLE III. Results of Tests of Intellect and Achievement—Case III.

	I's Score	L's Score
Vocabulary Tests		
Test A	108	69
Test B	100	86
Stanford Achievement Test		
Total Score	1174.5	913
Educational Age	18-6	14-1
Willoughby battery of 11 tests		
Total Score	346	138

*These twins are illustrated in this JOURNAL for July, 1931, Pp. 202 and 203.

that the differences are statistically significant.

In Cases I and IV the twins are adolescents, whose environments have always been unusually similar. In Case III the twins were much more different than in Case II, although the similarities of environment were greater in the former case. In Case II the difference in ability is in favor of the twin with the less favorable environment.

The data suggest that in some cases at least, identical twins may show a small but consistent congenital difference in mental ability. Certainly in these four cases stable differences have been found, which are difficult to explain on the basis of tangible factors in the post-natal environment. Since differences in physical traits such as size, weight, and strength seem to exist in some pairs of identical twins from the time of birth, it seems reasonable to assume the same may be true of the mental difference here reported.

Case I represents the first pair of adolescent twins on whom complete data were obtained; Case II and Case III are the first two pairs of adult twins studied. There are very few studies of identical twins reared together, and Case IV, reported by Dr. Koch, seems to be the only one furnishing the data necessary for comparison with our results. The writer does not believe that such situations are prevalent for identical twins, but all the results do support the hypothesis that in some cases the differences found, though small, are not random fluctuations.

The assumption of slightly unequal mental endowment in identical twins may be reasonable in view of the demonstrable fact of hereditary bilateral asymmetry in physical traits. Just as the right and left sides of a person's face may differ, so may the right and left sides of his brain. If the right and left cerebral hemispheres differ in functional capacity in normal cases, so may the brains of identical twins differ in functional capacity.

A word of caution is necessary here. These results do not in any way contradict the well-accepted conclusion that monozygotic twins are extremely similar in physical, mental, and social traits. The differences found are much smaller than one would find for representative pairs of siblings or like-sex fraternal twins. In an intensive study it has been possible to observe the slight differences between twins, who, when compared with siblings in general, display an essential similarity of mental make-up. It is significant that in these cases where the twins reared together do differ consistently, the differences are not always in line with such environmental differences as are ordinarily considered to be important factors in determining mental development. At least, a consistent explanation in terms of such environmental factors is elusive. No final conclusions can be stated at this time, but it is felt that an interesting problem has been ap-

TABLE IV. Results of Tests of Intellect and Achievement—Case IV.

	D's Score	V's Score
Army Alpha, form 8, total score	46	40
Terman Group Test, Form A, total score	59	42
Army Beta, total score	58.66	53.17
Performance Tests, total scores		
Ship Test (Glueck)	22	20
Feature Profile (Knox & Kempf)	7	7
Mannikin Test (Pintner)	7	5
Cube Imitation (Knox Pintner)	5	5
Courtis Arithmetic Tests, Series B, Form 1		
Total score, number attempted	15	11
Total score, number right	8	1

proached, and further study of these cases and others like them may reveal facts of fundamental importance bearing on the nature-nurture controversy, and on twin resemblance in particular.

AUTHOR'S NOTE: Since this article was written, Dr. Newman has discovered three or four more cases of identical twins reared apart, and a report of one such case is published in the preceding number of this JOURNAL. The new data reported in the present paper undoubtedly have some bearing on the same general questions, and the reader will wish to consult Dr. Newman's report for a more complete account of the findings regarding identical twins reared apart, and for information concerning the last case studied.

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Appendix

Physical Similarities of the Twins

Case I. B.E. and L.E.

The twins report that they are mistaken for each other very frequently by relatives, teachers, and friends, and sometimes by their father. Teachers report that they cannot tell the boys apart, in some instances, and others could not until they had seen them every day for three weeks. Their mother reports that she cannot tell they apart at a distance, and sometimes mistakes them when nearby if their backs are turned. She considers their facial expressions different, and notices that *L* has a somewhat fatter face. The examiner was unable to note these differences.

Disease History

Both have had measles and mumps, but neither has had whoping cough, scarlet fever, chickenpox, or smallpox. *B* had pneumonia (age 5 years) but *L* did not. Since that time *L* has been a bit stronger. Both are in good health, and are big, strong boys for their age (fourteen years).

Physical Measurements

	B.E.	*L.E.*
Height, inches	64.8	65.1
Weight, lbs.	114.	119.
Head length, Cm.	19.1	19.4
Head width, Cm.	15.0	14.1
Hair color	light brown	light brown
Hair color	N	N
Eye color	gray	gray

The letter designation of hair color was obtained by use of the Haarfarbentafel of Fischer and Saller. The letters of the alphabet in order indicate increasingly darker shades. This furnishes a more exact description than the mere naming of the color.

Reversal of Asymmetry

L is right-handed, and *B* reports that he is naturally left-handed, although he has learned to do most things with his right, such as writing or throwing a ball. He uses his left in eating, for example in using a soup-spoon. Results with a hand dynamometer indicate that *B*'s two hands are more nearly equal in strength than those of most right-handed persons. The results of the tapping tests are not definitely in agreement with the report on handedness. Data are furnished in the table below.

Tests of Handedness

		B.E.	*L.E.*
Strength of grip, in kilograms. Average of 8 trials with a hand dynamometer	R.	38.	43.5
	L.	36.5	39.
Speed of tapping. No. of taps in 10 seconds. Average of 7 trials	R.	62.1	58.4
	L.	53.6	45.1

Examination was made for position and direction of whorl of hair, as an additional indication of reversal of asymmetry. *B.E.* has a counter-clockwise whorl slightly to the left side; *L.E.* has a clockwise whorl slightly to the right side.

Fingerprints

The prints of all the twins are furnished in order that the reader may see the similarities and differences which exist. The police classification furnishes a rough indication of degree of similarity, and analysis by the methods outlined by Bonnevie[1] gives supplementary evidence. Although the similarities of finger prints of twins do not extend to the minute details, there is close resemblance here in type and shape of pattern and in quantitative measures based on ridge-counting. The similarity is much greater for twins *B.E.* and *L.E.* than is usual even for identical twins.

Other Evidences of Similarity

Both twins have a decayed upper and lower molar on both sides of the jaw. The teeth of both are crooked. Two of *B*'s front teeth show more decay than do *L*'s.

The following data on developmental phenomena were reported by the mother:

	B.E.	*L.E.*
Weight at birth, lbs.	8½	8
Age first walked (approx.)	1 yr.	1 yr.
First teeth (approx. age)	10 mo.	10 mo.
Age entered school	6 yrs.	6 yrs.
Voice changed (approx. age)	12 th yr.	12th yr. (about 3 mo. later than *B.*)

Since the mother has to rely on memory for these details, perhaps they are not extremely accurate, hence the writer does not wish to exaggerate their importance. However, the mother of twins would naturally notice *differences* in development if they occurred. In this case she reports that progress in locomation and in speech development proceeded at the same rate, and that there were no differences in age of cutting teeth. *B*'s voice changed about three months earlier than *L*'s. At present the voices of the twins seem alike to the examiner, although the mother considers *B*'s slightly more high-pitched.

The ears of the twins are very similar, both having a free lobe. They are like the father's in this respect. Their older brother's ears are much different, and like the mother's have no free lobe.

Similarity in body build is indicated by the fact that they can wear each other's clothes, including shoes, hats, coats, and trousers. Each reports that the other's clothes fit him very well.

Case II. Twins T.C. and D.C.

T.C. and *D.C.* are twin sisters 38 years of age. At present they seem superficially different in appearance, because *D* is tanned from exposure to the sun, and because she is much stouter. There is great similarity of body build, and both are inclined to be stout, but not to the same degree. *D.* has always been slightly larger and stronger.

When they were young, teachers, relatives, and close friends had great difficulty in telling them apart, up to the age of about 20 years. They report that both their parents sometimes mistook one for the other, the father more frequently. The examiner studied carefully pictures of the twins taken at ages 3, 4, and 16, and all these photographs revealed a striking similarity of the two girls.

Their disease histories as reported were very similar: both had measles, mumps, and whooping cough, at the same time. They did not have any of the other diseases of children.

Physical Measurements (at present)

	T.C.	*D.C.*
Height, in inches	63.2	63.5
Weight (reported)	160-165	190-195
Hair color	medium brown	medium brown
Hair color	P or Q	P or Q
Eye color	blue	blue
Head length, Cm.	18.2	18.7
Head width, Cm.	14.3	14.7

Both have a naturally light complexion, but *D*'s is more tanned as she spends more time outdoors. The designation of hair color by means of the Haarfarbentafel of Fischer and Saller was difficult, for their hair is somewhat faded, and gives the appearance of not being exactly like any of the samples. No clear indication of a whorl was discovered.

Reversal of Asymmetry

Both twins are right-handed, and report that neither has ever shown any tendency toward left-handedness. Two tests of speed of tapping and strength of grip were taken, with the results as indicated below.

	T.C.		*D.C.*	
Grip, in Kilograms	First	Second	First	Second
Right hand	22	25	35	38
Left hand	19	20	27	33
Speed of tapping. Taps in ten seconds				
Right hand	70	75	61	68
Left hand	63	63	61	63

The similarity of speed of tapping for her two hands may perhaps be an indication that *D* has a slight tendency toward being ambidextrous, but such a conclusion would not be warranted without more extensive study.

Examination of the finger prints indicates same-sided symmetry rather than mirror-imaging.

Other Evidences of Similarity

By inspection their teeth are similar. Both report they have poor teeth, and find it necessary to have considerable dental work done.

Although they are larger than average, they both have small shapely hands. The similarity in this respect is at once noticeable.

Their features are very similar. Both have round faces, small noses, and short upper lips. Both have freckles.

Case III. Twins I.R. and L.R.

When the twins were young, teachers, relatives, and friends had great difficulty in telling them apart. Their father sometimes mistook them, but their mother never did. Photographs taken at various ages were studied carefully by the examiner, and the indication was of great similarity. However, there are some slight differences in appearance, which give the impression of mirror-imaging.

At the present time it is not difficult to tell them apart, as *I* is much more active, and more alert intellectually. *L* is somewhat florid, while *I* is rather pale.

Physical Measurements

Height, inches	63.6	63.9
Weight, lbs.	143.5	146.
Head length, Cm.	19.3	19.8
Head width, Cm.	15.9	15.4
Hair color	white	white
Eye color	blue	blue

Reversal of Asymmetry

I is entirely right-handed, but *L* is left-handed in some things, using the left to hold a soup-spoon, etc., but the right for throwing a ball. Both use the right hand for writing.

Strength of Grip: Average of five trials with the hand dynamometer

Strength in kilograms:	*I.R.*	*L.R.*
Right hand	38.4	23.8
Left hand	32.2	16.2

Because of *L*'s incoordination, tests of speed of tapping were not attempted. These results serve to show quite clearly the differences between the two, but are probably not very useful as indications of right- or left-handedness.

Because of baldness (they are over 60 years of age) no data concerning whorl of hair are obtainable.

The indication from study of the finger prints is that of same-sided symmetry.

Other Evidences of Similarity

It is reported by an older brother that the twins learned to walk and talk at the same time, and have always been much alike in development. They are much smaller than their other brothers.

Neither had any trouble with his teeth until past middle age. Both lost their teeth suddenly, supposedly from the effects of pyorrhea. *L* preceded *I* by a few years in this respect.

About 30 years ago *L* found it necessary to wear glasses. A few years later *I* also had to get glasses.

The ears of the twins are similar in appearance, and all four ears have free lobes.

The twins can wear each other's clothes, including shoes, hats, gloves, coats, and trousers. These fit well, except that *L*'s shoes and hats are a trifle larger.

Unsolved Problems of Evolution

THE PROBLEMS OF EVOLUTION, *by* A. W. LINDSEY, Professor of Zoology at Dennison University. Pp. 236. Price $2.00. The Macmillan Co., New York. 1931.

WISELY has this title been chosen, for the book presents a series of evolutionary problems and leaves them still as problems—unsolved, except perhaps for the author's reserved opinion on what the answer might turn out to be eventually. His analysis is scholarly although at times somewhat dry.

Recent advances in evolutionary science or philosophy are so fragmentary and inconclusive that it is questionable if anyone is ready to do more than Lindsey has done in merely discussing the existing state of affairs. He has incorporated some of the newer genetic aspects but unfortunately has not included the recent, brilliant contributions by R. A. Fisher as summarized in the "Genetical Theory of Natural Selection."

Lindsey's own interpretation of evolutionary processes is neither Darwinian, Lamarckian nor mutational. He attempts a correlation of the three that savors, however, of Lamarckism:

The modification of the heritage may be sought in the variable environmental conditions which give it the opportunity for variable expression of its inherent properties. Acquired characters are cytoplasmic in their expression, but so are inherited characters. Cytoplasm is the material of construction in which all characters are wrought.

Two interpretations of the activity of the chromosomes in the development of acquired characters are possible. We may conclude that they always exert the same influence and that the cytoplasmic result is cumulative, or that the chromosomes also undergo a cumulative change which is responsible for the condition of the cytoplasm, that the ultimate expression of the acquired character is the result of an actual increase through use of the functional capacity of the genes involved in its production, and since the chromosomes must grow and reproduce like all living things, we cannot avoid the possibility that, in spite of their remoteness, a residuum of influence may reach them from the surrounding body.

All this seems like common sense, but like the older, historical common sense of Lamarck and of Darwin, it should not be, as it was and still is, so infernally refractory to experimental proof.

Lindsey's book is not particularly adapted to the lay reader but should be extremely useful to advanced students in biology.

E. W. LINDSTROM.

SOME DIOECIOUS PLANTS

E. N. Bressman

Associate Professor of Farm Crops, Oregon Agricultural College

A DIOECIOUS COMPOSITE

Figure 6

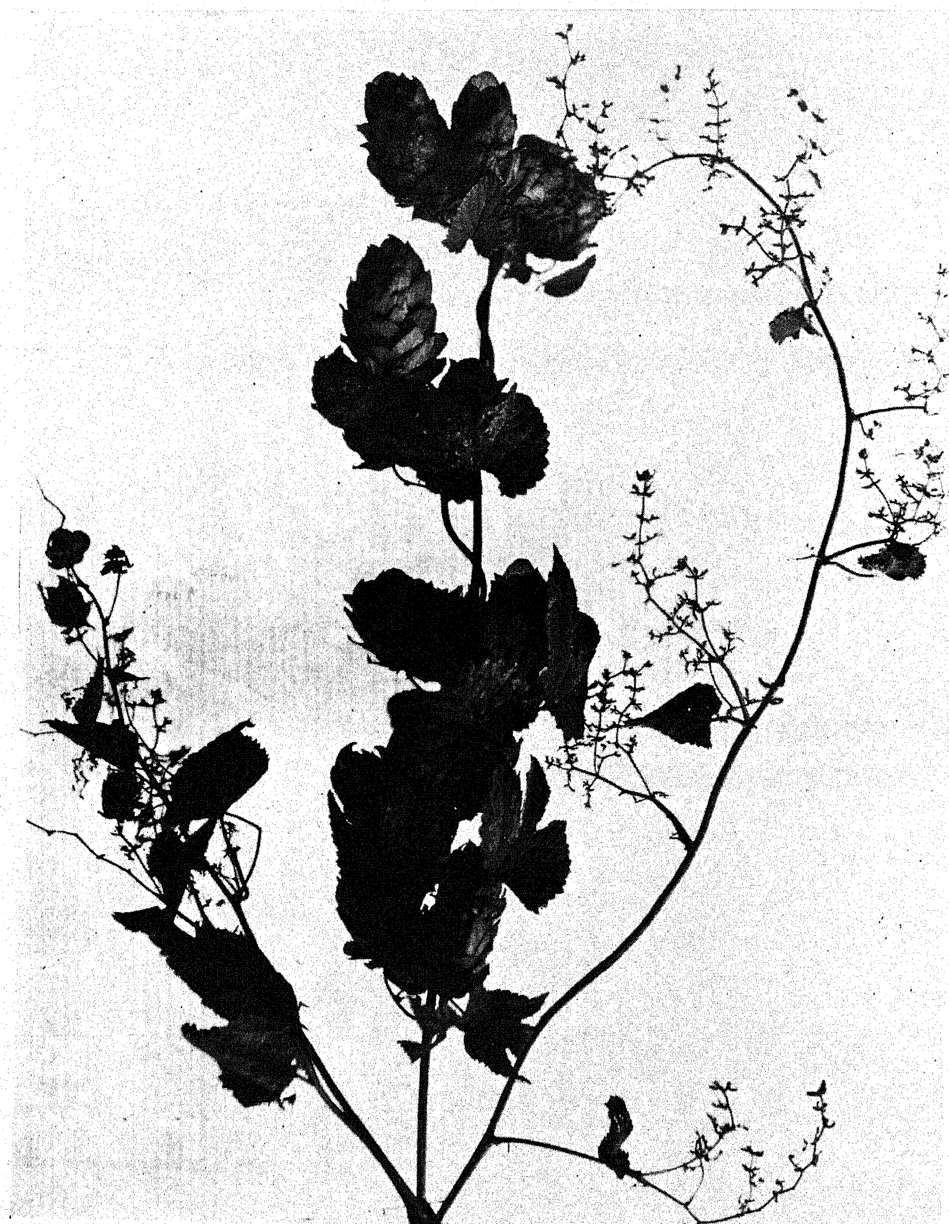
It is not generally realized that the Canada thistle (*Cirsium arvense*) is a dioecious plant. In this picture a patch of female thistles is shown (with the white heads) and a patch of male thistles in the background, without the white heads. The fact that the plants occur in patches all of the same sex probably accounts for the mistaken idea that the Canada thistle does not produce seeds. It is amply fertile when both male and female plants are present.

It appears that the early Assyrians and Egyptians many centuries ago knew that there were both male and female trees of the date palm. Their knowledge of sex in plants, however, was not extended to other plants nor known to botanists, and it was not until the experiments of Camerarius in the last of the 17th Century that sex in plants was demonstrated for the benefit of following generations. Our knowledge of this important phase of plant development dates back to his work.

Even today there are still differences of opinion in regard to inheritance of sex in plants. One group is of the opinion that sex is entirely Mendelian and governed by genes (factors) in the same way that other morphological characters are inherited. The other group believes that sex is due to physiological causes acted on by the environment. Perhaps both are right, at least in part.

At any rate, the dioecious plants; that is, plants which bear staminate and pistillate flowers on different individuals, furnish us a better understanding of sex in plants than any other of the members of this kingdom.

The number of plant genera which are dioecious is surprisingly large. Illustrations of this condition in plants



SEX REVERSAL IN HOPS

Figure 7

The female flowers are the cone-like structures on the central branch near the tip. The finely branched male flowers are on the branch at the right. The branch at the left is hermaphrodite, having both male and female flowers. This is a plant of male hops, which has, for reasons not clearly understood, produced some female flowers. A study of sex phenomena in dioecious plants is valuable in throwing light on the physiology of sex, and is of direct practical importance to breeders of economic plants.

usually include two or three common examples, such as the date palm, hemp, and hops. There are many other plants, however, which have this condition.

Most of the dioecious types are found in the lower groups of flowering plants, such as the willows, box elders, ashes, and similar trees. Others occur in the hemp, hop, mulberry, buffalo grass, some cucurbits, poplars, nettles, some salt bushes, osage orange, mulberry and asparagus.

One of the very highest groups of flowering plants contains a dioecious genus. This plant is the well known Canada thistle, *Cirsium arvense,* in the compositae (see Figure 6). Most of these are dicotyledonous plants, and most of the dioecious plants are found in this division. That they are not limited to this division is indicated by the dioecious condition of the date palm, which is a monocotyledonous plant.

In almost every case there are exceptions to the dioecious condition and some plants which are monoecious are invariably found. In some genera this variation is greater than in others. Hops, for example, exhibit both dioecious and monoecious plants, but the latter are rather rare, and occur as a rule on plants which were formerly males; that is, a male plant will exhibit male flowers and then show some female flowers, and finally perhaps exhibit the male condition again. Growers often make the statement that male plants will change to females. This was rather hard to believe until some of the recent work had shown that plants of hemp, for example, could be reversed in sex by the proper environmental conditions.

Recent work by Schaffner[2] shows that by planting hemp, *Cannabis sativa,* at different months of the year in the greenhouse one can make the plants reverse their sex. His work has a fundamental application to this whole problem of sex, as well as a practical application where certain sexes of plants are desired. Similar reversals in male hops have been noted by the writer (see Figure 7).

The difference in sex of plants of Canada thistle has been puzzling to many, and the literature is replete with misinformation in regard to this plant. Detmers[1] made a survey of the thistles in Ohio. She states that the two types of heads and florets were found. The staminate ones were oblong, while the carpellate heads were ovoid or flask-shaped. Contrary to the opinion of many other writers, she found that the plants are dioecious, and that a patch of thistles may be either staminate or carpellate. She is of the opinion that staminate and carpellate plants must be near enough to insure pollination if seeds are to be formed. This readily accounts for the mistaken idea in some sections that Canada thistles do not produce seed.

Some of the newer conceptions in animal breeding have been taken from the work with plants, because larger numbers could be worked with and results obtained at a far smaller cost and in a shorter time. It is surprising that little of this work has been done with dioecious plants. It seems that much of the work would be more applicable to animal breeding if plants of this type were used. The writer is just beginning experiments in hop breeding work. This dioecious plant is exhibiting some interesting things in its seedlings. It is hoped that in addition to the development of varieties of economic value that some light may be shed on the development of the different sexes on particular plants.

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Photograph from H. E. Zimmerman

NO RACE SUICIDE HERE

Figure 8

A French-Canadian family of twenty-one children, including five sets of twins. In the days when storks rampant might have been carried on the escutcheon of any New England family, such an amazing collection of children as this would hardly have qualified as "news." Today, thanks to the prevalent one- and two-child families, an obituary of the New England Puritans is in order. While a levelling off of population growth curves is not necessarily a catastrophe, it may be such if the large families (*and there still are large families*) are being produced principally by the less gifted components of the population. The battle of births and deaths goes on always, and when it comes time to write racial and class obituaries it is likely to be almost too late to discuss to any purpose questions of racial betterment.

THIS WORD "FAMILY"

The announcement of an article on "Does Family Size Run in Families?" elicited the comment from a correspondent that such a double-barrelled use of a word by a publication committed to definitions of genetic terms deserved the extreme penalty. It was pointed out that nearly a score of distinct meanings of "family" were in current use, and the word was nominated for inclusion in our Glossary. Such formal recognition of the word's parlous state is declined however, because the entire English language would soon be crowding our third cover were the precedent admitted. After trying out several other concatenations of words we are letting the title remain as Dr. Huestis originally submitted it. The other words available had almost as many definitions as "family" and none seemed an improvement. "Sibling" is "out," since its recent exposé in William Lyon Phelp's column in *Scribner's Magazine;* "inheritance" has legal connetations entirely misleading; "progeny" might be forced to serve, but does not really fit.

It is a curious fact that in spite of the multiplicity of meanings which "family" may convey, it is nevertheless true that this title probably will not mislead many people. This instance raises interesting questions in the problem, or problems, of explaining what words mean, and of the relative merit of priority and of use in their definition. To these questions there seems to be no satisfactory answer.

THE NEW ENGLAND PURITANS:

An Obituary

J. J. SPENGLER
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The people (of New England) are purer English blood; less mixed with Scotch, Irish, Dutch, French, Danish, Swedish, etc., than any other; and descended from Englishmen, too, who left Europe in purer times than the present, and less tainted with corruption than those they left behind them.—JOHN ADAMS.

WHEN these words were penned by John Adams more than a century ago the stork was as truly suited as the pine tree to grace the standards of New England. Women often were grandmothers before they had rounded out their fortieth year. Frequently, under the same roof, one found a mother suckling a ninth born and a daughter suckling a first born. The average number of births per family, according to Benjamin Franklin, was about eight, a figure practically substantiated by the recent researches of A. J. Lotka. From the loins of the New Englanders sprang a large part of the population which gradually flowed westward into the vast unsettled area lying east of the Mississippi.

The nineteenth century, however, witnessed a complete reversal in those folkways of the New Englanders which concerned procreation. Family size had already begun to shrink in certain classes of the population when President Madison remarked, less than a decade after the War of 1812, that "New England . . . . has continued to increase" despite the lack of immigrants and a steady efflux "to other parts of the Union." The civilization which developed in nineteenth century New England and which swept steadily westward, gradually stifled the procreative impulse until today the native women of New England are bearing too few children to replace themselves. Fertility among the foreign born women has declined in a similar fashion and will soon be on a parity with that of the natives.

Evidence of the decline in fertility in New England is to be found in genealogical records and in the birth returns published for various parts of New England. Three years after the Civil War, Dr. Nathen Allen of Lowell, Massachusetts, cited genealogical records to show that the average number of children per family had fallen from eight to ten in the seventeenth century to three at the time of the Civil War. F. S. Crum gives these averages for children per wife in New England and the Middle Atlantic States: before 1700, 7.37; 1700-49, 6.83; 1750-99, 6.43; 1800-49, 4.94; 1850-69, 3.47; 1870-79, 2.77. Other genealogical records reveal a similar trend. Among college graduates in the nineteenth century lower averages obtained. In so far as one can generalize from the genealogical records it appears that the decline became marked during and after the third decade of the nineteenth century.

A better picture of the general trend is to be found in the birth returns for the several New England States and for several of the cities in New England. These we shall consider in order. The average annual number of births per 1,000 native women, aged 15 to 49 years, is given in Table I. Corresponding averages for foreign born women are given in Table II.

I

The rates given in Table I reveal no definite downward trend in the fertility of the native women between the period of the Civil War and the period immediately following the World War. In Massachusetts, however, the pre-Civil War rate of 76 was never touched after

1860. In every state we find higher rates for every period since 1911-1915 than were observed in that period. To what extent this increase since 1911-15 is a true increase and to what extent it is the result of a more complete registration of births it is impossible to say. Improvement in birth registration does account, in part, for the failure of the fertility rate to decrease after 1865 and for the increase since 1910. In every state the rates were higher in 1921-25 than in 1919-20. Since 1921-25, however, native fertility has decreased in every state. This decrease is apparently the result of an increase in family limitation and not the result of a change in age composition. Comparison of the age composition of the native female population living in New England in 1920 with that living there in 1930 indicates that in 1930 the total number of births to native mothers ought to exceed the number registered in 1919-20

TABLE I. The Average Annual Number of Births Per Each 1,000 Native Females aged 15 to 45*

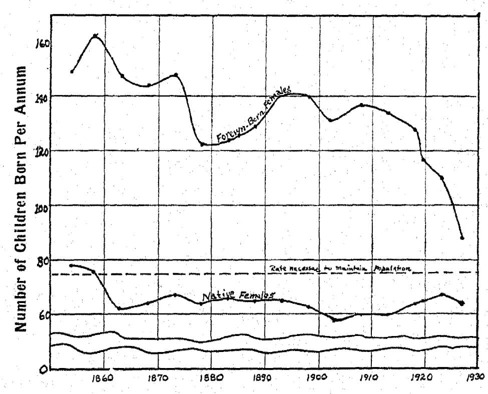
Years	Maine	New Hampshire	Vermont	Massachusetts	Rhode Island	Connecticut	New England
1853-55				78			
1856-60				76	65[a]		
1861-65				62	56		
1866-70				64	64		
1871-75				67	67		
1876-80				64	67	63[b]	
1881-85		53[c]		66	63	65	
1886-90		53		65	60	64	
1891-95	70[d]	57		65	61	64	65
1896-1900	69	57	76[e]	63	61	60	65
1901-05	69	57	76	58	59	57	64
1906-10	75	62	77	60	59	55	62
1911-15	75	65	77	60	59	56	62
1916-20	79	71	80	64	65	65	67
1919-20	81	73	79	65	64	65	68
1921-25	86	78	85	67	69	63	70
1926-28	81	72	80	64	66	60	67

* Rates for 1919-20, 1921-25, 1926-28 are for white population only; Rates for 1853-1920 are for colored and white. As colored are relatively few in number exclusion or inclusion does not affect rates. (a) 1857-60; (b) 1878-80; (c) 1884-85; (d) 1892-95; (e) 1900.

TABLE II. The average Annual Number of Births Per Each 1,000 Foreign-born Females, Aged 15 to 49†

Years	Maine	New Hampshire	Vermont	Massachusetts	Rhode Island	Connecticut	New England
1853-55				149			
1856-60				162	134		
1861-65				147	125		
1866-70				144	126		
1871-75				148	117		
1876-80				122	108	127	
1881-85		104		123	108	129	
1886-90		108		129	113	123	
1891-95	142	126		141	129	135	138
1896-1900	142	131	127	140	138	133	138
1901-05	137	127	147	131	134	135	133
1906-10	138	132	143	137	133	153	139
1911-15	122	121	131	134	125	161	137
1916-20	116	118	127	128	128	166	134
1919-20	115	115	117	117	127	143	125
1921-25	128	112	117	110	112	119	113
1926-28	110	90	105	88	92	90	91

† See footnote under Table I.



TRENDS IN BIRTH RATIOS

Figure 9

The average annual number of births for each 1,000 females aged 15 to 45 in the population of Massachusetts—native and foreign born. The surprising decline in the foreign-born ratio and the slight increase in the native ratio means that at the present time the future population of Massachusetts is being determined not by relative increases, but by relative rates of dying out! This chart is based on the data in Table I.

by 17 per cent. When the 1930 figures are made available, however, they will exceed the 1919-20 figures by only about 13 per cent. It is apparent, therefore, that fertility has declined among the native women since 1919-20, for changes in martial composition will probably prove inadequate to account for the lessened frequency of births.

The rates for the foreign-born women reveal that among them fertility has declined. In every state, for which data are given, the rates for the foreign-born declined after the Civil War until the closing decade of the nineteenth century when the foreign rate increased in every state but Maine. This increase is apparently the result of two factors: (a) a change in the ethnic composition of the foreign-born and (b) an improvement in the marital composition of the foreign-born. After 1890 the immigrant population was recruited in increasing proportion from Southern and Eastern Europe (Italians, Russians, Poles, etc.) among whom larger families and a greater frequency of births were found than among the Irish, British, Germans, etc. Between 1890 and 1920, of the foreign-born women aged 15 to 44 years, the percentage who were married increased from 53.8 per cent to 72.2 per cent. This improvement in marital composition is in part a result of the changed ethnic composition of the foreign-born population for, since the ratio of males to females was relatively higher among the immigrants from Southern and Eastern Europe the frequency of marriage among the females was higher than among the immigrants from Northern and Western Europe. Since 1915 foreign fertility has declined in four of the New England states and since 1920 the decline is observed in each state. In 1926-28 foreign fertility in New England was 34 per cent below the 1911-15 level and 27 per cent below the 1919-20 level. Part of this decline, only, is attributable to an increasing unfavorableness in age composition. Comparison of the age composition of the foreign females in 1920 and 1930 indicates that in the latter year they should have borne one fifth less children than in 1919-20. In 1928 they bore 38 per cent less and in 1930 nearly fifty per cent less. Foreign fertility therefore appears to be falling off sharply.

Comparison of the native and foreign rates reveals that maternity has always been more frequent among the foreign-born than among the native-born women. Until recently the foreign rate has often been at least twice as high as the native rate. Since the foreign rate is shrinking more rapidly than the native rate the two rates are rapidly approaching each other in magnitude. Thirty-five years ago the foreign rate in New England exceeded the native rate by 116 per cent; in 1919-20 this superiority was 84 per cent; in

1926-28 only 36 per cent. Much of this superiority is due to the more favorable marital composition of the foreign-born, just as much of the superiority of the fertility of natives living in Maine, New Hampshire, and Vermont is due to the more favorable marital composition. In light of the fact that the marital composition of the foreign born is no longer improving and in light of the fact that the practice of birth restriction is now dominant in the regions from which most of the immigrants come, the fertility of the shrinking immigrant population will soon approximate that of the natives.

II

In Providence, Rhode Island, we observe a trend similar to that observed in the New England states. The observed rates for native- and for foreign-born women have been as shown in Table III, since 1891.

The rates in Table III indicate that between 1891 and 1929 fertility for all women, 15 to 44, declined 18 per cent among the natives and 39 per cent among the foreign-born. Fertility among the native married women declined 8 per cent while that among the foreign-born declined 26 per cent. The excess of foreign over the native rate for all women fell from 124 per cent in 1891 to 67 per cent in 1928-29. Among the married women the superiority of the foreign-born dropped from 76 per cent in 1891 to 42 per cent in 1916-20. In short among all women and among the married women fertility has declined more rapidly among the native than among the foreign-born and the levels of fertility in the two groups are approaching each other.

If the fertility rates for native and foreign-born married women are corrected for the approximate number of births which escaped registration we find rates of 206 and 324 in 1874-76 and 177 and 317 in 1894-96. Between 1875 and 1895, accordingly, fertility among the native married women declined 14 per cent whereas that among the foreign-born declined only two per cent. The shrinkage since 1875 has been 25 per cent in the native and 32 per cent in the foreign married fertility. Convergence of native and foreign married fertility thus appears to be a twentieth century phenomenon.

Comparison of the fertility of Boston with that of Providence reveals a somewhat similar trend. In 1845[2] the native and foreign birth rates in Boston were 22 and 61 compared to Providence rates of 20 and 64 in 1856. Newsholme and Stevenson give as the corrected legitimate rates, (not adjusted for unregistered births), for native and foreign-born in 1875 in Providence, 16-10 and 27.79. In 1900 these were: Boston 18.2 and 31.1; Providence 16.00 and 31.1. In 1845 the births per 1,000 married women, aged 15 to 44 years, approximated those of Providence in 1875, being 199 among the native and 290 among the foreign-born married women. In 1919-20 the corresponding rates were 204 for the natives and 206 for the foreign-born married women. Assuming that all births were enumerated in Boston in 1845 and that in 1919-20 but few births were registered in Boston to non-resident mothers, it appears that in 1919-20 fertility among the native married women was nearly three per cent higher than in 1845 whereas fertility had declined 29 per cent among the foreign married. If we compare the age compositions of 1845 and 1920 we find that in 1920 native married women should have borne 318 per cent more children than in 1845 whereas they bore 358 per cent more. The foreign-born married should have borne 614 per cent more whereas they bore 491 per cent more than in 1845. In light of these figures the true fertility of the natives increased about 10 per cent while that of the foreign-born decreased 17 per cent between 1845 and 1920. If we al-

low for births to non-resident mothers,* however, the shrinkage in the fertility of native and foreign women in Boston since 1845 probably approximates the corresponding shrinkage in Providence since 1875.

What the shrinkage was in native fertility[3] prior to 1850 we do not know. In Rhode Island in 1776 the ratio of white children 16 years old or less to females above 16 was 1,636 per 1,000. In 1920 in Rhode Island the corresponding ratio for natives was 619; for foreign-born, 1,386; for the entire state, 928. The reduction in this ratio indicates a marked decline in fertility, especially among the natives, even when allowance is made for greater longevity in 1920.

In Boston, according to Lemuel Shattuck,[2] the birth rate was close to 50 per 1,000 during the half century preceding the American Revolution. This estimate is based on an observed rate of 30 to 34 christenings per 1,000 inhabitants in Boston between 1725 and 1754. In 1783 Edward Augustus Holyoke[4] presented a bill of mortality for Salem for the years 1782 and 1783. If we accept his population estimate of 9,000 we get birth rates of 35 and 43 for 1782 and 1783 and a death rate of 19 each year. Infant mortality rates were 114 and 99. As Holyoke's bill is only "as compleat as can be expected" in respect to births and as by no means all children were christened in Boston after 1700, it is probable that the crude birth rate ranged between 40 and 50 per 1,000 in the eighteenth century. In other words in the eighteenth century the crude birth rate was more than twice as high as that observed among the Boston natives in 1845 and at least three times as high as that observed in Boston in 1920, allowance being made for births to non-resident mothers.

III

Have the natives of New England been replacing themselves or have they been dying out as was several times contended in the latter part of the last century? A direct answer is impossible inasmuch as the necessary information is not available. That it is prob-

TABLE III. The Average Annual Number of Births Per 1,000 Women, Aged 15 to 44, and Per 1,000 Married Women Aged 15 to 44 Years, by Nativity, In Providence†

Year	Births per 1,000 Women, 15 - 44		Births per 1,000 Married Women, 15 - 44	
	Native	Foreign	Native	Foreign
1891	71	159	169	298
1894-96	64	158	158	285
1899-1901	57	148	140	256
1904-06	58	149	------	------
1909-11	57	147	138	236
1916-20	82	157	155	220
1924-26	69-84	122	------	------
1928-29	58	97	------	------

† An increasing number of births, not classified by nativity, are to non-resident mothers. Rates for all women 1916-20, are not adjusted therefor; rates for native married women, 1916-20, are reduced one tenth; foreigns, one twentieth. Rates for native women, 1924-26, are reduced one fifth to one seventh; foreign, one seventh. In 1928-29 native rates are reduced three tenths; foreign, one seventh.

* That fertility should be higher among the native married women in 1919-20 than in 1845 is unbelievable despite the fact that a larger per cent of the natives were of foreign parentage in 1920 than in 1845. Births to mothers who came to Boston for delivery in 1919-20, or an incomplete enumeration of births in 1845, must explain this apparent increase in real fertility. Children of white maternity, under 5 years of age, numbered 69,573 in Boston in 1920. These represent the survivors of about 76,500 births, giving an annual average for 1916-20 of 15,300 births, of white maternity compared with a reported annual average of 18,673 in 1919-20. If 6,000 of these are births to native white mothers we must reduce our 1919-20 rate for married women by about 30 per cent to 143. This rate would indicate a decline of about one fourth in the fertility rate of native married women of Boston between 1845 and 1920.

able that they have been dying out is easily demonstrated, however.[5] Assuming that every woman born reaches the age of 50 and thus completes 35 years of childbearing, 1,000 women could replace themselves if, as a group, they averaged 28.6 female births per year for 35 years (1,000 ÷ 35 = 28.6). But in bearing 28.6 females per year they would bear about 59 children per annum, for of each 205 births only 100 are females. In short if every woman completed the fiftieth year a fertility rate of 59 births per 1,000 women aged 15 to 49 years would approximately replace the population. In proportion as women do not reach the age of 50 this required replacement rate of 59 must be increased. In 1870, in light of the most favorable mortality conditions of Europe, this rate needed to be about 86; in 1890, about 82; in 1910 about 75. If we compare the observed rates given in Table I with these required rates it is apparent that in Massachusetts, Rhode Island, Connecticut, and possibly in New Hampshire the natives have been dying out since the time of the Civil War, or since rates are available. Such appears to have been true also of the natives who lived in Providence, Rhode Island, and who in light of comparison with European rates, maintained a level of fertility in the last three decades of the nineteenth century as low as that in any European city with the possible exception of Paris.

Present data and methods permit a more careful answer to the question as to whether or not the natives of New England are dying out. P. K. Whelpton[6] has shown that in 1920 the native whites of Massachusetts and Connecticut were still dying out and that in 1928 the entire white population of these two states was dying out.

Thompson's[7] recent study of the 1920 census reveals a 15 per cent deficit for New England in 1920, this deficit being greatest in cities of 100,000 and over and smallest in cities of 10,000 to 25,000. Only in the rural areas and in Maine and Vermont is there natural increase in the native population of New England. Deficits of 25 per cent are found in Massachusetts and Rhode Island; in Connecticut, 20 per cent; in New Hampshire, 10 per cent. A greater deficit (20 per cent) is found in the Pacific Coast Area than in New England as a whole. In the Middle Atlantic States a deficit of 10 per cent is found. In other sections some natural increase is observable. However, when the complete data of the 1930 census are available, it will be apparent that in the United States as a whole the population no longer shows a true rate of increase.

In summary we may say that shortly after the Civil War fertility among the New England native declined to a level insufficient to maintain the population. Other parts of the United States to which the culture of New England has spread are now marked by a similar birth strike even as are European countries with a culture similar to that found in New England. Western Civilization with its marked inequalities in income and its emphasis upon material achievement is rapidly destroying the will to procreate.

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3. *Census of Rhode Island, 1776,* edited by John Bartlett, 1858.

4. *Memoirs of the American Academy of Arts and Sciences,* 1783, 546-550.

5. For more careful discussion see the writer's article in the *Quarterly Journal of Economics,* August, 1930; also Kuczynski, R.R., "Fertility and Reproduction," Brookings Institution, Washington, 1931.

6. See *American Journal of Sociology,* May, 1930, p. 872; *Journal of American Statistical Association,* September, 1929, p. 238.

7. See "Ratio of Children to Women—1920," Census Monograph XI, 1931.

DOES FAMILY SIZE RUN IN FAMILIES?

R. R. Huestis and Aline Maxwell

University of Oregon

MILDLY astonished, for the most part, that their affairs should be an object of scientific enquiry, some seven hundred of our university students filled out questionnaire blanks upon the number of children born in their own and in their parent's families. The blanks were sent to their parents' homes to be checked and more than ninety per cent were returned to us. This commendable demonstration of parental interest, combined with the evidence of care taken in the filling out and checking of the blanks, gives us a considerable measure of confidence in the reliability of the figures with which we were presented, a confidence we hope we can establish also in the minds of our readers. Certain particulars contained in the blanks have been analysed and reported upon elsewhere.[5] The present study, which includes a few data which arrived too late for inclusion in our previous report, was arranged to test the truth of a supposition that is widely current among eugenicists.

Depopulation Dangers

Let us begin a brief outline of this with the platitude that the general decline in the birth-rate has been an outstanding sociological phenomenon of the last three-quarters of a century. There are no very complete studies of the amount of complete or relative organic sterility among human beings. However, certain aspects of their behavior are so patent that there is a general agreement, among authorities, that the increase in the knowledge and use of contraceptives will account adequately for the continued reduction in family size. It has been freely predicted, too, that as this knowledge seeps downward from its centre of origin among the illuminati, to more and more completely film the moiling masses of the unlettered, they will become unworthy of their title proletarians, and depopulation will begin.

Indeed, Kuczynski[4] has demonstrated, seemingly beyond a doubt, that the present birth rate in northern and western Europe is inadequate to maintain the present population. Dublin and Lotka[2] have shown, in a similar study of the exuberantly growing United States, that the birth rate little more than suffices for replacement. The paradox of an inadequate birth-rate and present growth is readily explained by the present age grouping. Due to the previous very high birth rate, in the population in question, there is now a large number of people in their twenties and thirties, ages in which they will reproduce, if they are going to, and ages, incidentally, in which they do not often die. An abundance of people of reproductive age naturally produces a greater number of births than deaths. This wave of young adults, carrying on through more and more advanced age decennials, will soon break upon the sands of eternity, and since no waves of equal magnitude will follow, the tide of population must ebb.

What Controls Family Size?

We do not think we can be charged with arranging our own intellectual set-up, when we say that there is a common reply to such omens of depopulation. It is to the effect that there are probably genetic differences in the desire for children as well as in ability to have them, comparable to differences in height, body build and the like, but that until recently esotericism of contraceptive knowledge has forced parentage upon most

people whether it was desired or not. With a more general distribution of information, the people who lack congenital philoprogenitiveness will cease to breed. Since this will entail a relative increase of "born" parents, these latter will check depopulation by having as many children as their environment will happily allow.

This argument has been advanced, for example, by Alexander Graham Bell[1] and by R. A. Fisher.[3] It is reasonable enough to go unattacked in eugenic circles, but should the eugenicist present it to the people more inclined to explain human behavior in terms of environment he will often be confronted with the following questions:

"Don't you think parental desire can usually be satisfied by one or two children?" "Don't you think that people who grow up in a larger than average family become so conscious of its economic disadvantage that they decide not to have one of their own?" "Don't you think it is largely a matter of what the people do that you live among, and that even if a women would like another baby, she will not have one if it is going to make her appear different?" Upon taking thought, one can see that in so complex a matter as the desire for a family large enough, on the average, to maintain a given population, the premise that there are hereditary differences in love of offspring, which will fundamentally direct things, needs a searching examination. We therefore welcomed the opportunity to test the amount of resemblance in family size in two generations of families now sending students to the University of Oregon.

Family Size in Two Generations

Decisions as to family size lies oftener with the mother than with the father of the family and relative sterility is more often to be found in women than in men. We decided, therefore, that the fairest test of the matter in hand would be to find the amount of correlation present between the number of children born in the families in which students' mothers were included, and the number of children born in the families which students' mothers had. This would be essentially a test of parent-offspring correlation in family size and thus comparable with similar tests for parent-offspring correlation in height, eye color or any other known hereditary character.

A word might be said here concerning the appropriateness of the use of the population employed. Very few of our families would be uncompleted, for most students are at least 18 years of age at college entrance. We did not tabulate any blanks handed in by Asiatic students so the population is exclusively white and is, moreover, largely derived from other parts of the United States or from Northwestern Europe. We were rather surprised to discover that where denominational preference was tabulated it was largely Protestant, only 45 out of 638 mothers being listed Catholic. Finally our population is one in which pronounced reduction in family size has taken place in one generation, 638 mothers having been born in families averaging 5.53 ± .07 children while these mothers had families averaging 3.28 ± .05 children; an average reduction of over two children per family, in a generation.

The statistical data are arranged in Table I. The material was divided into a group of families listed by students as having the advantages of completely rural upbringing and a group containing their more urbanized colleagues because we knew that we had a fair number of families in the former class and that their average size was somewhat greater. Both groups, it may be seen, exhibit small positive correlation coefficients in family size. It may be noted that in the composite total population the coefficient of correlation is slightly larger than that of either group taken

separately, a situation to be expected when a correlation table includes an unhomogenious array of variables. It should be mentioned, too, that we could only assume that both generations of city and country families had been reared for the most part in that environment. We knew that the last one had, and found that city families were substantially smaller in both generations.

From an examination of our table we may conclude that family size does run in families but not to any great degree, for if we had been testing the same population for height or eye color, we could have confidently expected coefficients of three or four times the magnitude of the ones we obtained for likeness in family size.

There are probably a number of circumstances which would tend to reduce parent-offspring correlation in this latter character. We cannot separate functional from volitional sterility. People from small families in the same society, who are themselves sterile, could not appear in our table although they might be exhibiting good evidence of congenital sterility. The environment of the two generations considered was not necessarily always the same, for the parents of many of our urban students were probably reared in the country and had their own families in the city.

From a theoretical standpoint, of course, accurate determination of the inheritance of a character should be made in a constant environment. However, it is also true that, to the extent that environmental differences affect the expression of a character, hereditary differences in it become of lesser practical importance. Our conclusion in this instance, based upon data which we freely admit does not by any means dispose of the question, is that family size is a character which at present is much more affected by the environment than by congenital differences in love of children or ability to have them. Reduction in family size is, as yet, a pretty general matter.

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TABLE I. Mean number of children born in 638 Families sending children to University of Oregon

	In 118 country families	In 520 city families	In all 638 families
In which students' mothers were born (x)	6.08 ± .18	3.11 ± .05	5.53 ± .07
Which students' mothers have (y)	4.05 ± .15	5.41 ± .08	3.28 ± .05
Correlation of x and y; r=	+ .104 ± .06	+ .111 ± .03	+ .124 ± .02

WHERE ANGELS FEAR TO TREAD?

A Review

WHAT IS LIFE? *by* AUGUSTA GASKELL. Introductions by Karl T. Compton and Raymond Pearl. Pp. 324. 3 Parts, 9 Chapters. Springfield, Ohio. Charles C. Thomas. 1928.

CAN SCIENCE EXPLAIN LIFE? *by* CARL F. KRAFT. Pp. 94. Price $1.00. Lancaster, Pa. The Science Press Printing Co. 1931.

MRS. GASKELL'S book, "What is Life?", is of interest because of its contained introductions by two such eminent scientists as Karl T. Compton and Raymond Pearl. Both are careful not to state that they actually believe in Mrs. Gaskell's hypotheses, and they make various reservations and qualifications, yet both are commendatory in their general tone, and endeavor to disarm in advance, as "dogmatic," the general condemnation of the book which they evidently anticipate. According to Compton, "her discussion of atomic physics is accurate, well balanced and worth reading for its own sake . . . The book stimulates serious thought and it is to be hoped that it will contribute to the successful solution of the problem 'What is Life?'" According to Pearl, "Mrs. Gaskell's discussions of various biological problems, particularly evolution, have a refreshing novelty and shrewdness which gives them a value by no means negligible." The book is characterized by Pearl as "extremely stimulating and original." Furthermore, he says, "the present is a propitious time for the appearance of Mrs. Gaskell's original and ingenious speculation."

X Equals Y Plus Z!

To the present reviewer the book represents an attempt at wish-fulfillment that is very thinly veiled indeed, and that follows closely the old traditional forms, albeit in terms of the patter of popular physics, and of an indiscriminately hashed assortment of biology. The most essential feature of the Gaskell complex, as I see it, is the archaic doctrine that "the organism is a dual system," composed of a material body, denoted as "Y" by Mrs. Gaskell, and an "immaterial" component, frankly identified with "the soul," and denoted as "Z" ("Zoe"), which suddenly leaves the body at death, for parts presumably unknown. It is the "Z" which organizes "Y" and which completely differentiates all living from all lifeless matter.

As it is the purpose of the book to rationalize "Z" into existence, the author erects a pseudo-scientific structure of protons and electrons, combined in a manner yet unknown to physics, and different from the combinations of proton and electrons known to exist in the atoms of ordinary matter. The primary difference is supposed to consist only in the fact that, in "Z", the electron is much nearer to the proton than is the orbital electron of any known atom. The resulting neutral combination is then conveniently supposed to have such "peculiar" (though not well-specified) physical properties, as would account for its having escaped the detection of physicists hitherto. Such a combination is arbitrarily assumed to be produced when an electron chances to approach too close to a proton, owing to some previous crowding of ions. Having now placed her creature, "Z," entirely in the realm of the unknown, the author has given herself *carte blanche* to make whatever she wishes of it. She supposes it to act as a kind of intra-atomic parasite when it is mixed with ordinary matter, a parasite that causes abnormal growths in its host. For the newly born "Z" (we cannot say "Z-material," since *by definition* this combination cannot be *called* material!) sets to work and manufactures more "Z" by disrupting and appropriating the nuclei of the atoms of the ordinary material around it,

and at the same time it organizes the remainder of the surrounding material into the form of "Y" characteristic of the species. Thus there necessarily (!) result protoplasm, growth, reproduction, mind, etc.*

As to why or how the construction of more "Z" should result from the activity of the first bit of "Z"; as to why or how "Y" should become organized by "Z" so as itself to grow, to reproduce, to differentiate and function *adaptively*, no explanation at all is given, despite a verbose smoke screen which amounts only to an assertion that such *must be* the result: the mechanism is apparently self-evident (to him that hath the wish). Nor is there the slightest shred of evidence that the proposed "Zoe" or "Z" combination could either come into existence as postulated, or have any of the effects attributed to it. The author, however, naively maintains that she has built her theory from the ground up and that it is only in this way that she has arrived at the soul. "Surely," she protests in advance, giving way to a defense reaction, "no man of science would cite the general belief of humanity in the duality of man based on religious beliefs, mysticism, dreams of a wandering double, etc., as negativing my claim to originality for my conception of the organism as a dual system; since my concept is gained exclusively from the facts of the pertinent physical sciences, and the appeal is only to these facts." The emotional background of her rationalizations is clearly revealed in such passages as the following:

> Human motherhood thus acquires a new and peculiar dignity: with the beginning of the soul dating from the same moment as the beginning of the body of her unborn child, indeed holy, and akin to the brooding of Tetragrammaton [explained in glossary to mean Jehovah] over a formless world, are the woman's long days of her *enciente* waiting.

A large part of the book is devoted to a mere rehearsal by the author of what she knows of atomic physics. This may impress the uninitiated, but it is no more than what one can get in any ordinary popular account, and it has no bearing on her "Zoe" phantasy, except insofar as it really emphasizes the total absence of any evidence for the latter. When it comes to the biological side, she is not so systematic in her rehearsal of the already known, and she takes more liberties with actually existing theories. Thus, for example, she argues that egg and sperm are opposite ions which attract one another; that mental precocity testifies to inferior intelligence; that cancer is the result of the birth of a new "Zoe," caused by high ion concentrations; that the erect carriage of man is explained directly by his originally higher "Zoe"; that the successive geological revolutions were times of the spontaneous generation of progressively higher and higher forms of life; and that species are fixed in that they do not evolve one into another.

Evolution Without a Tree

Each species began with its own kind of "Zoe," that was brought into existence by appropriate external conditions; the latter were progressively more complicated with the increasing age of the earth, and hence resulted in "Zoes" of greater and greater potentialities. Each given "Zoe," after being born, went on its predetermined orthogenetic way, through millions of years, to become a now stable species, incapable of further evolution. Whereas Austin Clark has proposed a parallel generation and evolution of different phyla and other "major"

*Let it not be supposed that the recently reported discovery of "neutrons"—nuclei composed of equal numbers of protons and electrons—constitutes a vindication of "Zoe." But, if the report is true, the above claims could soon be put to the test. It is to be expected that such bodies could not be formed by mere crowding of ions, and that they would be inert chemically.

groups, we see that Mrs. Gaskell has outdone him, in postulating a similar independence for every species. True, she admits that by X-rays and other artifices species may become radically changed, but she denies that such events could happen in nature. In support of her disbelief in the geneticist's conception of evolution, she quotes liberally from Bateson—*e. g.*, "As to almost all the essential features, whether of cause or mode, by which specific diversity has become what we perceive it to be, we have to confess an ignorance nearly total" (Bateson, *Problems of Genetics*)—and from Osborne—*e. g.*, "The old paths of research have led nowhere" . . . we must come "away from the matter and form conceptions" of heredity and evolution, and to "an energy conception." . . . "We may imagine that the energy which lies in the life-germ of heredity is very great per unit of mass of the matter which it contains" (Osborn, *The Origin and Evolution of Life*). At this point the reader may find it of interest to refer back to our opening paragraph, there to read the quotation from Pearl, commenting favorably upon Mrs. Gaskell's contribution to the evolution theory.

The Crucial Question

The self-published little book by Krafft, which has no authoritative introduction, and was, he tells us, refused publication by reputable journals, approaches the scientific much more nearly than does that of Gaskell. If it is the product of a psychosis, then the psychosis is that of a "mechanist" rather than that of an animist. Yet in essentials it resembles the book of Gaskell's, for it too must be regarded as a phantasy, in which, at bottom, desire replaces reason.

Krafft's introductory formulation of the problem of life reveals considerable analytical power, and deserves quotation because of its pertinence and soundness:

The phenomena of growth and reproduction . . . establish in nature a sharp line of demarcation between living and non-living things. Although growth and reproduction, in their broadest aspect, amount to nothing more than self-duplication, yet they are fundamentally different from any of the other processes heretofore known to science.

Biological growth does not proceed in the same manner as the growth of crystals because extensive chemical transformations take place during the process of metabolism whereas the growth of crystals involves no permanent chemical change whatever. It also differs from crystal growth in that the assimilation of food by the cells of living organisms does not require saturated solutions such as are necessary for the growth of crystals . . .

The ultimate cause of life, whether it be a certain substance or a specific detail of physical structure, must be the same for all living organisms because the specific differences which distinguish one living organism from another disappear as we go down the scale of plant or animal life . . .

That the fundamental life processes must be due, either wholly or partly, to specific chemical structures is generally admitted, but there is a prevailing opinion that the chemical configurations which are necessary for this purpose must be extremely complex. The failure of all previous efforts to devise some type of chemical structure which is capable of duplicating itself does not prove, however, that the solution of the problem must lie in the direction of extreme complexity. The complex chemical structures which make up the tissues of the higher plants and animals have developed gradually during the course of evolution, and the fact that they are necessary for the proper physiological functioning of the particular organisms in which they now occur does not prove that they were also the original cause of the incipient life processes in the most primitive submicroscopic forms of life from which these higher plants and animals have developed.

A New Twist

Alas that the promise of this really good introduction remains unfulfilled! What is the simple chemical configuration which the author offers us as giving the clue to the unique property of self-duplication? It is the "spirazine." To manufacture a spirazine artificially (on paper), all you do is to take an ordinary polypeptid molecule (a chain of amino-acids) and twist it into a regular cylindrical spiral, so that the semi-alternate N's of the chain will be opposite one another, on successive rungs of the spiral, and can thus form cross-con-

nections with one another, and so likewise (on occasion) can the semi-alternate CO groups. In this way the spiral becomes tightly bound together across its rungs, like a helical spring that has had two straight strings glued along its outside, crossways of the rungs and parallel with its long axis.

There is nothing inherently improbable in the existence of such twisting and cross-connections; in fact, as Krafft points out, it would be one way of accounting for certain break down products of proteins (the diketopiperazines). But it remains nothing more than one possibility among many. Krafft thinks he is driven to it as the only kind of conformation which will enable a polypeptid (1) to add on new amino-acids without disturbing the arrangement of the old ("growth"), and (2) to become divided up without change of pattern ("division"). As with Mrs. Gaskell, so with Krafft, it is at this crucial point, of growth, that the argument is hastily slurred over, and regarded as self-evident and compelling. Here lies the core of the delusion. This spiralling and these cross-connections—which are the only "contributions" of Kraft—are not only not made necessary by known facts of chemical structure, but, even if they existed, they would not help us in the least to explain growth and reproduction. If a spirally shaped chain can add to itself, there is no apparent reason why a straight one could not do so equally well. But in truth we know at present of no mechanism whereby one can do so, and the "secret" of growth may for all we know lie in some quite different mode of organization than that of polypeptids. The problem of organic growth remains as unsolved as ever.

The hardest jump being thus made by Krafft, it is for him, as for Gaskell, a relatively easy matter to continue the hurdle until every important problem of life—protoplasmic structure, cell division, heredity, consciousness—has been "solved" in similar fashion and at any rate consistently with his original basis. And, as with Gaskell, the further he proceeds the greater become the divergences of his hypotheses from the body of well-grounded theory. Thus, as to heredity, he says:

> When such a change [in a distinguishing characteristic of a species] does take place it is always irreversible and not governed by Mendel's laws, from which it appears that the corresponding alterations in the spirazine pattern of the germ cells must also be irreversible. Such alterations may consist of changes in the total number of spirazines or in their connections with one another through the alpha carbon atoms. On the other hand there are certain minor characteristics, such as the colors and textures of the various body-tissues, which may change freely from generation to generation in accordance with Mendel's laws without involving any permanent change in the nature of the species. Such minor changes take place with equal facility in either direction, from which it appears that the corresponding variations in the spirazine pattern must be completely reversible and must be confined to local changes only, such as the coupling or uncoupling of the adjacent spirazines through their carbonyl and amino groups, so as to produce that peculiar allelomorphism which is characteristic of Mendelian heredity but without producing any permanent change in the spirazine pattern as a whole.

For one at all familiar with genetic principles, such a passage stands self-condemned.

For the securing of energy by the organism, Krafft says the spirazines may "behave like Maxwell's demons" and so defeat the second law of thermodynamics. Unfortunately, organisms have not been observed to exhibit the spontaneous drop in temperature here required, any more than they have been observed to exhibit the spontaneous rise in temperature, unaccounted for by chemical reactions, that would be required by Gaskell's intra-atomic alterations.

Free Will

Turning now to the field of consciousness, we find that Krafft, despite

his mechanism elsewhere, has left a place for "free will." For he says:

> Through the medium of living matter . . . the behaviors of the individual atoms can manifest themselves. In chemically organized structures of this sort some of the atoms may occupy positions of advantage so that their specific individual behaviors may become amplified sufficiently to effect the excitation of nerve or protoplasmic fibres . . . The behaviors of the individual atoms, however, are not subject to the principles of statistical mechanics . . . The responsiveness of living matter to the activities of individual atoms can be readily demonstrated by certain optical experiments . . . .

There is no denying the truth of this proposition, *insofar as it goes*, and, in formulating it, Krafft has in essentials anticipated the very argument for "free will" which has recently been given by Arthur Compton in *Science*.*

The trouble with the above argument is that, like the arguments for the "spirazine" and "Zoe," it stops short at the crucial point. For, though the behavior of atoms or electrons is not determined by the laws of statistical mechanics, neither is it known to be "free" in the sense required by the "free will" advocates, if indeed the conception of the latter admits formulation at all. An event must be either determinate, and hence ultimately predictable (from knowledge of *all* prior conditions), or else indeterminate, *i. e.* capricious, haphazard, disorderly, "senseless." The so-called "self-determined" must logically fall in the one class or the other, and, in so far as "reason" enters as a determining (*sic*!) factor, the "self-determined" must certainly be in the first class (the determinate). Why, except through a confusion of ideas, should the second type of behavior (the capricious) seem more desirable to us than the first (the orderly)? And why, except in order to fulfill such an unreasonable wish, should we believe that the second type of behavior (the capricious) really exists? "Accidental events" do certainly exist, in the sense of events determined by causes too numerous or variable for us to compass, or by hidden causes, acting independently of those factors which we can at the time sense, and so giving results that collectively obey the rules of the mathematics of probability. But these events nevertheless fall in the last analysis into the first class, the determinate. And so great has been the success of science, thus far, in ultimately tracing down determinate sequences in "accidental" behavior that when, as in the case of the electron behavior described by Heisenberg's "principle of uncertainty," we find a new case for application of the mathematics of probability, this gives us no right to assume that we are dealing with a case of absolute indetermination, rather than of "caused accident," as defined above.

Certainly, in the case of electron behavior, it has never been shown that there are not other factors than the velocity, position and direction of motion of the electron, and the magnitude, position and direction of the incident quantum, which might help to determine the character of the reaction between them, nor that these other factors may not some time be discovered, perhaps by a more detailed analysis of the very phenomena in question. So, as a purely theoretical example, the direction of recoil, etc., of the electron from a first incident quantum might be found of aid in the prediction of its direction of recoil from a second incident quantum, owing to some peculiar individual conformation or motion (such, for instance, as spinning) that it might possess, but at present factors like the latter are hidden from us because they are independent of the factors of velocity and of original translational direction with which we have been dealing in experiments. I do not wish to be guilty myself, in this hypothetical example, of setting up a phan-

*Compton, A. H. 1931. The Uncertainty Principle and Free Will, *Science*, 74: 172.

tasy as fact: I use it only to show that the opposite conclusion, that of the essential indeterminateness of electron behavior, is as yet far from proved, and that therefore, at this stage of the investigation, the various edifices in the realm of biology, psychology, philosophy and sociology, that are already being constructed thereupon, are the real phantasies. Let us beware lest we be led unwittingly along such lines as those of Krafft and Gaskell!*

Dreams

It may seem that we have devoted too much space to two books like these under review, and indeed the reading of one or two of them is enough for a lifetime. They are of some value, however, as object lessons of what science is not. It is evident that any one of a hundred, or of a hundred thousand, "principles" like that of the "Zoe" or the "spirazine," derived from known or unknown realms of atomic physics, chemistry, theology, or mathematics, if handled in similar fashion, would have served just as well to "explain" the secrets of life and of the universe as would the formulae that these individuals' particular complexes happened to lead them to choose. The method of their application to the problems at issue is essentially that of the subconscious mind.

One of the most eminent biologists of the world has informed me that, on the night before his oral examination for the Ph. D. degree, he dreamt he was defending his dissertation in a spacious hall before an audience which included Darwin and Huxley. After an impressive prelude by the orchestra, he arose and announced: "Ladies and gentlemen, I do not wish to appear immodest, yet it is necessary for me to explain to you that I have really discovered something very profound, in fact it is none other than the secret of life itself. It proves to be startlingly simple and clear, as you will admit on seeing my demonstration." Turning to the blackboard, he drew two triangles, one, labeled ABC, in red chalk, and the other, its mirror-image CBA, in green chalk. "And this, ladies and gentlemen, as you can readily see, is the secret of life." They saw, and all became clear to them; there followed an outbreak of uproarious applause, a triumphant burst from the orchestra, the drop of the curtain, and an anti-climactic awakening. We submit that this mathematical solution deserves a place along with those from atomic physics and from chemistry here being considered.

Speculations

The objection is not that the works under review are speculations, unaccompanied by experiment, but that they are bad speculations, in which wishes first pass for possibilities, then possibilities for probabilities, and finally probabilities for necessities. They contain no search for alternatives, no weighing of chances, but merely flow like fiction. It is such works as these that have done incalculable harm to biology, including the science of heredity, by tending to carry all speculation, *including good speculation,* down into disrepute along with themselves. For another chapter should be written to show into what grievous errors and what shameful delay our science has often been led through the dearth of well-balanced speculation, of harnessed imagination, that recognizes itself for what it is. Whereas good speculation represents one of the highest and most coordinated types of activity of the mind, bad speculation represents one of the least coordinated, worthy only

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*Since the above was submitted, similar criticisms of the "free will" interpretation of Heisenberg's principle have been published by Bertrand Russell in "The Scientific Outlook" (1931, Norton and Co.); by William A. Noyes. (1931, "The Uncertainty Principle," *Science,* 74:595-596); and by Henry Morgenau (1931, "The Uncertainty Principle and Free Will," *Science,* 74: 596).

of the subconscious. But at the present time the discrimination between good and bad speculation on the part of the scientific public, has fallen to such a level that it can almost be said that the worse the speculation, the better its reception, as illustrated by comparison of the receptions accorded the works of Gaskell and of Krafft, respectively. At any rate, the more speculation sticks to the known, the more may it be condemned by the specialist in its field, even if it be good, while the *entirely groundless* emendations of the emotionalist may gain the greater approval by virtue of their very defects.

There should of course be a large place in our lives not only for the most rigorous type of ratiocination, but also for more or less unreigned and wholly unfeigned flights of fancy, both for purposes of recreation and stimulation and also to make us aware of distant possibilities and purposes, and of general types of situations which we have not yet met in actuality but for which we should, nevertheless, be not entirely unprepared. This function, however, is not filled by tiresome treatises that parade as actual science. It is filled rather by such frank and full-fledged phantasies, unfortunately all too rare, as are given us in the early stories of H. G. Wells, or in the contemporaneous ones of John Taine.

H. J. MULLER.

The "Tasting" Tests

THE samples of paper treated with Phenyl - thio - carbamide, which were sent out with the 1932 bills, produced a remarkable response in family pedigrees of taste reaction, and in requests for additional paper. For a time we were rather swamped by the requests, but ample supplies of treated paper are now available, and any member of the A. G. A. who wishes to make additional tests or to demonstrate the reaction to classes, clubs, etc., can obtain the treated paper from the office of the Association.

We also wish to thank those who have returned family pedigrees. It is hardly possible to acknowledge all of these individually. A large amount of data are being accumulated which throw additional light on the inheritance of taste differences. It is clear that in most cases "tasting" is dominant to "non-tasting," but there are enough inconformities to suggest that modifying factors sometimes inhibit the "taster" gene. This is indicated by the fact that occasionally non-tasters, or very slight tasters, do have children who find the substance definitely bitter.

Much more data are promised by a number of classes in genetics and eugenics, in which extensive tests are being made. When more of this material is available a report will be published in the JOURNAL. In the meantime we have in early prospect an article by Dr. A. F. Blakeslee and Dr. A. L. Fox on the interesting subject of the chemistry and genetics of these heretofore unrecognized sense differences.

There is no question that as an introduction to the study of heredity, or as an interest-arousing preliminary to a lecture on the subject, this reaction has great possibilities. In any considerable group both tasters and nontasters are sure to be found. After the arguments between them have subsided the difference serves as a pointed text on the subject of Mendelian segregation. In addition to this the substance has other possibilities—we hesitate to call them advantages—as is disclosed by the following letter which resulted from the distribution of the test paper, and which of itself justifies all the difficulties of its preparation. We regret that our correspondent should hide his light of authorship under the bushel of an incognito:

Oconomowoc, Wis.,
January 31, 1932.

American Genetic Association,
Washington, D. C.

GENTLEMEN:

Say, what the hell you think you're doin' with this here taste stunt? You are breaking

up families & ruining homes & making bad feelings. You are wrecking families what has lived together fer nigh onto 30 years without nothing more serious happenin' than a few fights over goin' to a show & a few dishes broken once in a while in a argument about the wife's relations or cigar ashes on the rug. Yes, & a broken chair once.

When I got yer envelope with the taste stunt I cut off some of thet trick paper and chewed it and it tasted just like ordinary paper to me. And then I called the wife and give her a good chunk of it and she no moren got it in her mouth than she made a face like the minister the time we had him to Sunday dinner and he put vinegar on his puddin instead of syrup. An she spit it out and she was hoppin mad. "Did you eat any of it yeself?" she says and I says "Sure, it tastes all right to me." That made her madder yet and she wouldn't talk to me the rest of the afternoon. So you see what yer tastin done fer me.

And that aint all. Then at supper she comes out high and mighty and says, "If you aint a better taster than that an can't even taste that bitter paper than I been wasting good victuals on you fer 30 years. And I ain't going to waste more of em on you either." You can believe it or not, my meals aint been the same ever since. An I aint been able to change her mind.

So you see that yer trick paper done fer me, me whose always et well for the past 30 years. Its a crime to send thet stuff all over the country & to ruin homes. I bet you ain't took none home to try on yer own wife either. If you have I hope you have caught yours.

You have ruined my life.

Truly,

S. HELP MEGOD.

Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

THE FANTASTIC CLAN: THE CACTUS FAMILY, *by* JOHN J. THORNBER and FRANCES BONKER. Pp. XIV + 134. Price $3.50. The Macmillan Co., New York, 1932.

For those who have been bitten by the desert's charm and prickd by its countless thorny plants, this book has many a thrill.

THE PROBLEM OF GENIUS, *by* WILHELM LANGE-EICHBAUM, *translated by* EDITH and CEDAR PAUL. Pp. XIX + 187. Price $3.00. New York Macmillan Co., 1932.

Is genius a biological phenomenon or sociological?—a matter of genes or a pyramiding of hero worship to the N*th*? Lande-Eichbaum believes that the geniuses of popular imagination are talented individuals, yes, but super-individuals—a figment of the collective imagination! While this may in some instances be admitted, nevertheless it is true that the range of talent is great—and that an arbitrary classification as "Genius" or "talent" is purely a verbal abstraction.

THE CULTURE OF THE ORANGE AND ALLIED FRUITS, *by* H. CLARK POWELL, Prof. of Horticulture, University of Pretoria, South Africa. Pp. 355, 83 illustrations, two maps. Price one pound, one shilling net, South Africa Central News Agency, Ltd., 1930. Johannesburg.

A very extensive handbook on citrus culture in South Africa especially, and in other citrus growing regions in general.

NEW MINDS: NEW MEN? The Emergence of the Soviet Citizen, *by* THOMAS WOODY. Pp. IVI + 528, many illustrations. Price $4.00. Macmillan Co., New York, 1932.

Mr. Woody summarizes twenty months of observation of the Soviet Union with a question-mark in the title and this opening text:

"Do you remember, I asked you then, 'will there be men among us?' And you answered 'There will be.' Oh, Black-earth force! And so now, from here, from my 'beautiful distance' once more I ask you: Well now, Uvar Ivanovitch, 'will there be?'

"Uvar Ivanovitch wriggled his fingers and fixed his enigmatic eyes upon the distance."

More enigmas out of Russia!

PARTIAL TWINNING IN A HEREFORD CALF

Figure 10

Twinning tendencies, expressed at various stages of embryonic development too late to result in separate twins, produce double monsters, ranging in degrees of completeness from "Siamese Twins" to individuals with double heads, or only with double faces. This case is of interest because the monster is not completely symmetrical, as is usually the case. It has two heads, necks, and tails, but only six feet. This partial twin calf was born recently on the ranch of D. F. Urstbel, near Canadian, Texas. Mr. Fred L. Frass, ranch manager, kindly furnished the photograph. The skin has been stuffed and Mr. Frass is willing to dispose of it to an institution interested in the purchase and preservation of such a specimen.

MENTAL DEFICIENCY IN GREAT BRITAIN

Report of the Mental Deficiency Committee, being a Joint Committee of the Board of Education and the Board of Control, London, H. M. Stationery Office. 1929.

THE extensive report of the Mental Deficiency Committee contains a wealth of material which will be of interest to all students of eugenics. Some day the people of Great Britain may be stirred to do something really effective toward reducing their burden of defective humanity, but at present they seem to be doing everything they can to increase it. The English people, like those of other countries, are not sufficiently aware of the evils which result from their apparently increasing population of subnormals. In general the intellectual classes seem to be even more ignorant of biology and its bearings on social problems than those of our own country. Nevertheless, Great Britain is in advance of most countries of Europe except perhaps Norway, Sweden and Germany. Eugenically France is hopeless and Italy is even worse. As to other countries there is little to modify one's conviction that so far as eugenics is concerned we are living in a benighted world. I have recently been informed that even in England some very simple talks on heredity over the radio evoked a considerable amount of protest not only from scandalized individuals but from a number of organized groups. This information afforded me quite a shock and naturally called to mind the opposition to the teaching of evolution in the backward sections of our own country.

The report we have mentioned is a valuable educational document which it is hoped, will have its influence in enlightening the public. Of course very few people will read it, but it will have to be considered by the important few who are entrusted with the responsibility of dealing with the defective elements of the population. It aims first of all to give the number of mental defectives in certain fairly typical areas of Great Britain. For their investigations the Committee selected six areas, each containing about 100,000 inhabitants. Three of these were urban, two rural and one partly urban and partly rural. Through the cooperation of local officials, social workers, physicians and school authorities a large amount of information was collected in regard to all sorts of defectives, whether feebleminded, epileptic or insane, in the regions investigated. An intensive study was made of the schools and of various institutions in which defectives were confined and no child was rated as a mental defective unless he was so regarded by the head teacher, and was also so classed on the basis of group tests and on independent examination by the medical investigator.

The whole report consists of four parts published in three volumes. The first part deals with the general plan of the investigation, the nature of mental defect, and a review of legislation on mental defectives. The second part on the Mentally Defective Child is concerned with present provisions for mentally defective children, the general findings of the Committee, the relation between defective and retarded children, and suggested administrative and educational reforms in the care and training of defective children. Out of a population of 623,000 in the areas studied the total number of defectives of all ages was 5,334, or 8.56 per 1,000. The total percentage of mental defectives was found to be higher in rural than in urban areas in the ratio of 10.49 in the former to 6.71 in the latter. If the percentage of defectives in the whole country were the same as in these areas there would be in England and Wales 288,600 de-

fectives of which 202,600 would be urban and 86,000 rural. Making allowance for the inclusion of some defectives whose condition should be attributed to faulty education and also for the exclusion of a certain number of adult defectives not certified as such, the Committee concludes that mental defectives constitute approximately 8.00 per 1,000 of the total population. This proportion is about twice as high as that found by the Royal Commission on Feeble-Mindedness whose report was published in 1908.

Much of the increase in defectives indicated by these two reports is attributed to a more complete ascertainment of defectives in the later survey and to saving more lives during infancy and childhood in the defective classes. It is of interest, however, that the percentage of low grade defectives, idiots and imbeciles is also about twice as high as in 1908. Inasmuch as it is not probable that many of this low group escaped detection, this increase affords a certain presumption in favor of the conclusion that the increase of the higher forms of feeble-mindedness may be, at least in part, real. It was also found that the disparity in the proportions of defectives in urban and in rural areas had also increased. Although the Committee is very careful in drawing conclusions, the statement is made after discussing various other explanations of its findings that "after due allowance has been made for these and other considerations to which great weight must be attached, the facts that (1) our investigation revealed twice as many lower grade defectives as did that of the Royal Commission twenty years previously, (2) that the ratio of the different grades to each other remains the same in the two inquiries, and (3) that the disparity in the ascertained incidence in the urban and rural areas has markedly increased — all these make it hard to believe that there has not been some increase in the incidence of mental deficiency during this period."

The high percentage of defectives in rural areas cannot be explained, in the opinion of the Committee, as a result of more complete enumerations since the disparity was most pronounced in the school population which was studied with especial care. It is noteworthy that the lower grades of defect were 40 per cent more frequent in rural areas. The explanation of rural defectives most favored by the Committee is that "the selective migrataion of the better stock from the rural districts has left behind a population inferior in mental quality at any rate and that the intermarriage of this inferior stock has produced a larger number of mental defectives during the last 50 years than in previous centuries when the population was more evenly distributed." This appears to be a very probable explanation and certain studies indicate that it applies to a number of cases of rural degeneracy in the United States.

In Chapter V of part III there is a discussion of Mental Defect as a Genetic and Social Problem. Mental defect, which is conceded to be hereditary in a large proportion of cases, is a "group problem." While some defectives are found in the best families, the great majority arise from inferior stock. A collection of families including mental defectives "would include as everyone who has extensive practical experience of social service would readily admit, a much larger proportion of insane persons, epileptics, paupers, criminals (especially recidivists) unemployables, habitual slum dwellers, prostitutes, inebriates and other social inefficients than would a group of families not containing mental defectives. The overwhelming majority of the families thus collected will belong to that section of the community, which we propose to term the social problem, or subnormal group. This group comprises approximately the lowest 10

per cent in the social scale of most communities." Many of these people may be emotionally deficient or unstable instead of feeble-minded and hence become social problems.

The problem of how to reduce this burden inevitably forced itself upon the attention of the Committee and it is discussed in the usual careful and conservative way. Sterilization, even if it were thoroughly applied, would not, it is claimed, effect much reduction. In general the Committee regards it with disfavor, although its attitude is that of open-mindedness instead of the more usual one of dogmatic opposition. One objection is that if parents were aware that their children might possibly be sterilized they would be less likely to allow them to be taken under public control and "the certifying Officer would be more reluctant to issue his certificate and the Magistrate would be less willing to make the Order." As a matter of fact the process has not worked out in this way in the United States. Parents who recognize the mental defect of their children are often desirous of having them sterilized so that they cannot transmit their defect, and with more enlightenment on the subject such an attitude would probably become more common.

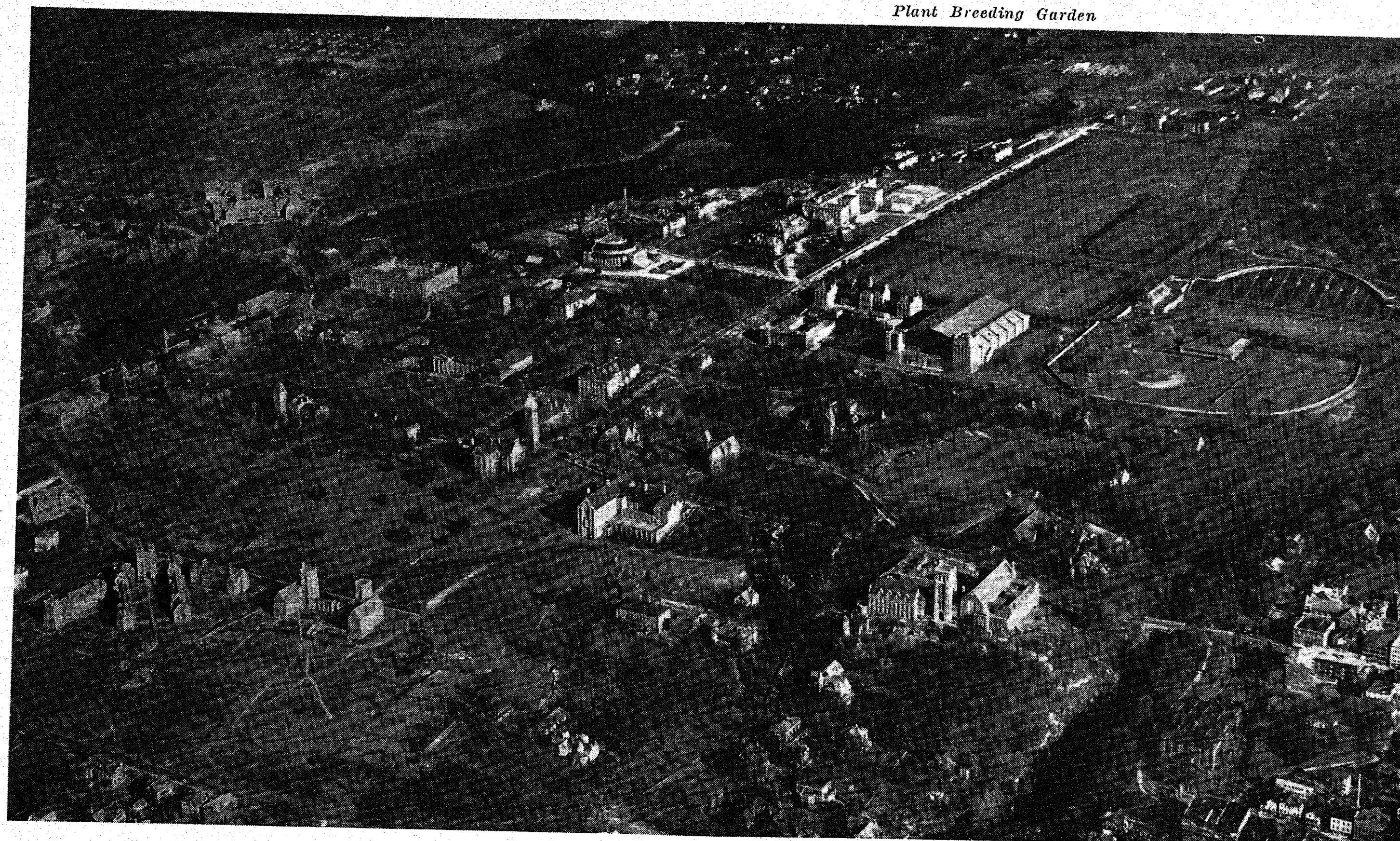
The Committee states that "A more cogent ground for advocating sterilization is that its application would ease the economic burden by enabling defectives who would otherwise have to be permanently segregated in institutions to return to the community with no risk of their becoming parents, and that a number of them could live happily and harmoniously outside institutions. If it could be proved that sterilization could safely and profitably be applied even to certain groups or categories of defectives, the question of its adoption would no doubt deserve careful attention."

The references to the effect of sterilization in California indicate that the Committee had not availed itself of the recent studies of Popenoe on the effects of the operations in that state or it could not have referred to the system of discharge after sterilization as a "failure." On the contrary the results of sterilization combined with supervision go a long way toward justifying this procedure according to the admission of the Committee.

The fourth and largest section of the report is that of Dr. E. O. Lewis whose investigations with those of his co-workers furnished much of the material employed in the preceding sections. This accounts for quite a good deal of repetition found in different parts of the report. It would be impossible within a short space to give an account of the detailed information given in Dr. Lewis' report. Physical defects including undersize, malnutrition, rickets, defects of sight and hearing, epilepsy and other nervous disorders were very much more prevalent among mental defectives than among normal children. As might be expected a close relationship was found between mental defect and illegitimacy. In the Eastern Counties area the 196 mentally defective women had given birth to 118 illegitimate children.

The study of family histories was a more or less incidental feature of the investigation, nevertheless considerable information was gathered which showed a marked tendency for several mental defectives to occur in the same family. This tendency was more pronounced among the ordinary cases of feeble-mindedness than in cases of idiocy and imbecility which seem to be more sporadic in their distribution.

S. J. Holmes.



Photograph by C. S. Robinson

SCENE OF SIXTH INTERNATIONAL GENETICS CONGRESS

Figure 10

Airplane view of Cornell University campus and environs, where will be held the Sixth International Genetics Congress from August 24 to 31, 1932. Willard Straight Hall, the headquarters of the Congress, is in the center of the picture. The live plant exhibits will be grown in the Plant Breeding Garden, which can be seen at the upper right, in the low ground beyond the University buildings. Already there are over five hundred members in the Congress. A fine program and remarkable exhibits are assured.

The Sixth International Congress of Genetics

Ithaca, New York, August 24-31, 1932

BECAUSE of the world-wide economic depression, the question of postponing the Congress forced itself on the Council as an unavoidable issue requiring immediate decision. After the most careful study of the present situation and the extremely uncertain limits of its duration, the Council unanimously decided to proceed with plans for holding the Congress on the dates originally planned, August 24-31, 1932.

Since, however, there had been uncertainty in the minds of some scientists as to whether the Congress would be held, and since this uncertainty might have led a number of potential members to delay in sending in their dues, the Council decided to take special action on memberships as follows:

It was voted to extend the date up to which members from the United States and Canada could join at the lower rate of $10.00 for full membership and $6.00 for student membership to May 1, 1932. It was further voted that the dues for these two classes of members after that date should be $12.00 and $7.00 respectively. It will be recalled that foreign memberships remain at $10.00 up to date of the Congress. The present quota of members of all classes is over five hundred.

Transportation

The Red Star Line finds it necessary to substitute the S. S. Westernland for the S. S. Pennland which we wished to use as the official steamer of the Congress. Since these two steamers are sister ships and practically identical, the change is of no importance. It is merely a change in name, and nothing else. The date for arrival in New York City is now set definitely as Sunday, August 21st. This will necessitate a change in the plans for New York entertainment, which will be announced later. It will not affect the offer of Columbia University and the Carnegie Endownment to entertain the foreign delegates during their stay in New York City.

In addition to a post-Congress excursion to New England and possibly one to Washington, D. C., (Congress Quarterly, Vol. I, No. 4, p. 3), the following trip has been suggested by Professor Huskins of the Department of Botany of McGill University, Montreal, Canada. A trip from Ithaca to Buffalo, Niagara Falls, Toronto, thence by steamer through the Thousand Islands to Prescott, and by rail again to Ottawa, Lucerne-in-Quebec, Montreal, Quebec, and New York, will cost about $45. McGill University offers hospitality for a couple of days for those who are interested in this trip.

Program

The program committee is progressing in arranging its material. Its chairman estimates that over two hundred papers and demonstration papers will be presented. It is planned that a number of morning meetings will be held dealing with topics of general interest such as "The Relation of Cytology and Genetics," "Mutations," "Species Hybrids" and "Genetics and Evolution." There may be changes in, or additions to, these topics. The afternoons will largely be given over to special section meetings at which the more specialized topics will be discussed, probably according to the experimental material used.

These afternoon meetings will, in no way, be considered of less importance than the morning meetings. They represent the reports of advances on the various frontiers of genetic knowledge. The morning

papers are in the nature of general discourses on fields of research rather than on specific problems. Both types of contribution are absolutely essential to the success of the conference.

The knowledge that Dr. T. H. Morgan, President of the Congress, is recovering from a very serious accident met while automobiling, and that he will be able to participate in the Congress, has removed a great source of grave concern and worry from the minds of geneticists the world over.

Exhibits

Material is already being grown for a part of the exhibit of living material to illustrate many of the principles and advances in plant genetics. It is hoped that this exhibit may be one of the outstanding features of the Congress.

Progress is also being made in the exhibits of other fields of genetic research. It is confidently hoped and expected that the work of every genetics laboratory in the United States and Canada will be exhibited and that material will also be received from many European and foreign laboratories. The interest already shown and the material promised make certain an exhibition, original in plan, and unique in extent.

Lodging for Members at Ithaca

The Administration of Cornell University has quoted rates in residential halls for a period of 4-7 days, of $1.75 per day. Private rooming houses, adjoining the campus quote rates of from $1.00 to $1.50 per day, depending on the facilities offered and length of occupancy. It is planned to publish, during the early summer, a more detailed survey of facilities and rates for the information of members. The replies to this will serve as a guide in making final arrangements.

Membership

The dues for personal membership in the Congress for residents of the United States and Canada are $10.00 if paid before May 1, and $12.00 if paid after that time. The dues of members from other countries are $10.00, payable at the time of the Congress. Membership includes a copy of the Proceedings of the Congress. Other classes of membership are: Student Membership, $6 until May 1, $7.00 after May 1; Sustaining, $100; Institutional, $100; Patrons, $1,000. Application for membership may be forwarded to the Treasurer of the Congress, Robert Cook, 306 Victor Building, Washington, D. C. Additional information regarding the Congress may be obtained from the Secretary General, C. C. Little, Jackson Memorial Laboratory, Bar Harbor, Maine.

PERSONAL MEMBERSHIP APPLICATION FORM

C. C. Little, *Secretary,* Sixth International Genetics Congress, Jackson Memorial Laboratory, Bar Harbor, Maine.

Dear Sir: Please enroll me as a member of the Sixth International Genetics Congress. This gives me the right to attend all the meetings of the Congress, and to receive the *Proceedings* and other publications of the Congress.

- ☐ I enclose $5 as payment of half my dues, and will remit the balance ($5) at the time of the Congress.
- ☐ I enclose $10 in full payment of dues.

Name ....

Street and No. ....

City, State and Country ....

Checks should be made payable to the Sixth International Genetics Congress

Please forward Application with remittance to Robert Cook, Treas., 306 Victor Building, Washington, D. C.

The

Journal of Heredity

Vol. XXIII MARCH, 1932 No. 3

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Pareto's "Residues"

"Like the instinct to make combinations these emotions have always been as much a part of human nature as the organs of the body, and expressions of them are found in most remote epochs of history and pre-history. What have varied since have been simply the explanations—the derivations."

Hybrids

Fertile hybrids of wheat and rye.—Hybrids of peach and almond, of possible value as ornamentals.

Did Shakespeare Know of Plant Hybrids?

Is "the art that does mend nature—change it rather," the art of the plant hybridist?

Bud Mutation in The Apple

A discussion of the 168 bud variations known at the present time—some of them of great value.

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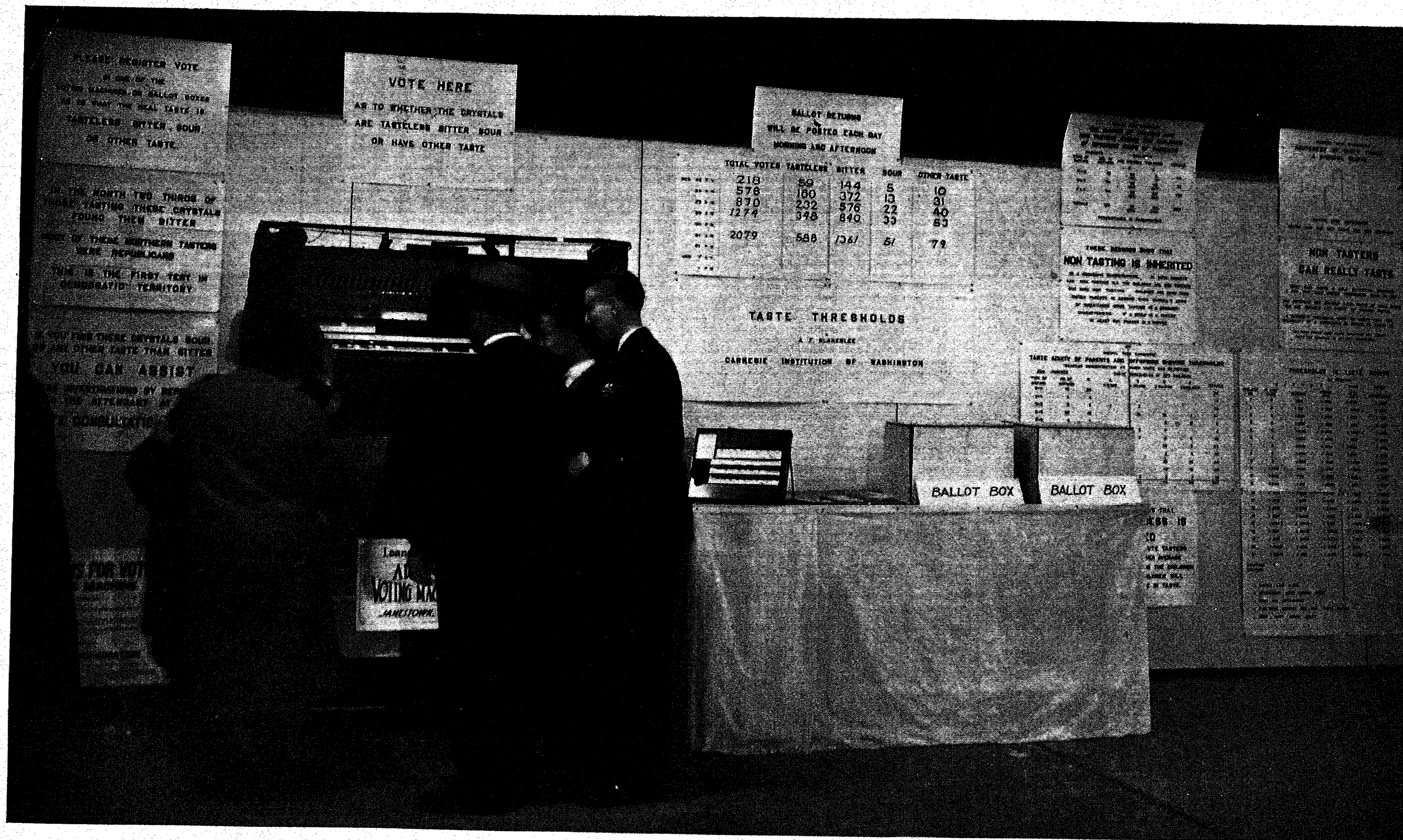


FOR ONCE EVERYBODY VOTED RIGHT!

Frontispiece

General view of the Taste Exhibit at New Orleans at the time of the meeting of the A. A. A. S. in December, 1931. A total of 2,550 persons took part in this demonstration of taste differences and recorded on the voting machine their reactions to P. T. C. Each of us probably has a different and unique combination of sensory reactions and hence each lives in a somewhat different world from his fellows. Realization that these differences exist and are inherited would have a profound effect upon our outlook on social and educational problems,

OUR DIFFERENT TASTE WORLDS

P. T. C. as a Demonstration of Genetic Differences in Taste

A. F. Blakeslee,
Carnegie Institution of Washington
and
A. L. Fox,
E. I. Du Pont de Nemours and Co.

THE present paper was originally planned to explain the use of a taste test as a class-room demonstration of the innate differences which exist among humans in their sensory reactions. The editor of the Journal of Heredity suggested that the article should be built around an illustrated account of the Taste Exhibit held at New Orleans in connection with the recent meeting of the American Association for the Advancement of Science. The suggestion of the editor will be followed and the exhibit will be allowed to explain itself largely through photographs.

Each visitor who started in to view the exhibit was first supplied with an envelope containing a gelatine capsule filled with phenyl-thio-carbamide and a peppermint "life-saver." The directions for use were printed on the envelope as shown in Figure 1. The long name, phenyl-thio-carbamide, we are shortening to the nickname *P.T.C.*, an abbreviation which we shall use throughout this paper.

The junior author's discovery that P.T.C. is tasteless to some and bitter to others was first reported in *Science News Letter*, April 18, 1931. Elsewhere[5] is given an account of the chemistry of related thio compounds of which P.T.C. may be considered as the type. This account is the basis for the part of the exhibit on the relation between chemical constitution and taste shown in Figure 4. The senior author's studies of the hereditary basis of taste reactions to P.T.C. have been reported in a publication with M. R. Salmon[4] and separately,[3] and are summarized in the placards and tables of the exhibit.

The first placard directly over the box containing the taste envelopes (Figure 2) raised the question: *WHAT TASTE WORLD DO YOU LIVE IN?* This question was answered by a placard (Figure 8) at the other end of the exhibit. "Try this harmless substance and learn whether you are a taster or a non-taster," was an invitation to the spectator to "know thyself," and an assurance that P.T.C. is a harmless chemical. Over 2,500 persons tested the substance at the exhibit without injury, although one young lady, who neglected to read the directions to take only a minute quantity, and who swallowed a large part of a capsule full of crystals was temporarily seasick. The two-year-old child of the junior author swallowed several of the filled capsules and was likewise temporarily incapacitated. The harmlessness of P.T.C., when taken in moderate quantities, is therefore well established. The placard "sample is sufficient to test self, family and friends" may have been responsible, in part, for the numerous reports sent in regarding tests made by visitors after their return home, although reports of such tests were not specifically requested. A teacher of physics tested 45 persons from the one capsule.

Visitors were requested to register their vote in the voting machine (Frontispiece) as to what the real taste is: *Tasteless, Bitter, Sour,* or *Some Other Taste.* These four choices were available in labelled levers on the voting

machine which was kindly loaned to the exhibit by the Automatic Voting Machine Corporation of Jamestown, N. Y.

In order to draw a comparison between voting at elections and registering taste sensations in a voting machine the following placard was prepared:

> ***In the North Two-Thirds***
> ***of Those Tasting These Crystals***
> ***Found Them Bitter***
> ***Most of These Northern Tasters***
> ***Were Republicans***
> ***This Is the First Test***
> ***In Democratic Territory***

Most visitors realized that no one else could tell them how to vote regarding the taste of P.T.C.—that matters of personal sensations could not be decided by a majority vote. There was general agreement therefore with the remark that this was the only balloting ever held in which every one of the several thousand voters could be sure he voted right. A few, however, failed to realize that taste is a subjective matter and said: "The stuff tastes bitter to me. What ought it to taste like?"

At the end of the five half days during which the exhibit was open, the final records on the voting machine were as follows:

Totals	Tasteless	Bitter	Sour	Other taste
2550	715	1670	59	106
	(28.0)	(65.5)	(2.3)	(4.2)

Percentages are given in parentheses.

Who is "Abnormal"?

To those who find P.T.C. strongly bitter, it seems almost incredible that any one could call it tasteless. One visitor flatly refused to believe the recorded votes for tasteless. He was asked to watch the voting. A man who was voting tasteless kindly consented to demonstrate his ability to eat a large part of a capsule of the substance with perfect composure. The skeptical visitor couldn't understand it. "There must be something wrong with the man; he is abnormal" was the only

ARE YOU A TASTER?

This envelope contains a gelatine capsule filled with
PHENYL THIO CARBAMIDE
a harmless chemical discovered by Dr. A. L. Fox, of the duPont Co., to be tasteless to some and bitter to others. A few report other tastes.

Kindly try the crystals (only a minute quantity is necessary) and register your

VOTE as to WHAT THE REAL TASTE IS,
in
VOTING MACHINE IN EXHIBITION ROOM

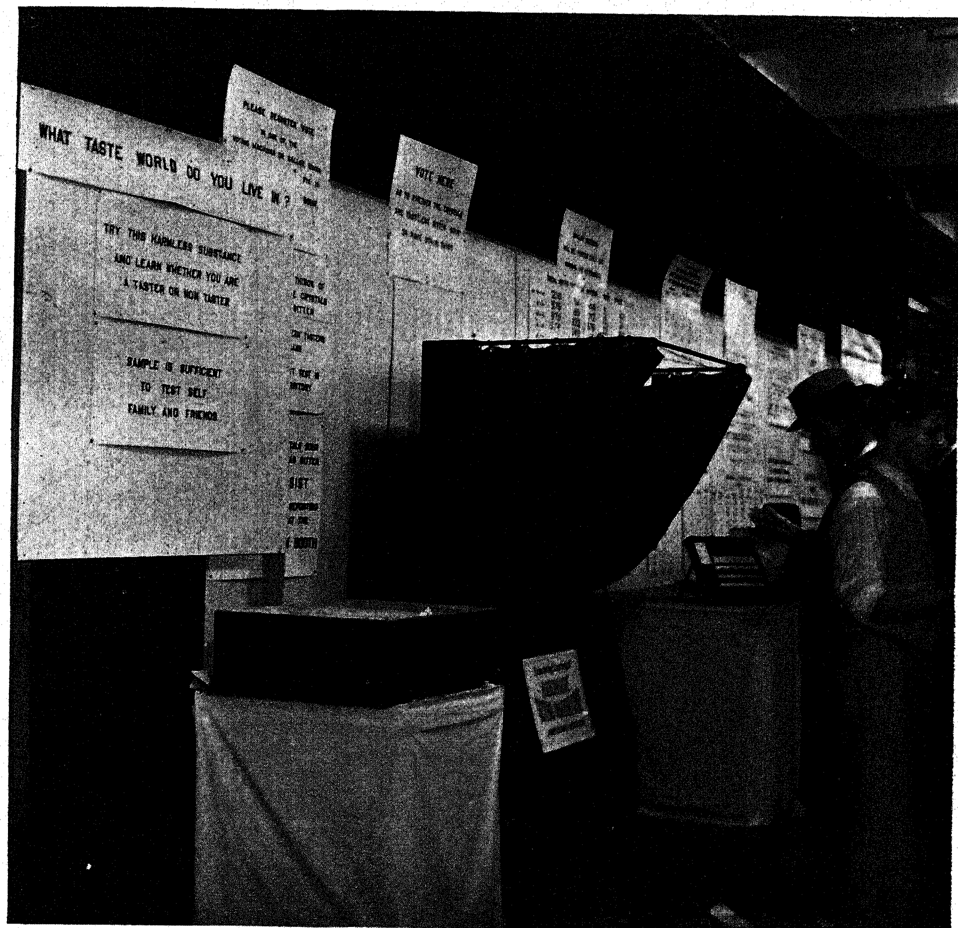
at Exhibit on Taste and its Inheritance. Don't eat the peppermint Life Saver first. It will take away the taste if you find the crystals bitter.

If you are a taster and get any other sensation than bitter, you are AN EXCEPTIONAL PERSON OF MUCH INTEREST TO SCIENCE. It would be appreciated if you would report any unusual taste reactions to the attendant at the Taste Exhibit, or to A. F. Blakeslee, of the Carnegie Institution of Washington, who is making a study of sensory thresholds in taste and smell.

THE "TASTER" ENVELOPE
Figure 1

This and the following illustrations show various features of the Taste Exhibit at New Orleans. This envelope was given out to all who wished to test themselves for their taste reaction to P. T. C.

explanation that seemed possible. Our skeptical friend had blue eyes but it did no good to suggest that blue-eyed people could as well be called abnormal since lack of brown pigment in the eyes and lack of ability to taste P.T.C. seem to be inherited in much the same manner, "No siree, my eyes are all right even if they are blue, but anybody who can't taste this bitter stuff certainly is abnormal" and he left the exhibit with an expression of deep pity evident for 28 per cent of those who had voted. Those who found the crystals tasteless were naturally at a loss to understand why anyone should make a wry face and call them bitter. One husband, who was a non-taster, was



"KNOW THYSELF"
Figure 2

In the first poster of the exhibit an invitation was extended to try a self experiment in chemistry, physiology, and heredity. The results were recorded on the voting machine in the center of the picture. As is so often the case with those who make this test, the "tasters" and the "non-tasters" frequently found it difficult to believe that those who disagreed with them were actually telling the truth. A wide dissemination of this test might increase the realization that those who fail to agree with us may be as honest and faithful to the truth as ourselves, but that the picture their senses bring them may be as different from those that we perceive as black is from white.

overheard trying to induce his wife to taste more of the substance. "There's nothing to it, Mary. It hasn't any taste at all," he emphatically explained but the wife, apparently, was an acute taster and preferred seeming to make a fuss over nothing to tasting again something she didn't like.

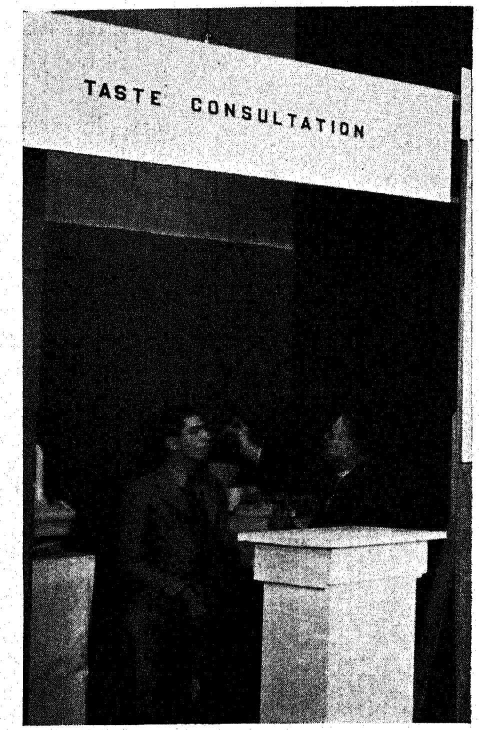
There are several factors which prevent the records on the voting machine from representing accurately the sensory reactions of those who tasted the chemical. Taste and, to an even greater extent, smell are subject to unconscious suggestion, such that one is more likely to report the sensation which others near by are known to be experiencing. The frequently expressed feeling on the part of the majority who are tasters that the non-tasters are in some way abnormal, may have prevented some of the latter from voting

"tasteless." It is clear, however, that between a quarter and a third of a rather large sample of people could detect no taste in a substance which to the rest of the population was bitter in varying degrees, or had some other taste.

Thresholds of Taste Acuity

What sour, or "other taste" really means could be determined only by a detailed individual test. Judging from threshold studies[3, 4] some of the reports of sour were due to faulty use of terms, while other cases were due to inability to discriminate between the sensations of bitter and sour. Considerable information of value was obtained through the cooperation of visitors to the exhibit, who responded to the request: *"If you find these crystals sour or any other taste than bitter, YOU CAN ASSIST these investigations by reporting to the attendant at the Taste Consultation Booth"* (Figure 3). In this booth were available a series of bottles filled with solutions of different test substances. P. T. C., quinine and picric acid represented bitters; hydrochloric acid represented a sour; saccharine a sweet and table salt a salty taste. The threshold, or concentration at which a person could first detect the substance, was roughly determined by administering the solutions in soda fountain straws. By this method rough thresholds for different substances were determined for a considerable number of the visitors by Mr. M. R. Salmon or by the senior author. Several new cases of inability to distinguish sour from bitter were discovered and the belief thereby strengthened that such indiscriminations are relatively common.

Weak powers of discrimination were not rare among the persons tested. Bitter and sour could be distinguished in such cases but apparently tasted much alike. Among the "other tastes" reported, some said the substance was sweet; some that it was salty; some that it tasted like bitter almonds; some like camphor and some like sulphur. The last three tastes are probably more properly to be classified as odors. One of the persons who reported P. T. C. as sweet was given a threshold test. He picked out solutions of P. T. C. and of saccharine as being both sweet and alike in taste without knowing what was being given to him. He had the usual reactions of bitter from quinine and sour from hydrochloric acid. A number of reports of sweet from P. T. C. have come to us since the exhibit. Salty is another taste sometimes attributed to P. T. C. and, in one case tested, the subject picked out a solution of sodium chloride as tasting like the crystals of P. T. C.



DETERMINING THRESHOLDS
Figure 3

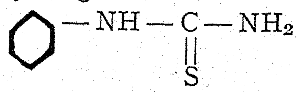
The only exact way to measure sense differences is to abandon verbal description and test with solutions of which the investigator knows the strength and constitution but not the "subject." Sense thresholds for P. T. C. show a wider range than for many other substances, but variations in taste acuity were observed with all the substances tested.

With the information learned from the exhibit it would have been better

to have given an opportunity of voting for all four of the primary tastes: bitter, sour, sweet and salty. It should be emphasized again that only careful and detailed tests can give accurate information regarding a person's thresholds or taste discriminations. People differ apparently in the intensity of sensation they experience which seems to have little relation to the thresholds at which they can first detect the substance. People also differ in their likes or dislikes toward the same substance. Thus peppermint "life-savers" had been found one of the best means of taking away any unpleasant taste from acute tasters of P. T. C. and were, therefore, supplied in all the test envelopes. Among the thousands who took the test, however, there were found a few who distinctly disliked the taste of peppermint. Such rough tests and observations which have been discussed may be of considerable educational value in showing that innate and hereditary differences actually do exist in the sensory reactions of different people.

Chemistry of P. T. C.

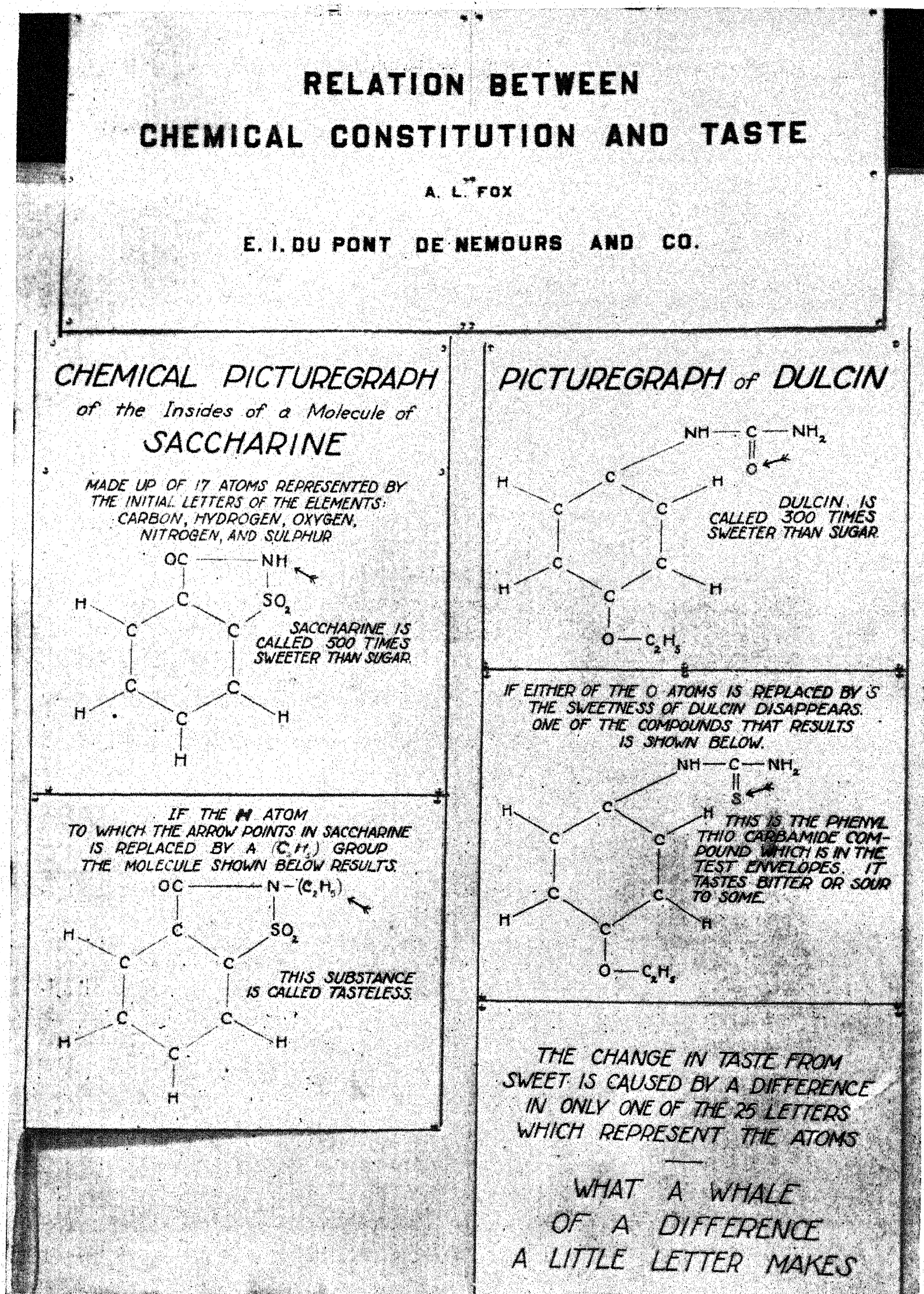
Before discussing the genetics of taste acuity, it may be well to learn something about the chemistry of the substance which has such different tastes to different people. In Figure 4 are given some illustrations of the relationship between chemical constitution and taste. The junior author in the publication cited[5] has shown that the taste peculiarity of the phenyl-thio-carbamide compound is in the $C=S$ linkage group. A chemical "picture-graph" of the substance given out at the exhibit is shown at the lower right of Figure 4. In the exhibit we have used the term phenyl-thio-carbamide in a generic sense to include related compounds with similar taste properties. The substance given out at the exhibit was, strictly speaking, para-ethoxy-phenyl-thio-carbamide, as chemists would at once recognize from its structural formula. The substance used in the tests shown in the following charts is more properly called phenyl-thio-carbamide for which an abbreviated formula may be given as follows:—



The taste of the two substances is practically identical except that the latter P. T. C. is apparently slightly stronger. At least a few who were found to be weak tasters of the P. T. C. used in our genetic studies were unable to taste the longer named substance given out at the exhibit.

The charts in Figure 5 summarize the evidence for the inheritance of taste capacity for P. T. C. If neither of the parents can taste the substance, none of the children have been found to be tasters. If one parent is a taster and the other a non-taster, somewhat more than half of the children have been tasters; while if both parents are tasters somewhat less than a quarter of the children are non-tasters. Such proportions would be expected if the inability to taste P. T. C. were due to a single Mendelian recessive factor. The critical mating, for such an hypothesis, is that between two non-taster parents from which one would expect only non-tasting children. From such matings we have recorded on the chart a total of 22 children all of whom are, in fact, non-tasters. Snyder[6] working with the related para-ethoxy-phenyl-thio-carbamide reported 17 non-taster children from matings between negative parents. Several additional negative children might be included in this group from reports of those who attended the exhibit. The conclusion is justified that non-tasting is inherited but many hereditary factors are involved in taste, and apparent exceptions to above mode of inheritance may be expected, especially if crystals only are used in tests, as shown earlier.[3] Some taste dilutions, but not crystals.

For Figures 5 and 6, rough thresholds were found by giving graded solutions by means of soda fountain straws. It will be seen that the tasting abilities of the parents influence the tasting capacities of the children.



CHEMISTRY AND TASTE

Figure 4

This and the four following figures are reproductions of some of the posters shown in the general view of the exhibit (Frontispiece). They are self explanatory.

PHENYL THIO CARBAMIDE

THE NUMBER OF TASTERS AND NON TASTERS AMONG THE CHILDREN OF DIFFERENT KINDS OF PARENTS

PARENTS		CHILDREN		
TYPE OF MATING	NO. OF MATINGS	NON TASTERS O	TASTERS T	TOTALS
O×O	10	22 (100)	0 (0)	22
O×T	39	32 (43.2)	42 (56.7)	74
T×T	54	22 (16.8)	109 (83.2)	131
TOTALS	103	76 (33.5)	151 (66.5)	227

PERCENTAGES IN PARENTHESES

THESE RECORDS SHOW THAT

NON TASTING IS INHERITED

AS A RECESSIVE CHARACTERISTIC. IF BOTH PARENTS ARE NON TASTERS ALL THE CHILDREN ARE NON TASTERS, BUT SOME NON TASTING CHILDREN ARE BORN FROM TASTER PARENTS IN ACCORD WITH MENDEL'S LAWS OF INHERITANCE SINCE TASTING IS A DOMINANT CHARACTERISTIC. IF A CHILD IS A TASTER, AT LEAST ONE PARENT IS A TASTER.

PHENYL THIO CARBAMIDE

TASTE ACUITY OF PARENTS AND OFFSPRING SHOWING THRESHOLDS

(WEAKEST CONCENTRATION AT WHICH TASTE WAS DETECTED)

206 PARENTS		TASTE THRESHOLDS OF 227 CHILDREN					
TYPE OF MATING	NO. OF MATINGS	O	A 1:5,000	B 1:20,000	C 1:80,000	D 1:320,000	TOTALS
O×O	10	22					22
O×A	2	4					4
O×B	8	9	1	4			14
O×C	25	18		10	19	4	51
O×D	4	1			2	2	5
A×C	1			3	1		4
B×B	5	2	1	3	2	2	10
B×C	12	8			10	9	27
B×D	2	2			4	4	10
C×C	17	5			19	16	40
C×D	12	3			10	12	25
D×D	5	2		1	5	7	15
TOTALS	103	76	2	21	72	56	227

THESE RECORDS SHOW THAT

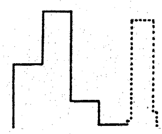
TASTE ACUTENESS IS INHERITED

IF THE PARENTS ARE ACUTE TASTERS THERE WILL BE A LARGER AVERAGE NUMBER OF TASTERS AMONG THE CHILDREN AND THESE TASTER CHILDREN WILL AVERAGE MORE ACUTE IN TASTE.

Figure 5

PHENYL THIO CARBAMIDE

ACUTENESS OF TASTE FORMS A BIMODAL CURVE



DOTTED LINE REPRESENTS
THOSE WHO DID NOT TASTE THE DRY CRYSTALS

NON TASTERS CAN REALLY TASTE

MOST CAN TASTE A COLD SATURATED SOLUTION.
A FEW WHO HAVE HIGHER THRESHOLDS NEED IT HOT

THE CRYSTALS ARE SOLUBLE 1:400 PARTS OF COLD WATER
BUT ARE SOLUBLE 1:17 PARTS OF BOILING WATER.

A PERSON'S THRESHOLD MUST BE LOWER THAN THE SOLUBILITY OF A SUBSTANCE IN ORDER TO TASTE IT

WHY SOME PEOPLE TASTE DILUTE SOLUTIONS BUT CANNOT TASTE THE CRYSTALS IS NOT YET UNDERSTOOD.

THRESHOLDS IN TASTE ACUITY

(21 PERSONS)

SUBJECT NO	BITTER: P THIO CARB	BITTER: QUININE	BITTER: PICRIC ACID	SOUR: H CL (38%)	SWEET: SACCHARINE	SALT: NA CL	ACIDITY OF SALIVA (pH)
1	0	1:5M	1:20M	1:800	1:20M	1:400	7.0
2	0	1:5M	1:20M	1:1,600 a	1:80M	1:400	6.8
3	0	1:20M	1:20M	1:800	1:20M	1:400	6.6
4	0	1 20M	1 20M	1 400	1 10M	1:400	7 1
5	0	1 20M	1 80M	1 1,600 a	1 20M	1:400	7.0
6	0	1 20M	1 80M	1 800	1 40M	1 400	6.8
7	0	1 20M	1 80M	1 800	1 40M	1 400	7 0
8	0	1 80M	1 80M	1 800	1 40M	1 400	6.8
9	0	1 80M	1 80M	1 1,600	1:40M	1 400	6.2
10	1:20M	1 80M	1 20M	1 1,600	1:40M	1:400	6 9
11	1:80M	1 5M	1 20M	1 400 b	1 10M	1 200 c	
12	1 80M	1 5M	1:320M	1:400	1:40M	1 200 e	6.8
13	1 80M	1:20M	1:80M	1 1,600	1:40M	1 200	7 0
14	1 80M	1 20M	1:80M	1 1,600	1:80M	1:400	6 7
15	1 80M	1 20M	1:320M	1 400	1:80M	1:200	6.9
16	1 80M	1 320M	1 80M	1 1,600	1:80M	1:800 e	7 1
17	1 80M d	1:320M d	1 80M	1 1,600 d	1:80M		
18	1 80M	1:320M	1 320M	1 1,600	1:40M	1:400	6.9
19	1:80M	1:320M	1 320M	1 1,600	1:80M	1:400	6.5
20	1:320M	1:20M	1:80M	1:800	1:40M	1:400	6 8
21	1:320M	1:20M	1:80M	1:1,600 b	1:40M	1:400	6 7
DILUTION FACTORS	4	4	4	2	2	2	

a TASTES LIKE ALUM
b. ASTRINGENT, NEXT GRADE SOUR
c. SOUR AND ASTRINGENT
d. P.TH.CARB., QUININE AND HCL TASTE ALIKE
e. SOUR, NEXT GRADE SALTY

Figure 6

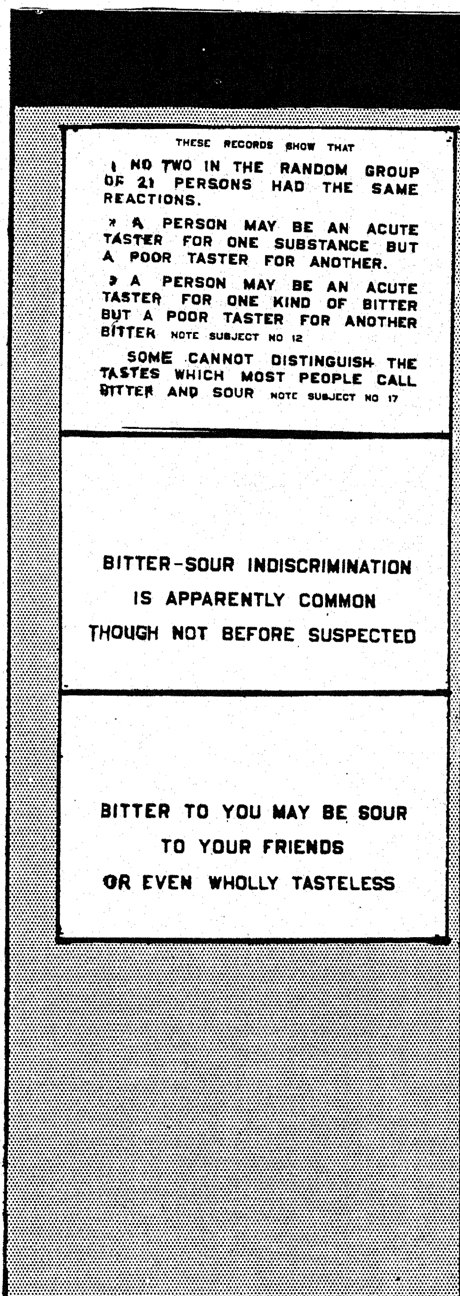


Figure 7

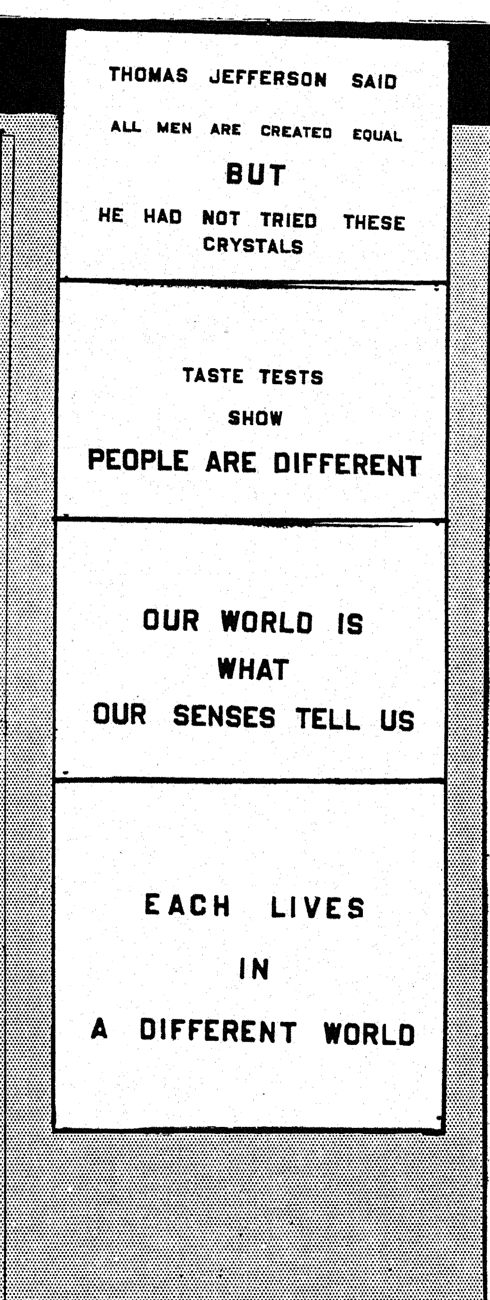


Figure 8

Non-tasters can really taste if the P. T. C. is only gotten into the mouth in a sufficiently concentrated condition, as in cold, or better in a hot saturated solution. Some who are non-tasters to a small amount of the dry substance, taste it if a large amount is taken. Taste acuteness, therefore, forms the bimodal curve shown in the upper part of Figure 6.

The table in Figure 6 gives rough thresholds of 21 subjects for several substances. The conclusions from the data in the table are given in the charts of Figure 7.

It may be remembered that the exhibit started with the question: *"What taste world do you live in?"* An answer and a conclusion is attempted in Figure 8. This figure forms the end of the exhibit which was an experiment in genetic "extension" and at the same time an attempt to obtain some better idea of the range of variation in respect to taste by a survey of a relatively large sample of people. For more detailed discussions of the topics, the reader is referred to publications already cited[3, 4, 5].

Other Substances Show Taste Differences

P. T. C. is not the only substance that is capable of dividing people into tasters and non-tasters, although it is the best so far known. Prof. A. W. Evans has kindly sent the senior author an herbarium specimen of fumar protocetraric acid which is a constituent of certain lichens, the bitter taste of which is supposed to be a criterion distinguishing them from closely related species. To Prof. Evans as well as to a number tested by the senior author the substance is entirely tasteless while to others it is bitter in varying degrees. Cetraric acid, a related compound, obtained from Eimer and Amend under the name cetrarin, was bitter in different degrees to almost all persons tested. Only one out of 17 found a considerable amount of the substance to be tasteless. Another substance is under investigation which is tasted by only a small minority. The junior author has found that crystals of benzoate of soda, as well as a 10 per cent solution, are tasteless to some but distinctly bitter to others. This is a compound which has been advertised as a tasteless substance The senior author tested 26 persons with a 1/10 per cent solution which is the concentration used commercially in the preservation of food. Nine persons, or a third of the group, found it to have a distinct taste which was generally described as sweet or bitter. As pointed out in Figures 6 and 7, these peculiarities in taste are largely dependent upon innate differences between people in their thresholds for the substance tested.

A similar peculiarity has been found to exist in regard to odors which are interpretable, in large part at least, by similar differences in thresholds. The peculiarity of floral odors of two particular plants of Verbena discovered by one of us[1] is of interest in that over 90 per cent of the 40 people tested failed to find fragrance in one or the other of the two flowers but not in both. Inability of some to detect odors in certain kinds of flowers are probably relatively common. The senior author recently found marked differences in a group of Barnard students in respect to their reaction to two varieties of Freesias. Some found them to be without odor, but to others they had different degrees of fragrance. To a considerable number the odor was distinctly unpleasant. Reactions to the two varieties were the same to some, but to others the two smelled entirely different. Other examples might be added, but these that have been given are sufficient to show that the taste reactions from P.T.C. are not peculiar to this substance alone and are paralleled by examples from the sense of smell.

The Worlds We Live In

The exhibit and the further examples emphasize the fact that people are innately different in their powers of sensory response. To the conclusion that each lives in a different world some would say "Yes, but what dif-

ference does it make?" A little consideration would sometimes lead them to realize that much of our educational system and of our other efforts at human betterment are based on the tacit assumption that people are essentially equal in their innate capacities. These matters have been discussed elsewhere.[2] It is our belief that a full realization of the extent of differences between individuals would revolutionize the philosophy of "the man in the street," and through his philosophy would also affect his laws, religion and other efforts at social advance. P.T.C. has been found to be a simple but the most effective method we know for demonstrating to students the fact of innate, but unsuspected, differences between people in physiological response. A number of teachers have requested material of P.T.C. for use with their classes which we have been glad to supply.

The editor of the JOURNAL OF HEREDITY, with material which we sent him, has devised a method of impregnating paper for test material which has certain obvious advantages over the loose crystals. The paper is soaked in a ½ per cent solution of P.T.C. in acetone and allowed to dry. A fairly constant concentration is thus secured in a given amount of the paper. Samples of P.T.C. treated paper have been sent out to many members of the American Genetic Association on a trial experiment and such paper is being supplied in the insert with the present publication and in the reprints of it. The Association is prepared to furnish P.T.C. paper in quantity for class use at a nominal charge. We bespeak success to the JOURNAL OF HEREDITY in this new effort at "Genetic Extension."

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1. BLAKESLEE, A. F. 1918. Unlike reactions of different individuals to fragrance in Verbena flowers. *Science* 48:298-299.

2. ——————. 1931. The genetic view-point. *Science* 73:571-577.

3. ——————. Genetics of sensory thresholds: taste for phenyl-thio-carbamide. *Proc. Not. Acad. Sci.* 18:120-130.

4. —————— and SALMON, M. R. 1931. Odor and taste blindness. *Eugenical News* 16:105-108.

5. FOX, A. L. 1931. The relationship between chemical constitution and taste. *Proc. Nat. Acad. Sci.* 18:115-120.

6. SNYDER, *L. H.* 1931. Inherited taste deficiency. *Science* 74:151-152.

The P. T. C. Taste Leaflet

A SAMPLE of P. T. C. obtained from Dr. Blakeslee, inspired the idea of using the material in a modified form to demonstrate to a larger group these interesting taste differences. In order to do this safely some modification of the technique used at New Orleans seemed necessary. The crystals had obvious disadvantages. A large dose, while apparently not injurious, might nevertheless cause much discomfort and possibly lead to complications. This suggested the idea of "diluting" the P. T. C. by impregnating paper with it. The first leaflets to be sent out were treated by swabbing the paper with a brush dipped in the dry crystals. The method was slow and somewhat erratic, and the finished product was not very uniform. A number of solvents were tried and all of them had disadvantages of one kind or another,—of price; of availability; of aftertaste; or of speed of evaporation. Acetone was finally adopted as most satisfactorily meeting all the requirements.

The arrangements of the blank chart have also gone through several revisions. In its present form it appears to be fairly satisfactory, as its import is generally understood, to judge from completed charts that are returned. By the use of dotted lines and symbols an attempt is made to convey the idea that the generations represented are at the tips of the branches of the "tree," or rather, of the "bush" of life.

It is believed that the combination of test paper and the blank chart has

considerable promise for "extension" purposes in human heredity. Those who make the tests and fill in the charts are actually engaging in a research in human genetics. Among our membership this is hardly an extraordinary innovation, but among the population at large this might be regarded as distinctly a step in a very promising direction. If it were possible to bridge the gap between this character, which has no particular "practical value," and the growing list of others, of the utmost importance to the individual or to society, in which the same principles of heredity are operative, the value of the test will be still further enhanced.

The completed charts that have been returned, contain much material on the inheritance of these taste differences. A number of very interesting points have been brought to light. These data will, it is hoped, be the basis of a later article. Some of the exceptions reported may be the starting-point for further research.

The request for additional copies of the charts have been almost overwhelming. Nearly five thousand have been sent out for use in classes and for demonstration before clubs, etc. No other demonstration of heredity, of which we are aware, has been so promptly and so enthusiastically adopted. —Editor

(Text of "Taste-test" Leaflet)

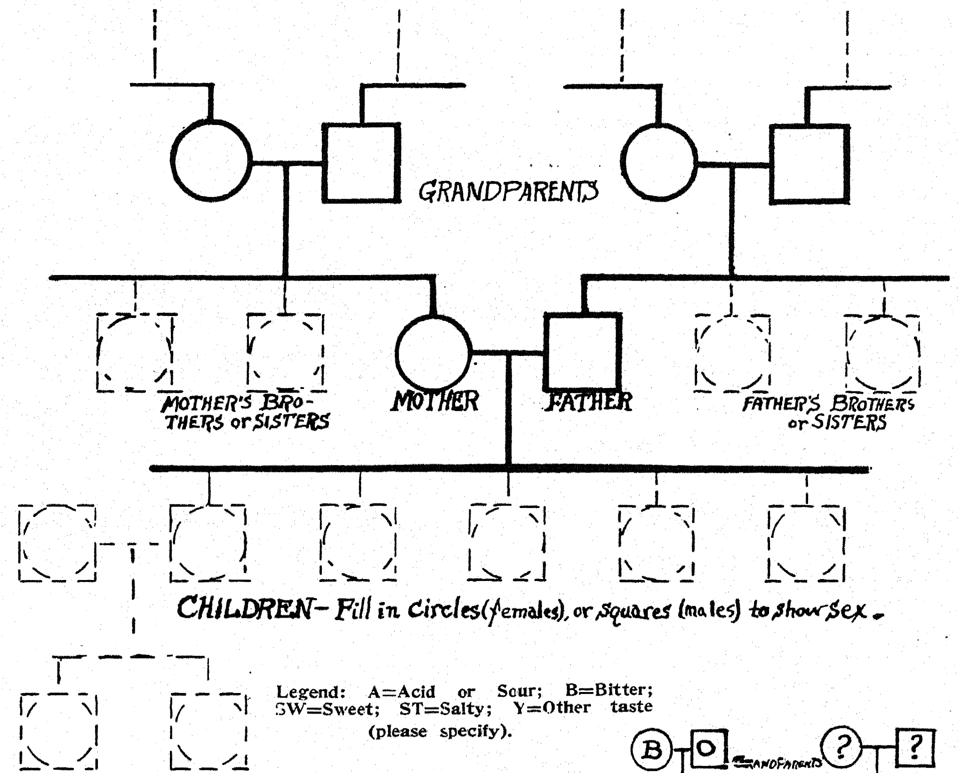
"TASTERS" AND "NON-TASTERS"—INHERITED DIFFERENCES IN TASTE REACTION

The attached paper is treated with P.T.C. (phenyl-thio-carbamide). This substance is peculiar in having a strong taste to some people and in being tasteless to others. The differences in reaction are inherited, inability to taste the substance apparently being inherited like a recessive. (A. F. Blakeslee and A. L. Fox, Journal of Heredity, March, 1932, and separate articles in *Proc. Nat. Acad. Sci.*, January, 1932.) Those who are interested in testing themselves or their friends for this reaction may do so by placing a bit of the treated paper on the tongue. The compound is harmless even in considerable quantities. Some "tasters" can detect the microscopic quantity in a few drops of a 1/500,000 solution, and even less sensitive tongues can detect the taste in a very small piece of the treated paper. Data on family reactions will be appreciated and the information may be entered on the form on the other side of this sheet. Additional treated paper can be secured from A. G. A.

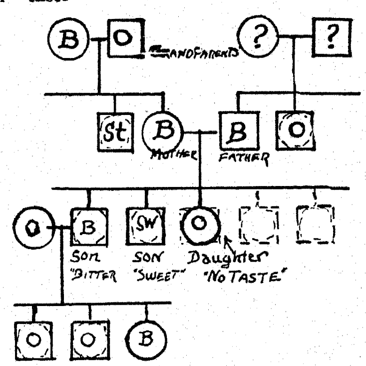
While a majority of "tasters" agree in the classification of the taste of P.T.C. it should be emphasized that exceptional reactions are not "abnormal" any more than the ability to taste or not to taste the substance could be classified as an abnormality. Thus the characterization of the taste as "sour," "salty," "sweet," etc., is of interest, as very little is known regarding the heredity of these less frequent reactions. It is best not to prejudice the "testee" by describing the flavor before a test is made, as the psychological effect of expecting a certain result tends to influence our conclusions.

Below is a chart in blank which may be filled out if it is possible to study the heredity of taste-differences in the reader's family. In this chart, O = no

The white paper under this cover is treated with P.T.C. (phenyl-thio-carbamide). On the average 7 people out of 10 on chewing up a bit of the treated paper will detect a definite taste. Others will taste nothing These peculiarities in taste discrimination are inherited.



Legend: A=Acid or Sour; B=Bitter; SW=Sweet; ST=Salty; Y=Other taste (please specify).



SAMPLE FILLED-IN CHART

Name of Person Making Test: ....

Address ....

....

Please indicate position on chart by an arrow.

Please return filled-in chart to:

AMERICAN GENETIC ASSOCIATION
306 Victor Building **Washington, D. C.**

A TEST CHART OF TASTE REACTIONS

Figure 9

In the above chart may be entered the taste-reactions to P. T. C. of the members of a family. A sample chart, filled out, is at the right as a guide to the method of recording the results. It is rarely possible to get the reaction of four generations, but when this can be done such a record is of especial interest. This kind of chart is used in recording the inheritance of other characteristics, such as hair or eye color, musical ability, etc. It is only necessary to adopt a different set of symbols to describe the characters being studied.

A single chart such as this may not by itself give definite evidence of the way a character is inherited. It serves, however, as a link in a chain of evidence available when a large number of such charts are brought together for study. Such a collection of inheritance charts is one of the most useful ways we have to study human heredity. The cooperation of many individuals in preparing and returning such charts makes possible real advances in this most important field of knowledge.

taste; A = Acid or sour; B = Bitter; SW = Sweet; ST = Salty; Y = other-taste. (Please specify.) *Any differences in classification of the taste of P.T.C. are of great scientific interest and should be especially noted.* In few instances can this chart be completed for more than two generations, and parent-offspring groups in most cases will be all that can conveniently be obtained. Information on any other relatives will be appreciated.

The chart when filled out may be returned so that the data may be placed on record for statistical purposes. Those who wish to keep this folder may sketch the taste-reaction chart on a separate piece of paper, or an additional copy can be obtained from A. G. A.

Books Received

BOOKS are acknowledged in this column as received, and such acknoweldgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

MAN AND ANIMAL, THEIR ESSENTIAL DIFFERENCES, *by* HERMANN POPPELBAUM, Ph.D. Pp. 174. Price, $2.00 Postpaid. Anthroposophical Publishing Co., London and New York, 1931.

Another of those bridges between the physical and measurable on the one hand; and the "astral", the mystical and mythical on the other.

PSYCHOSE EN CRIMINALITEIT BIJ TWEELINGEN, *by* AUGUSTE MARCEL LEGRAS. Pp. 105. Kemink en Zoon N. V., Utrecht. 1931.

Case histories of twenty-six pairs of identical twins afflicted with mental derangement and criminal tendencies. As a control an equal number of fraternal twins similarly afflicted. Very important, our limited knowledge of Dutch leads us to believe.

DIABETES, Its Control by the Individual and the State. *By* ELLIOT P. JOSLIN, M. D. Pp. 70. Price $1.00. Harvard University Press, Cambridge, 1931.

The treatment and prevention of an increasing affliction that was, before Banting and Best, an early death sentence; but since then only an aggravation to those who have the wits and the will to live. Causes discussed at length; important among them heredity.

AN INVESTIGATION INTO THE RELATION BETWEEN INTELLIGENCE AND INHERIANCE, *by* EVELYN M. LAWRENCE. B. Sc., Pr.D. Pp. 80. Price 8 Shillings and Sixpence. Cambridge University Press, 1931.

Until twins reared apart have been given much more intensive study, such investigations as this are necessarily inconclusive.

CANCER, WHAT EVERYONE SHOULD KNOW ABOUT IT. *By* JAMES A. TOBEY, DR. P. H. Pp. xxix + 313 + x. 17 Illustrations. Alfred A. Knopf, New York. 1932. Price $3.00.

A statement of the cancer problem as it affects the individual, emphasizing that cancer *in its early stages* is curable. Discounting of heredity factor perhaps unfortunate from the racial point of view.

THE UNIVERSITY SERIES, First and Second Units. The University Society, New York, 1932.

Illustrated paper-bound books giving authoritative information that he who runs may become informed on such matters as astronomy, physics, chemistry, anthropology, fossils, etc. "Heredity and Variation" by L. C. Dunn presents the genetic point of view.

STATISTICAL ANALYSIS OF AMERICAN DIVORCE, *by* ALFRED CAHEN, PH. D., Assistant Statistician on Consumption Habits for the President's Research Committee on Social Trends. Pp. 149. 18 Charts, 11 Tables. Price, $2.25. Columbia University Press, New York. 1932.

Post mortems of human happiness, and a biopsy of a grievous disease or symptom affecting the family — "the most important institution of human society."

AN INHERITED UDDER ABNORMALITY IN CATTLE*

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DURING a visit with a class in Dairy Cattle Judging to a farm near the University, the peculiar udder abnormality shown in Figure 10 was first noticed. The udder was normal but poorly shaped on the right side, but on the left only one teat was present. Little attention was given to the abnormality at that time as this cow was the only individual in the herd which carried the defect.

The following year, on a similar trip to a farm just across the road, two more cows were observed which carried the same abnormality as the cow observed the year previously.

This abnormality is very objectionable as it materially affects the usefulness of the cows carrying it. Such animals are very difficult to milk due to the presence of only three teats, and consequently the owners have used them for nursing calves in order to avoid the inconvenience necessitated by hand milking. The production is also materially lessened by this abnormality. The owners state that the production is about 30 per cent lower than in the normal individuals of the same family.

With the appearance of two more cows exactly like the first in udder conformation, it was evident that the abnormality was probably caused by a new inherited factor in cattle.

A careful examination of the udders of these individuals shows them to be of very poor shape with two quarters and teats on the right side and only one teat on the left side. The left half of the udder is nearly as large and of almost the same shape as the right half.

The study of the inheritance of this character is presented in Figure 11.

Discussion of Pedigree Chart

The first individual observed to carry this abnormality (Figure 10, and Figure 11, II-1) was a registered Gurnsey cow purchased by her present owner as a heifer calf at a Guernsey Sale. She is now 15 years of age and the dam of 12 offspring. Of these individuals eight were males and four were females. As far as can be ascertained, the ancestors of (II-1) were normal and all of her calves have been normal except the last two (III; 13 and 14). Her full sister (II-2) was also normal.

The first of the progeny of (II-1), a male, was used in the herd for several years. Even though he was mated with his own dam to produce (III-4) no abnormalities other than poorly shaped udders appeared in his offspring.

This bull (III-2) was mated with two cows (III-1 and 3) owned by brothers living on adjoining farms. The results of these matings were the individuals (IV-1 and 2) a male and a female. The cow (IV-1) had a very poorly shaped udder, which seems to be a characteristic of this family, but otherwise she was normal.

When (IV-1) was mated with (IV-2) the resulting individual was a female (V-1) carrying the abnormality exactly as it appeared in (II-1). This cow (V-1) produced 4 normal male calves which were sold for slaughter. Her dam (IV-1) produced in addition

* Prepared in the Department of Zoology at Ohio State University under the direction of Dr. L. H. Snyder. I also wish to acknowledge the kind cooperation of the breeders who have made this study possible.

ZELMA OF WHITE COTTAGE 79682

Figure 10

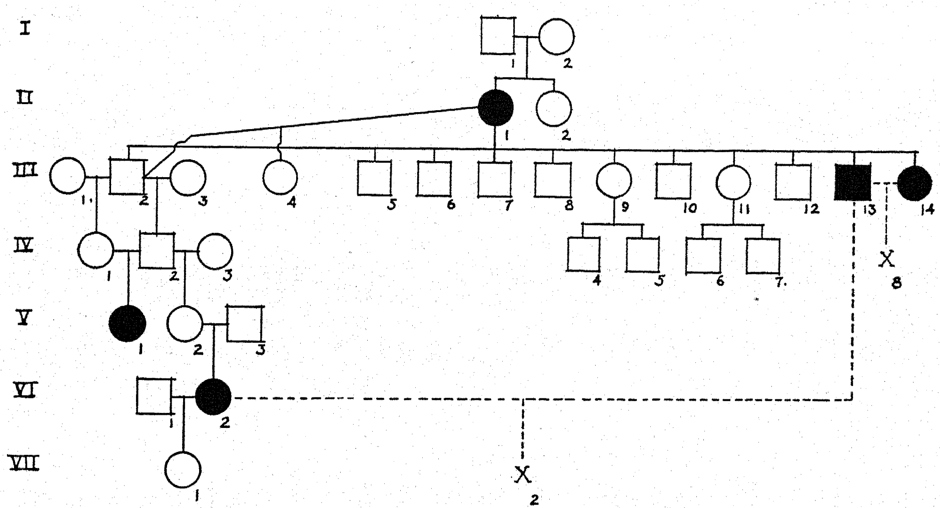
This cow, a registered Guernsey, is the foundress of the family shown by the chart in Figure 11. The udder is poorly formed, but the striking feature is the presence of only one teat on the left half. This peculiarity appeared in the next generation and in the third and fourth succeeding generations, being apparently inherited as a recessive. To our knowledge she is the first individual to carry this udder abnormality. The photographs illustrate the nature of the defect.

two male calves which were sold for slaughter.

The male (IV-2) was mated with a Jersey cow (IV-3) producing a normal female (V-2) with the exception of a poorly shaped udder. When this individual (V-2) was mated back to a Guernsey bull the result was the abnormal individual (VI-2) and Figure 12. Neither of her parents produced any other abnormal individuals.

This cow (VI-2) has produced two normal calves, one by an Ayrshire bull and the other (VII-1) by a Guernsey bull. The sire of (VII-1) was also the sire of (III-13 and 14).

Returning to the remaining progeny of the original cow (II-1), her daugh-



THE INHERITANCE OF AN UDDER ABNORMALITY IN CATTLE

Figure 11

Inheritance chart showing seven generations of a family of Guernsey cattle carrying a recessive factor for abnormal udders. Dotted lines indicate proposed matings, to test mendelian nature of character.

ter (III-4) sired by her son (III-2) was normal except for the very characteristic ill-shapen udder, which is common in this family. This cow (III-4) was the dam of four females and two males, all of which were normal. The males (III-5, 6, 7, 8, 10, 12) were used in grade and pure bred herds in the locality and as yet no abnormal individuals have been produced. So far as can be ascertained none of these bulls is in service at the present time.

The females (III-9 and 11) had the very poorly shaped udders characteristic of this family, but neither of them produced abnormal offspring. Each of these cows has produced two males (IV-4, 5 and 6, 7) which were slaughtered as calves.

The last of the produce of (II-1), (III-13 and 14) are full brother and sister sired by (VI-1). These individuals are both abnormal. The heifer (III-14) has exactly the same udder abnormality as her dam.

The full brother of (III-14), (III-13) also shows abnormal rudimentary development. The rudimentaries on the right side are quite pronounced while those on the left are hardly perceptible. He is in service in a grade Jersey herd and as yet has produced no abnormal calves.

In order to carry this study to completion, the owners of these abnormal individuals have consented to permit the matings indicated by the dotted lines in Figure 11. These matings should confirm the evidence which seems to show a simple recessive Mendelian factor, responsible for this defect. Theoretically, all individuals resulting from these matings (IV-8) and (VII-2) should carry the abnormality.

In addition to the family presented in this study, a similar abnormality has occurred in another local herd. In this herd a pure bred Guernsey heifer, produced by parents in no way closely related to the family discussd in this study, carries the abnormality with the teat placements reversed. She has two teats on the left side of her udder and only one on the right side. She does not possess the very poorly shaped udder common to the other family. It is

hoped that arrangements can be made whereby she may be mated with (III-13) to ascertain if her abnormality is caused by the same factor which is carried by the other family. There are no abnormal individuals in her pedigree as far as we can determine.

After the results of the proposed matings have been produced it is also planned to make a detailed anatomical study of the udders of these individuals.*

Summary and Conclusions

The data presented in this study indicate the appearance of a new inherited factor in cattle.

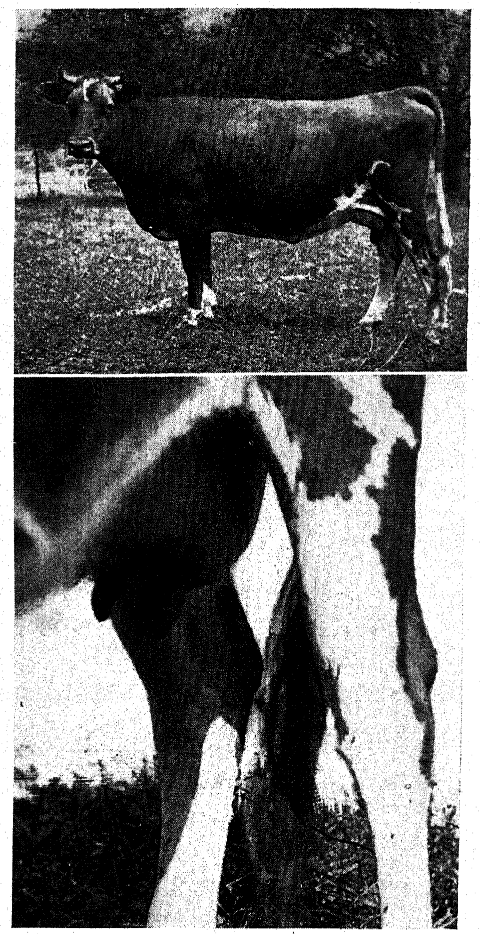
At the present time this abnormality has been recorded only in the Guernsey breed.

The evidence points toward simple Mendelian recessiveness in the inheritance of this abnormality as it skips one or more generations and may appear when both parents are normal. Proposed matings have been arranged in order to prove the other requirement for recessiveness; namely, that homozygosity is necessary for its appearance.

This factor is of great economic importance to breeders of dairy cattle, as, due to its recessive nature its presence may be disseminated throughout an entire herd or community before an abnormal individual appears.

The attention paid to rudimentaries in selection of bulls, especially in foreign countries, may be of more value than we have thought here in America.

It would seem that the best way to stamp out this undesirable factor would be for the breed associations to insert a clause in their registration requirements which would prevent registration of individuals, either male or female, which did not show normal udder or rudimentary development.



THE ABNORMALITY CROPS OUT AFTER FOUR GENERATIONS
Figure 12

Photographs of (VI-2) showing the abnormality four generations removed from the source.

* AUTHOR'S NOTE: Since this article was written, C. W. Turner (Anatomy of the Mammary Gland of Cattle. *Missouri Agricultural Experiment Station Research Bulletin*, 160, 1931) has reported a male bovine fetus of the Hereford breed which carried an abnormal rudimentary development similar to that reported in this paper.

A FERTILE TETRAPLOID TOMATO

Cross-Sterile With Diploid Species

E. W. Lindstrom
Iowa State College

TWO distinct forms of the tomato are accorded species rank by Bailey[1]. One is the common tomato, *Lycopersicum esculentum*, Mill. (*Solanum Lycopersicum*, Linn.), and the other is the currant type, *L. pimpinellifolium*, Dunal (*Solanum racemiflorum*, Vilm.). Both species have the same chromosome number (twelve pairs) and hybridization between the two is readily accomplished although a small degree of gametic sterility is plainly evident in the F_1 and F_2 generations.

Tetraploids from the *esculentum* species have been produced by several workers, but no one has heretofore reported a *pimpinellifolium* tetraploid. Because the latter is markedly different from the other tetraploids it merits some attention, particularly since it is the most fertile of all tomato tetraploids thus far recorded.

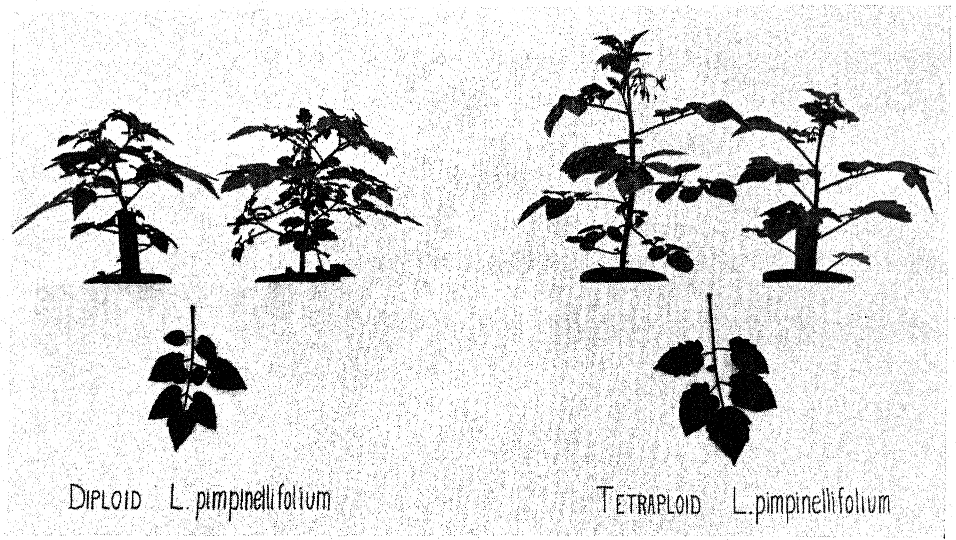
The least fertile tetraploid is the one originating from a doubled haploid *esculentum* form (Lindstrom[4] and Lindstrom and Koos[5]). In this the chromosomes in each set are identical. Although the reduction division seems to proceed with reasonable normality, giving daughter cells mostly with 24 chromosomes, the final seed development is usually less than ten per cent normal. The fruits are distinctly smaller than those of the diploid from which this tetraploid arose by the decapitation-callus method.

Tetraploids from ordinary diploid *esculentum* forms have been reported by Winkler[7], Jorgenson[2], Lesley and Lesley[3], and Sansome[6]. There seems to be considerable variability in the chromosome association and fertility of such tetraploids. None of the reports indicate a very high degree of fertility. A dozen tetraploids of this sort have been produced in our laboratory. These have all been derived from varietal F_1 crosses. Chromosome association in these is variable, there being both bivalent and tetravalent groupings. Seed development cannot be classed as more (and often less) than fifty per cent normal. The tetraploid fruits, although numerous, are somewhat smaller than those of the diploid from which these tetraploids were derived asexually by the decapitation-callus method.

Tetraploids from the *pimpinellifolium* species are here described for the first time. They were produced asexually by decapitating young, homozygous *pimpinellifolium* plants and allowing a callus to form on the cut stem. Nuclear or chromosome doubling took place in a few cells of the callus, from which adventitious tetraploid sprouts arose. Only three of the 100 tested sprouts proved to be tetraploid. Mosaic infection of the callus seriously interfered with the production of larger numbers. From one of the largest of these original tetraploids, three sexual generations have now been produced with no difficulty since there has always been a high degree of fertility. Preliminary cytological studies show chromosome association almost entirely in the bivalent condition, there being relatively few indications of tetrasomic grouping.

Description of *Pimpinellifolium* Tetraploid

From the very beginning these tetraploids were distinctly larger than the parental diploid. This was true for the seeds, cotyledons, seedling and mature leaves. The latter are distinctly thicker and firmer than those of the fine-leaved diploid. In Figure 13 are shown young plants of the diploid and tetraploid at the stage when the first flow-



DIPLOID AND TETRAPLOID FORMS

Figure 13

Young diploid and tetraploid (at right) plants of *L. pimpinellifolium*. The tetraploid was produced by a doubling of the chromosome number in callus tissue.

ers open. At this stage the axillary sprouts of the true diploid *pimpinellifolium* species are plainly evident, while those of the tetraploid are just commencing to develop. In *esculentum* plants of the same age and under the same conditions these axillary sprouts are still undeveloped. Because of this difference in development of the axillary branches, it is difficult to estimate the relative growth rate of the diploid and tetraploid. The latter seems to be a trifle slower in germination and in the very early seedling stage, but soon overcomes the diploid in plant height and length of leaves. The leaflets of the tetraploid are always larger than those of the diploid.

The mature tetraploid plant (Figure 14 left) is noticeably larger and more vigorous than the parental diploid. It carries much of the parental diffuse, viny and twiggy growth habit particularly when grown in the field. It is characteristically much less pubescent than the *esculentum* species. In fact, it greatly resembles the currant species in its lack of hairy pubescence, although the older stems usually possess some long hairs which are not found on the diploid.

The flower racemes are distinctly elongate and distichous, bearing 10-20 fruits or berries. The tetraploid flowers are almost twice as large as the diploid flowers, particularly in the length of sepals, petals and anthers (Figure 15). Pollen development is abundant and good, although some 10-20 per cent pollen abortion is evident. The normal pollen grains are uniformly larger than the diploid grains.

Fruit formation by self-fertilization is usually excellent, although there is some variability in this feature. Of 106 plants raised to maturity, only 10 were partially sterile (including two almost completely so) as shown by fewer and usually smaller fruits. Fruit size of the fertile tetraploids is on the average twice that of the parental diploid (Figure 15). In fact many of the tetraploid plants bear fruits almost as large as the well known Red Cherry variety of *L. esculentum*. Fruit measurements of twenty parental diploids and twenty tet-

raploids were obtained from ten fruits from each plant. These data appear in Table 1.

Seed formation in the tetraploids was surprisingly good on practically all plants with the exception of the 10 partially sterile types. The seeds were approximately twice as large as those of the *pimpinellifolium* parent. Practically no abortive seeds were noted in many plants. Abortive seeds, on the other hand, are very characteristic of the *esculentum* tetraploids.

Three generations have now been grown from self-fertilized *pimpinellifolium* tetraploids. The vegetative and floral phenotypic aspects have remained constant, the only real variation being that of fruit size. In other words this tetraploid breeds fairly true to type.

Hybridization of Tetraploid and Diploid

Fully sixty crosses between the *pimpinellifolium* tetraploid (as female) and diploid were made. Fruits set in practically all cases, but only very abortive, non-viable seeds were produced. The parthenocarpic development of these fruits was so astonishingly good, considering the complete absence of normal seeds, that it seems as if fertilization must have been effected but embryonic growth had ceased very early. A few reciprocal crosses were made, but no viable seeds were obtained. Accordingly, this tetraploid is completely sterile with its parental form. Several crosses of the *pimpinellifolium* tetraploid and various *esculentum* diploids as pollen parents were made, but not a single viable seed resulted. Such cross-sterility with the other common forms and species of the tomato, coupled with the great phenotypic deviation from type of the *pimpinellifolium* tetraploid, could very well assure this form the rank of a new species in a broad sense, although its chromosome constitution is in reality atypical as it is now constituted. In time this feature would probably be changed by mutations, translocations and inversions so that 24 different pairs of homologous chromosomes would be evolved, making it essentially a new species.

Detailed c o m p a r t i v e cytological studies of our various tetraploids are now under way. The *pimpinellifolium* tetraploid arose from a reasonably homozygous, inbred strain of the Red Currant variety. One would fully expect most, if not all, of the chromosomes to associate in the tetrasomic condition. This was true in the homozygous tetraploid *esculentum* form that originally came from a haploid. Lesley and Lesley[3] also described an *esculentum* tetraploid that arose in a nearly pure variety. Here the chromosome association was practically all in the tetrasomic condition. The apparent absence of tetrasomes at late prophase and metaphase in the *pimpinellifolium* tetraploid suggests an anomalous situation which the following observations may explain.

In the second and third generations of these tetraploids an unlooked for variation occurred. Whereas the original tetraploid was typically red-fruited, like the diploid parent, a few yellow-fruited tetraploids appeared in the two selfed generations from it.

The parental strain of the Red Currant variety was homozygous for red fruit color and had remained so for four generations. As a matter of fact there has never been a single, yellow-fruited *pimpinellifolium* plant in any of our cultures. Accordingly, the origin of the yellow color must be sought elsewhere.

TABLE 1.—Fruit measurements (average per plant) of twenty diploid and tetraploid *pimpinellifolium* plants.

	Polar diameter mm.	Equatorial diameter mm.	Number locules	Weight grams
Diploid	11.5	11.9	2	1.0±0.01
Tetraploid	14.3	15.6	2	2.1±0.07

TETRAPLOIDS OF TWO SPECIES

Figure 14

Tomato tetraploids—Left, of *L. pimpinellifolium*. Right, of *L. esculentum* (variety F_1 hybrid). The tetraploid form at the left is self-fertile, but it is sterile in crosses with the parent diploid form. In a broad sense it might then be considered a new species,—or at least the raw material for the eventual development of a new species.

The possibility exists of an outcross to a yellow *esculentum* plant in the generation immediately preceding the tetraploid. Unfortunately seed from the diploid sprouts of the individual plant decapitated to form the tetraploid was not saved so as to test this point directly.

There were no obvious indications of *esculentum* characters in the tetraploid and the two generations from it except perhaps for a flower and fruit size slightly greater than expected and for minor variations in plant height. It should be noted, however, that *pimpinellifolium-esculentum* hybrids show such a striking dominance of *pimpinellifolium* characters that an outcross might easily be overlooked even in the diploid condition. Nevertheless such hybrids exhibit a distinct, although sparse, pubescence (long hairs) on young foliage and stems. The tetraploid, however, was strikingly like its diploid ancestor in the *complete absence* of these long hairs on the younger parts, although on the older stems there was a greater development of this pubescence than is found in the pure *pimpinellifolium* species.

For these reasons it seems difficult to assume that there is *esculentum* blood in this tetraploid. Nevertheless the evidence of bivalent chromosome association, the yellow fruit color, the cross-sterility with both tomato species and the high degree of self-fertility all point to that condition. Tetraploids from *esculentum-pimpinellifolium* hybrids are now being developed to check this situation. They should behave very much like the tetraploid described in this paper.

Genetic Observations

The fruit color data from the self-fertilized tetraploid afford some evidence on the method of segregation in a tetraploid tomato. From two selfed tetraploids 101 red- and 5 yellow-fruited plants were obtained. This suggests that the genotype of the tetraploid was *RRrr*. If the four chromosomes involved assorted at random a 1 RR+4 Rr+1 rr proportion of diploid gametes would result, giving a 35:1 ratio of red to yellow if fertilization were at random also. This would give a theoretical expectancy of 103:3 for this particular case. A deviation of two is well within the limits of random sampling errors and indicates that chromosome pairing in the tomato tetraploid may occur wholly at random among the four chromosomes.

In an *esculentum* tetraploid a similar ratio was found. In this case the F_2 ratio was 26:1 (53 tall:2 dwarf). Combining the two sets of data a 22:1 ratio (154:7) is obtained. This is consistently less than the theoretical 35:1 ratio and might indicate that the eight *chromatids* of the tetraploid were distributed more or less at random.* Complete random assortment of eight strands would result in a 3 RR+8 Rr+3 rr proportion of gametes, giving approximately a 21:1 ratio of dominant to recessive phenotypes upon self-fertilization. If the *Rr* genes are far removed from the spindle fiber attachment such an assortment becomes possible. The tall-dwarf genes of the tomato are known to be at one end of the first chromosome.

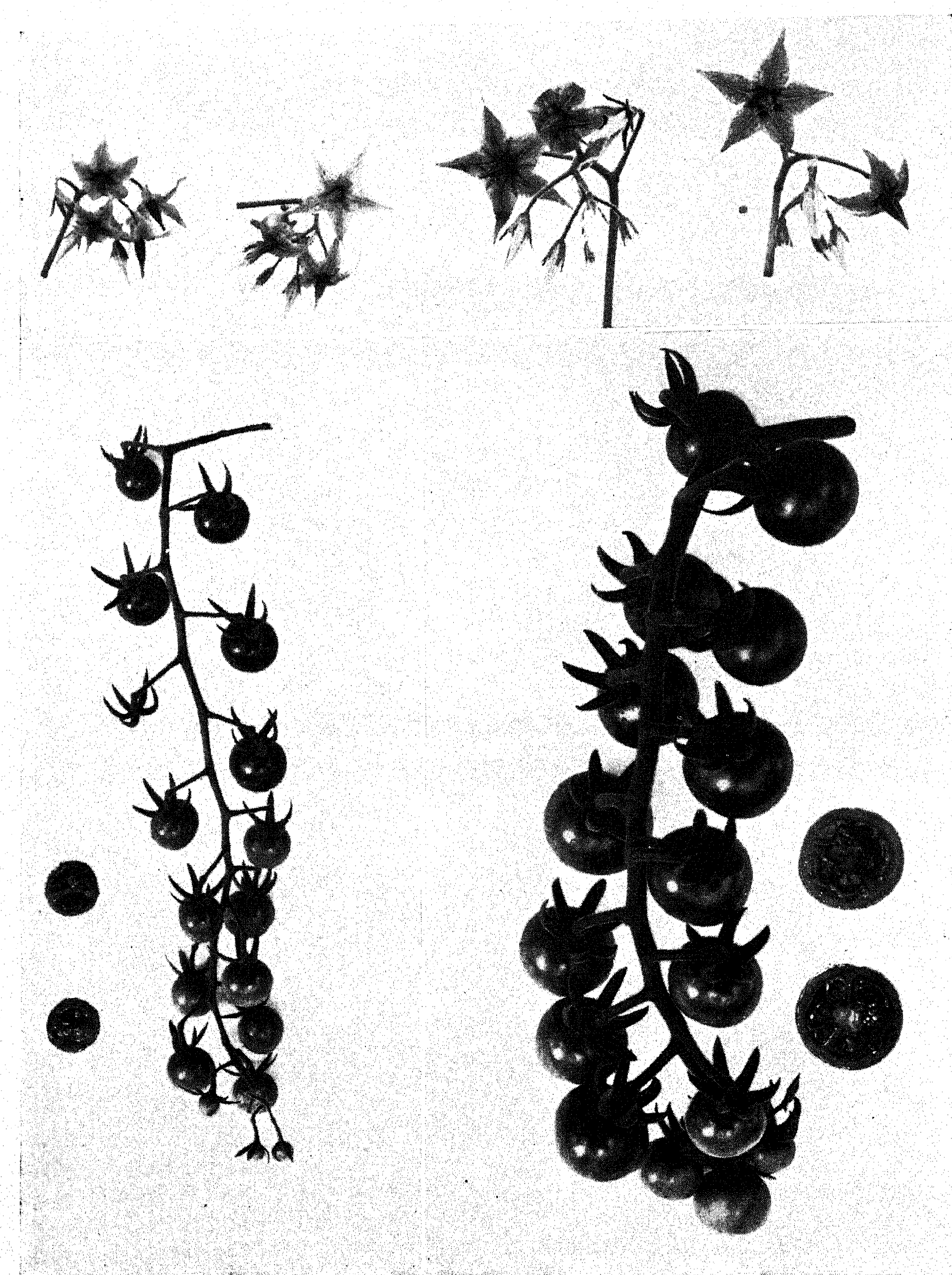
Summary

A tetraploid tomato, produced from the diploid *L. pimpinellifolium* (currant type) species, is described for the first time. It proves to be the most self-fertile of all known tomato tetraploids; and phenotypically it is greatly different from them.

The tetraploid was found to be completely cross-sterile with its diploid parental species as well as with the *esculentum* species. Accordingly this self-fertile form could be classed as a new species in a broad sense.

Self-fertilized tetraploids of the con-

* This possibility was suggested by Dr. C. L. Huskins.



FLOWERS AND FRUIT OF DIPLOID AND TETRAPLOID

Figure 15

On the left are shown the flowers and fruit of the normal diploid *L. pimpinellifolium*. On the right are the tetraploid forms. The differences are marked enough to entitle the new form to specific standing. The fact that it is sterile with the "parent" diploid but self fertile, would tend eventually to produce through chromosome mutations and translocations, a form having all the characteristics of a new species.

stitution *RRrr* (red-yellow fruit color) gave progeny with a 20:1 ratio of the two fruit colors. This might indicate a random assortment of the four chromosomes but it also suggests a strong tendency for random distribution of the eight chromatids.

Addendum

Since the above was written, three new *L. pimpinellifolium* tetraploids have been produced from tested pure-line diploids of this species. These are characteristically larger than the diploid but are less sturdy and smaller-fruited than the original tetraploid, which might seem to prove that the original form carried some *esculentum* blood. Nevertheless continued plantings from this original tetraploid, now totaling 248 mature plants comprising four successive generations, have failed to uncover a single individual with even an indication of the typical, hairy *esculentum* pubescence on young foliage and stems.

As a check on the above situation, six different F_1 *esculentum-pimpinellifolium* tetraploids (25 plants) have recently been produced, all of which carry the sparse, partially dominant, hairy pubescence of the *esculentum* species. These tetraploids also are exceptionally self-fertile, but cross-sterile with diploids of the two species.

Further data on the purity of the parental diploid *pimpinellifolium* species have been obtained by testing 60 sister plants as well as grandparental and great-grandparental generations. In no case was a single yellow-fruited form discovered. That the yellow color arose as a mutation in the decapitated plant is perhaps possible, but constitutes too rare a phenomenon to serve as an explanation.

Accordingly, the original *pimpinellifolium* tetraploid, while giving circumstantial evidence of *esculentum* blood, is certainly not typical of such species hybrids. It must still be termed a *pimpinellifolium* tetraploid which is highly self-fertile and cross-sterile with the two diploid species of tomatoes.

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Nutrition and Constitution

DER EINFLUSS DER ERNAHRUNG AUF DIE KONSTITUTION DES ORGANISMS (The influence of nutrition upon the constitution of the organism), *by* A. KATASE. VI, 162 pp., 77 text figures. Publishers: Urban & Schwarzenberg, Berlin and Vienna, 1931. Price, ten marks.

KATASE'S very interesting book is a summary of the work which he and his many pupils have carried out during the last decade at the medical academy of Osaka. The present account is the more valuable since most of the original work was published in Japanese periodicals; it also incorporates many results as yet unpublished. The experiments were designed to analyze the effects of various mineral salts and of the principal organic constituents of food material (carbohydrates, fat, proteins) upon constitutional characteristics of various laboratory animals and of man. In further experiments the influence

of mineral salts, vitamins, hormones, spices, radiation, and so on, upon the metabolism of the principal food constituents was studied. A great variety of morphological, histological, and physiological criteria were used as indicators of the effect of the numerous feeding experiments, the most important of which were: the alkalinity or acidity of the blood, the composition and histological structure of the bones and teeth, the mineral composition of various other organs, the functioning of the hemapoietic organs, the peristaltic movements of the intestines, and many others. The details of the results cannot be reviewed here. Some of the general conclusions, however, to which Katase is led by these results are of great importance to the geneticist.

It appears from these studies that in young growing animals certain nutritional conditions will bring about either hyperacidity or hyperalkalinity of the blood, and that in turn these abnormal reactions of the blood exert a pronounced influence upon the constitution of the organism. Katase believes that fundamentally there are three different types of human constitution: the normal constitution, the acidosis constitution, and the alkalinity constitution. In the normal constitution the blood alkalinity is maintained chiefly by calcium and sodium salts which insure optimal conditions for the vital reactions. The acidosis constitution which Katase believes to be identical with the typus longus or microsplanchnicus of Viola and the asthenic type of Stiller, is characterized by narrowness of the chest, relatively great length of the long bones, low muscle tonus, small and abnormally shaped heart, subnormal development of the uterus, and so on. The alkalinity constitution is assumed to be identical with the typus brevis or macrosplanchnicus of Viola and the apoplectic type of Stiller; it is brought about by magnesium or potassium salts as the chief determiners of the alkalinity of the blood. The principle features of this constitution are a wide chest, relatively short long bones, well developed musculature, a high resistance to juvenile diseases but a tendency towards early symptoms of senility.

Since it was possible to produce experimentally in laboratory animals practically all the characteristics of the acidosis constitution and at least some of the features of the alkalinity constitution, and since it could be shown that these nutritional factors may already bring about constitutional changes in the developing fetus, Katase believes that the different constitutional types are determined entirely by the nutritional conditions obtaining during embroyonic and postnatal growth of the organism. It appears certain that students of the inheritance of constitutional traits will have to take carefully into account the influence of nutrition and of blood alkalinity or acidity upon the characters under consideration. *A priori* there is no reason to assume, however, that all individuals will show the same type of response to certain nutritional conditions. In fact, from all that we know it appears very probable that certain individuals respond with hyperacidity to nutritional conditions which in other individuals do not disturb the normal alkalinity of the blood. The question remains to be studied, then, whether different constitutional types do not at least in part develop upon the basis of inherited tendencies of reaction towards alkalinity or acidity of the blood.

Walter Landauer.

HEAT-INDUCED CHLOROPHYLL MUTATIONS IN MAIZE

Preliminary Report on Chlorophyll Deficiencies Induced by Heating Dormant Seeds

FRED H. HULL AND EDGAR F. GROSSMAN *
University of Florida

DURING the course of recent investigations at the Florida Agricultural Experiment Station an unusually high proportion of striped plants was produced by corn seeds that had been subjected to heat treatment. The striping, similar in appearance to the hereditary type known as japonica, was due to chlorophyll deficiency. The question then naturally arose relative to the manner in which heating acted on dormant embryos to produce chlorophyll deficiences and whether heritable variations might be induced. Further experiments were immediately begun to investigate the effects of heating dormant seed. The results of one season's work are presented herewith as a preliminary report.

Striped plants were first discovered in field plantings of three varieties of corn, *Zea mays*. The seeds for these plantings had been subjected to a series of heat treatments in a constant temperature water bath for various lengths of time. These heat treatments were given in the following manner: ten seeds were placed in a dry test tube which was tightly stoppered and immersed in a water bath which was stirred continuously. The water temperature did not vary over two-tenths of a degree Centigrade for any single treatment. For the series of treatments the temperature varied from 56 to 80 degrees Centigrade by one degree intervals.† The time varied from five to 360 minutes. After treatment 487 treated lots and 34 untreated lots of ten seeds were planted in the field.

The time of exposure extended beyond the lethal point for all three varieties for every temperature above 68°C. Germination and growth were retarded in the sub-lethal range. The most noticeable effect, however, was the white striping which was confined largely to the sub-lethal range. Ninety-three striped plants were observed. They were confined to 47 of the 487 treated groups. Every plant in eight of the groups was striped. The number and distribution of the striped plants made it appear rather certain that striping had been induced by the heat treatment. Striped plants may be found with a frequency of perhaps one per thousand in all three of the varieties of corn used in this test following the usual methods of producing and handling seed. None appeared, however, among 323 plants from control seed.

The degree of striping varied from almost entirely white to a few faint lines. A number of the more extreme cases of striping did not survive the seedling stage. Some plants showed striping on the first four or five leaves only. Others showed stripes throughout the plant and still others showed stripes only in the upper leaves. In one plant striping appeared in the tiller but not in the main stalk. Unfortunately no inflorescence contained a white or striped sector which might

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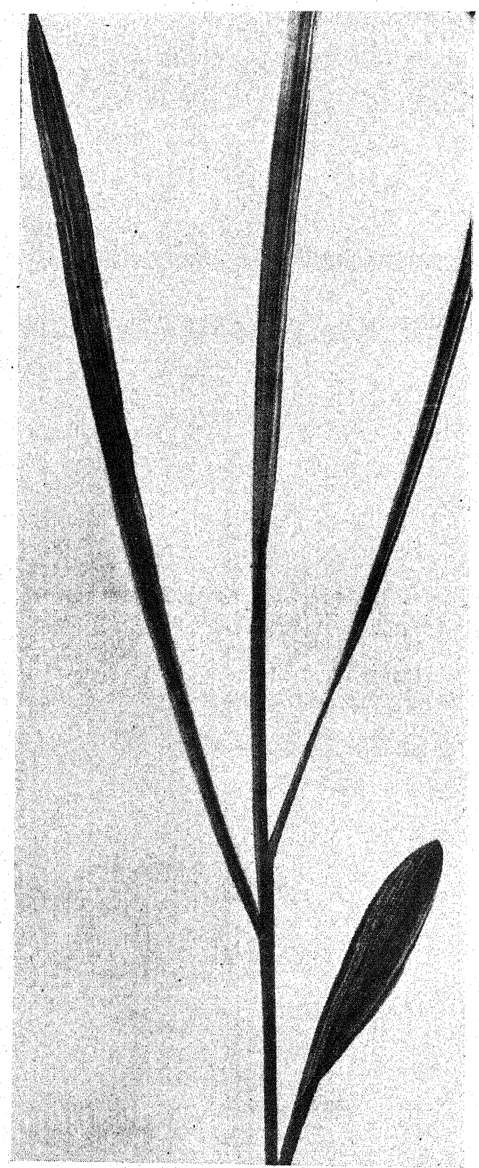
† Temperatures reported in this paper are of the water bath. It was found that the temperature of the seed reached that of the water bath about ten minutes after immersion.

have been used for a specific breeding test.

The next step after finding the striping should have been to run similar tests with corn of known and appropriate genetic constitution. The season was too far advanced, however, to plant corn with any assurance of obtaining seed. It was decided, therefore, to use the material growing in the field. Self-pollinations were made of striped and non-striped plants from treated seed and of plants from control seed. Reciprocal crosses were made between striped and control plants. The yield was cut severely by extreme drouth, but a few ears were harvested. Seedling tests from the controls first showed that the samples of two of the three varieties contained a high proportion of plants heterozygous for a recessive striping. This striping is practically indistinguishable in young plants from that induced by heat. The third variety was almost entirely eliminated by the drouth. The original stock of seed for each variety was obtained from only a few ears. It appears that in each of the two variety samples at least one ear had been included which came from a plant heterozygous for recessive striping. Control plants and striped and non-striped plants from the treated series produced approximately equal proportions of striping in seedling progenies by self-fertilization. The evidence is therefore against rather than for the assumption of inheritance of heat-induced striping through the cytoplasm. It is based upon only seventeen striped plants, however, some of which had only a few faint stripes and none of which had striped sectors in an inflorescence. Recessive gene mutations very probably would not become homozygous in the second generation because the gametes of maize are produced on widely separated parts of the plant. It will be necessary to carry this material through another generation of self-fertilization to test for induced gene mutations.

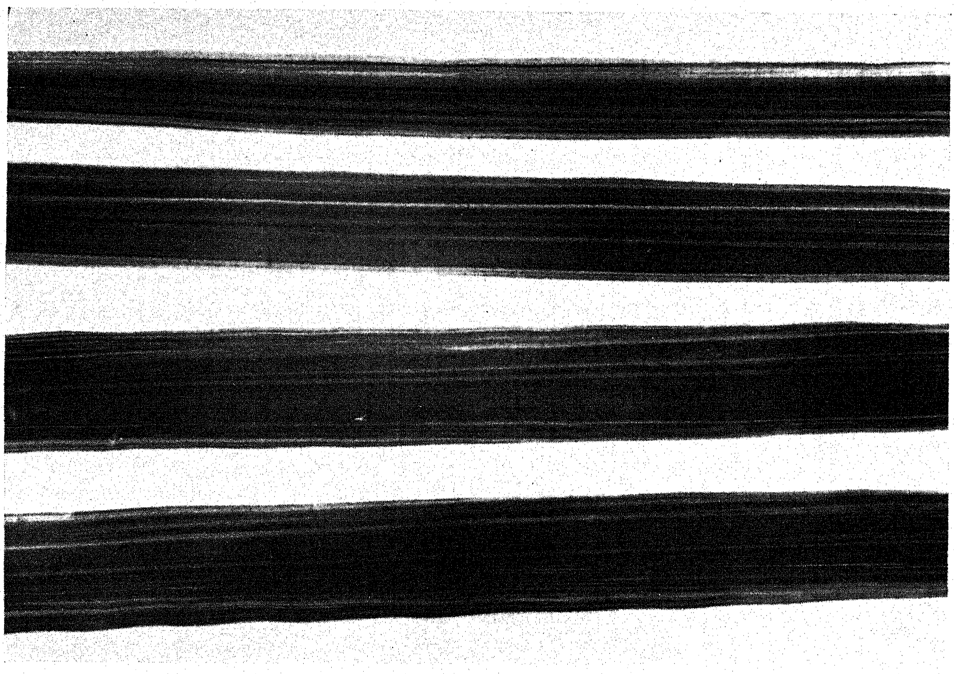


HEAT INDUCED STRIPING

Figure 16

Maize seedling from heat-treated seed showing striping induced by the treatment. In the sub-lethal state many of the seedlings were all striped. Whether this change is inherited in some instances has yet to be proven.

As soon as the striping was discovered in the field a further test was planned to confirm the results and to gain more definite information relative



ENLARGED VIEW OF LEAVES

Figure 17

The striping very closely resembles some forms of striping known to be inherited. Treated seeds of teosinte, a relative of maize, also produce striped plants, but heat treatment of barley and peanuts produced no visible effect.

to the duration and intensity of heat treatment which might effect striping in corn. The seed used for this test was the remnant of a one bushel lot of shelled seed which had been reserved for other experimentation. This sample must have combined at least 100 ears and therefore should be fairly representative of the variety. Previous tests with more than 1,000 plants from this variety have shown that about ten per cent are heterozygous for some seedling chlorophyll deficiency. In other words about ten per cent of the plants, if self-fertilized, have progeny segregating for some seedling chlorophyll deficiency.

Twenty selected seed were used for each treatment which was applied as described above. The temperatures ranged from 60°C. to 83°C. by one degree intervals. The time varied from one minute intervals at the higher temperatures to 30 minute intervals at the lower temperatures. After treatment the seed was planted in the greenhouse. The results of this experiment, conducted in midsummer, are shown in the accompanying table. Striping was largely confined to the higher temperatures. As the length of exposure increased the per cent of striping also increased, usually reaching 100 per cent just short of the lethal point. There is a marked drop in the per cent of striped plants as the temperature decreased, regardless of the length of exposure. Heating was not continued beyond the sub-lethal duration of exposure, however, in the temperatures below 70°C. It is quite possible that a greater per cent striping would have been in evidence at the lower temperatures had the duration of the exposures been lengthened.

Experiments with Barley and with the Peanut

An experiment with the common peanut, *Arachis hypogaea,* was also planned as soon as the striped corn plants were discovered. Peanuts could be planted at that time with good assurance of producing a normal crop. Since the peanut plant is very highly self-fertilized, heterozygosity for recessive chlorohyll deficiency must be rare. Chlorophyll variegation, however, is rather common in commercial peanuts and occasionally pure yellow seedlings appear. These deficiencies behave irregularly and are believed to be of the cytoplasmic type. The production of both gametes in the same flower is advantageous in that, if a mutant sector occurred, the chances of getting homozygous mutant progeny is much greater than with corn. Stadler[3] has pointed out the advantage of using a diploid species for such experiments. The genus, *Arachis,* has not been sufficiently studied to determine whether *A. hypogaea* is diploid or not. Confidence is therefore lessened in the conclusions to be drawn from this experiment since if *A. hypogaea* is a polyploid species a considerable number of recessive mutations might occur without detection.

Two strains of peanuts from different varieties were available that had been isolated by single plant selection three generations back. Both of these strains had been closely inspected for chlorophyll deficiency in the three generations and none had been found. A series of heat treatments were given to seeds of these strains at 70°C. The time of exposure was varied by four minute intervals and extended beyond the lethal point. The only heat effect observed was retarded germination and growth in the immediately sub-lethal series. Approximately 700 progeny from these sub-lethal series were grown well beyond the seedling stage. All of them appeared entirely normal in every respect.

Stadler has proven the suitability of common barley, *Hordeum vulgare,* for measurement of increase in mutation rate by short wave irradiation. Barley is largely self-fertilized, known to the diploid, and its reaction to short wave irradiation is known. Though it is practically impossible to grow barley to maturity in Florida during the summer, it seemed desirable to determine if heat treated barley seed would produce striped plants and to determine the sub-lethal range. Forty seeds of high germination per cent were used for each treatment. The series of treatments ranged from 65°C. to 82°C. The lengths of exposure extended beyond the lethal point at each temperature. Soon after treatment the seeds were planted in the greenhouse. No chlorophyll deficiencies were observed although the plants were grown to about the fifth leaf stage.

Since heating seeds of barley and peanuts did not produce chlorophyll deficiencies as in maize it was decided to try teosinte which is more closely related to maize. Limited amounts of poorly viable seeds of the two species, *Euchlaena perennis,* and *E. mexicana,* were available. A small series of heat treatments resulted in several striped plants from both species. The numbers were too small, however, to indicate the relative proportions of striping in the two. This experiment will be repeated soon on a larger scale to study the effect of heat treatment in relation to polyploidy. *E. mexicana* is believed to be diploid and *E. perennis* tetraploid.[1] It is therefore interesting to note at this time that striping is easily induced by heat treatment of dormant seed of either species.

The experiments reported here indicate that heating dormant seeds of maize and of both species of teosinte to the sub-lethal point will frequently cause chlorophyll deficiencies in the plants. Efforts to produce similar results in common barley and peanuts have failed entirely. No explanation of these differences appears at present.

Inherited Effects

Studies of the inheritance of effects of heat treatment have been limited to first and second generations of

corn and peanuts. The results are all negative, or inconclusive due largely to unsuitability of the material for such studies or to lack of knowledge of the cytology and genetics of the material. White stripes in mature corn plants grown from heated seed indicate that heat may cause changes in the living cell of such fundamental nature as to preclude entirely the appearance of chlorophyll. Such changes, however, do not prevent the cell from growing and dividing. The deficiency is frequently if not always transmitted to daughter cells. The possibility is thus suggested that changes in the cell induced by heat may also be transmitted through the gametophyte to the next sporophyte

In future experiments the effect of heat on gene mutation and chromosome irregularity will be of major importance. According to present conceptions any treatment of seed which caused chlorophyll deficiencies in the immediate plants by affecting the chromatin could do so only in plants heterozygous for recessive chlorophyll deficiency.† Inactivation of the dominant gene in any manner would allow the recessive allelomorph to become evident. The results with corn reported in the accompanying table show every plant striped in many of the sub-lethal series. Since very little more than ten per cent of these plants could have been heterozygous for any chlorophyll deficiency the obvious conclusion is that most of the striping is due to direct effect on the cytoplasm. Inheritance of heat induced chlorophyll deficiency through the cyptoplasm is also of great interest. If it can be demonstrated it will become desirable to investigate from this standpoint the origin and inheritance of variations in chlorophyll intensity in closely related strains such as inbred lines of corn.

Experiments are now in progress with appropriate strains of corn and with common barley to study the effect of heat treatment on rate of gene mutation and on chromosome deficiencies. The inheritance of chlorophyll variations through the cytoplasm is also being investigated. It is hoped that plants will be secured with deficient sectors in ear shoots and tassels so that specific breeding tests on these sectors may be conducted. The results of these experiments will be presented in a later report.

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TABLE I.—Percent of striped plants obtained from groups of 20 Tinsdale corn seed exposed to various constant temperatures for specified lengths of time.

(* indicates that seed failed to germinate)

°C.	Length of Exposure in minutes: 2	4	6	8	10	12	14	16	18	20	22
83	0.0	11.7	58.3	100.0	0.0*						
82	0.0	5.2	62.5	100.0	0.0*						
81	5.2	0.0	23.5	61.0	0.0*						
80	5.2	11.7	29.4	42.8	100.0	0.0					
79	0.0	0.0	10.5	13.3	60.0	100.0					
78	0.0	0.0	0.0	46.6	88.8	100.0	0.0*				
77	0.0	0.0	15.0	37.5	71.4	85.7	100.0				
76		0.0	0.0	38.8	83.3	100.0	0.0	100.0			
75		0.0	0.0	26.3	62.5	86.6	81.8	100.0	100.0		
74		0.0	0.0	20.0	35.7	43.7	70.0	100.0	66.7	100.0	100.0
73				11.7	21.4	77.7	33.3	75.0	100.0	75.0	

°C.	Length of Exposure in minutes: 15	18	21	24	27	30	33	36	39	42	45	48	51	54	57	60	63	66	69	72	75	78	81	84	87	90
72	90.9	100.0	85.7	100.0	100.0	100.0	0.0*	0.0*	0.0*																	
71	31.6	62.5	36.3	83.3	66.7	57.1	66.7	75.0	0.0	100.0	100.0	0.0*														
70			50.0	46.1	35.7	62.5	66.7	50.0	75.0	66.7	100.0	100.0	0.0*	100.0	0.0*											
69						35.7	50.0	36.3	50.0	30.0	44.4	80.0	50.0	37.5	83.3	0.0	66.7	33.3	66.7	66.7	66.7					
68						25.0	10.5	30.7	23.5	50.0	53.8	30.7	25.0	46.1	50.0	33.3	33.3	33.3	40.0	33.3	40.0	25.0	66.7	33.3	60.0	33.3

°C.	Length of Exposure in minutes: 28	32	36	40	44	48	52	56	60	64	68	72	76	80	84	88	92	96	100	104	108	112
67	11.7	22.2	13.2	15.7	12.5	0.0	31.2	41.6	45.4	23.0	28.5	50.0	64.2	36.3	50.0	20.0	50.0	25.0				
66	0.0	0.0	7.6	20.0	5.5	11.0	18.1	12.5	0.0	6.2	33.5	8.3	16.6	0.0	0.0	0.0	0.0	30.7	30.0	0.0	28.5	12.5

°C.	Length of Exposure in minutes: 40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200	210	220	230	240	270	300	330	360	420	480
65	11.1	5.6	0.0	11.7	6.2	23.5	18.1	50.0	7.6	37.5																	
64		11.1	21.0	5.5	6.2	11.1	29.4	28.5	8.3	0.0	8.3	7.1															
63			0.0	0.0	0.0	0.0	0.0	0.0	12.5	14.3	7.1	0.0	15.3	9.0	6.7	6.7	0.0	0.0	8.3	0.0	0.0						
62						0.0			0.0			0.0			5.2			6.6			0.0	0.0	0.0				
61						0.0			0.0			0.0			0.0			0.0			0.0	0.0	7.6	0.0	5.8		
60									5.2			0.0			0.0			0.0			0.0	0.0	0.0	0.0	0.0	0.0	0.0

† The probability of mutation of both genes of an allelomorphic pair within a given cell is assumed to be too small for consideration.

A Three-Coated Sheep

In *Nature* (London) for January 23, 1932, Doctor Albert S. Barker, of the University of Leeds, discusses the constitution of the pelt of a species of sheep from Central Asia, *Ovis astore*.

With rather wide variations, the coat of the domestic sheep is considered to be made up of two kinds of hair: Long, fibrous hair, and fine wool. In the various breeds the proportion of these differ and the classification of the breeds, on the basis of these two hair types, has offered difficulties which it appears that *Ovis astore* may help to solve. In this species there are three types of hair: respectively, a long white fibre; a short black or dark brown fibre; and fine wool. The genetic relations of these three hair types is being investigated, but no data are yet available.

In connection with the breeding of Karakul sheep for fur production this discovery may be of some importance. It is known that crossing the Karakul on breeds having predominately fine wool does not produce nearly as satisfactory a fur as crosses with the coarser wooled breeds. If the kinds of fibre in the pelt of *Ovis astore* are found to be independent in inheritance, as is quite possible, the presence of genes for one or the other type of fibre might be of great importance in fur sheep production.

Pity the Definer of Genetic Terms!

Editor, The Journal of Heredity,
Washington, D. C.

DEAR SIR:

I was interested in reading "A Glossary of Genetic Terms" in the November Journal. I think they are a very poor set of definitions and if taken at their face value would make "confusion worse confounded" in genetics which is already bearing an overload of fairy tales which have no foundation in *scientific* biology. Here are my reactions to a few of the definitions:

Autosome — Definition incorrect — should be: Any chromosome except an allosome (or except the allosomes).

Chromosomes—General term for autosomes and allosomes. The chromosomes are not made up of genes, but linin and chromatin. The chromatin material or the linin may have hereditary potentialities or genes. See Johansen's definition who introduced the word *gene*.

Diploid—Poor definition. In certain cases we know that the chromosomes are not "paired" in the somatic tissues. They are paired in the reduction division stage.

Gamete—a poor definition—Either sex may have reproductive cells regularly which are never gametes. "Sperm or ovum in animals, pollen grain or ovule in plants." This statement is the height of absurdity. The ovule is a sporangium with thousands of cells. The pollen grain is a male gametophyte which may have as high is forty or more cells and in some cases produces 16 spermatozoids. It is no wonder that geneticists get exceedingly muddled when dealing with such subjects as Xenia, ectogony, etc.

Homologous. This is a very poor and misleading definition. The bodies of many plants are haploid, as mosses, liverworts, numerous algae, gametophytes of ferms, etc.

Meiosis. Another misleading definition. Reduction has nothing to do directly with gametogenesis. It often occurs at the time of the germination of the zygote. In the higher plants it precedes the formation of the spores a whole generation ahead of the time of gametogenesis which occurs in a haploid individual. In some of the algae and in the metazoa, reduction precedes gametogenesis.

Microspore. Pollen. Wrong again. A microspore is an uninucleate cell from which the pollen grain develops. The pollen grain may have dozens of cells.

Mutation. Poor definition. Not good English.

Parthenogenesis. A hazy definition. There are plenty of "germ cells" never intended to be fertilized, like the spores or a moss or fern for example.

Sex-linked. There is no such "critter." What is intended is allosome-linked in contrast to autosome-linked. One can see how absurd the term becomes when he sees a case of sex reversal and the given allosome is then in an individual or tissue of the opposite sex from the usual.

Somatic. A poor definition. The chromosomes are not "paired" in the soma, they are in meiosis. Somatic *refers to the body* not "resembles the body."

Sporophyte. The sporophyte is not a "phase." It is a generation. The sporophyte gives rise to spores from which the gametophytes develop. The gametophytes are produced in the sporangia in the higher plants, not in the "flowers."

Zygote. Poor definition. In some cases there are not differentiated sperms and ova. Nor is there necessarily a "new individual in the single-cell stage." In many cases the zygote undergoes reduction on germination and the "individual" following is haploid.

Sincerely yours,

JOHN H. SCHAFFNER,
Research Professor, Ohio State University.

In Reply to Professor Shaffner's Criticism of Definitions of Genetic Terms

SINCE having undertaken to publish a glossary in each issue of the JOURNAL, the Editor has been confronted with a number of criticisms—some constructive, some destructive, most of them different, and all of them right. This situation demonstrates the difficulties of such an undertaking, but it also emphasizes the need for such a glossary, so in spite of Bricks and Bouquets (Dr. Schaffner may be surprised to learn there have been some of the latter!) the glossary is being continued.

Attention should be called at the outset to the fact that these definitions are intended for rather a special purpose. In the first place they are genetic definitions. From a histological or cytological point of view, chromosomes may be made up of linin and chromatin; nevertheless in the present state of our knowledge, the chromosome is, from the genetic point of view, also made up of genes. At least the genes apparently form an essential part of the structure of the chromosomes; and they are arranged in an orderly fashion. In a brief *genetic* definition of the chromosome this is the essential point.

Since these definitions are planned as a glossary for specific articles rather than for dictionary use, the terms are defined with an eye to rather special circumstances. It is quite possible, though lexicographically deplorable, that the definitions may vary in different issues to conform to the varying meanings used by different authors. Professor Schaffner's communication demonstrates this point, and when we have an article on the genetics of ferns or mosses a complete new set of definitions may be required. These definitions would not serve for a discussion of the genetics of fruit-flies, maize or men, or of most of the organisms on which extensive genetic research has been done.

Another important point which Professor Schaffner overlooks is that the definitions must be in a form to be as readily intelligible as possible to people with an average biological vocabulary, such as we would expect to be pos-

sessed, say, by a chemist or physicist member of the American Association for the Advancement of Science. The definition of an autosome in terms of allosomes is seen, for this reason, to fail entirely to meet the requirements. While neither "autosome" or "allosome" is in the latest edition of *Webster's New International Dictionary* (even under "new words"), "autosome" is much more widely defined and discussed in genetic and cytological texts than is "allosome." We would be guilty of intellectual Pharisaism in its worst form if we furnish our readers with a "guide to genetic terms," and give them such stones as "allosomes" for the bread of interpretative explanation.

As an example of the difficulties of such definitions may I comment on the criticisms involving "ovum" and "egg." In mammals, an *ovum* is a sex cell. Plants do not in accepted botanical parlance have *ova,* but their *ovules* are not sex cells. One cell in an ovule is a sex cell; this cell is the *egg*. Returning to animals again, the avian *egg* is not a sex cell; by the time it reaches the breakfast table it is a zygote with frills. Thus to define a *gamete* as an "egg" is both right and wrong. To define an *ovule* as a gamete is, unfortunately, always wrong. The carry-over from *ovum,* after struggling with a number of such complexities in definition, is not difficult to explain. When it is also considered that limited time is available on account of the exigencies of publication, the explanation for some of the slips in earlier glossaries is not hard to find. Just as an indication of the difficulties of catching such errors "on the run" I may say that these definitions were read before publication, but not critically pondered over, by four competent biologists. After publication all of them readily admitted the correctness of some of Professor Schaffner's objections, though not of all.

Detailed discussion of all of Professor Schaffner's criticisms is hardly required; some of them we find are not shared by geneticists generally, and some of them seem to us rather hypertechnicalities. At the present time genetics deals much more with the higher plants and animals than it does with ferns, mosses, and algae. To bring in the alternating "generations" of the cryptogams is needless confusion with many genetic problems.

ROBERT COOK.

Preservation of Specimens and Standardization of Measurements

METHODEN ZUR KONSERVIERUNG VON ORGANEN AND GANZEN ORGANISMEN (Methods for preserving organs and whole organisms), *by* G. SCHMEIDEL; ANTHROPOLOGISCHE MESSUNGEN AM LEBENDEN MENSCHEN (Anthropometry of living humans), *by* R. H. POST. Abderhalden's Handbuch der biologischen Arbeitsmethoden, Abteilung VII, Methoden der vergleichenden morphologischen Forschung, Teil 2, Helt 2. Price, 4.80 marks. Urban & Schwarzenberg, Berlin and Vienna, 1931.

This 362nd issue of Abderhalden's encyclopedia of biological methods contains a short summary of the methods for preserving organs and whole specimens, chiefly for museum purposes, and a critical review of the more important methods now in use for taking anthropometric measurements of humans. Post's report on anthropometric methods was carried out for the Eugenics Society as on attempt to demonstrate the necessity of an international standardization of these methods. Nobody who reads this report can doubt the urgent need of such standardization.

WALTER LANDAUER.

POPULATION TRENDS IN VERMONT

THE fifth annual report of the Eugenics Survey of Vermont has recently appeared—an 85-page illustrated booklet that bears on a very important eugenic problem, "Selective Migration from Country to City.* There has been much talk about the drain on the country of our increasingly urban civilization, buttressed with no little evidence, much of it unfortunately circumstantial. This eighteen-month study of three Vermont rural communities presents first-hand information of just how the present movement of population is affecting our racial stock. By taking part in the social life of three villages and by reference to tax lists, etc., the workers who compiled this material have attempted to find out about every person who moved into or out of these communities during the preceding twenty years.

Vermont is in some ways an exceptional state for such a study. Its entire population only a little greater than that of Rochester, N. Y., in every census since 1850 approximately forty per cent of native Vermonters have been living in some other state. Immigration into the state has been slight, in fact it is probably the only state in which proportion of immigrants has not changed for a century. Seventy-one per cent of the entire population is native to the state—a unique situation in this restless land. Thus Vermont presents an unusual opportunity to find what is the effect of long-continued drain on a stock that has been changed by immigration but very little.

Even in Vermont, predominatingly rural as it still is, the trend has been towards the cities. "The only increase in the state since 1830 except for one decade has been due to the increase in the ten towns whose population exceeds 5,000." In spite of an agricultural population reduced in numbers, the value of agricultural products has increased, and the average well-being of those remaining on the land has improved. Economically this seems a desirable situation, but racially it may be quite the reverse, if the leaders tend to be continually recruited to the cities, and to other states.

The background of the study is three townships, fictitously called "Pomona," "Beaufield," and "Sylvania,"—respectively a self-sufficient urbanized little farming community, a farming town of a high average type, and a mountain township in the western part of the state, very sparsely settled. The peak of population in all three towns was reached in 1830-50. Then Pomona had a population of 1,084, Beaufield of 1,264, and Sylvania 1,606. Now the score of inhabitants stands 725, 627, and 563. All these rural townships have shrunk until they are from one-third to two-thirds their former size, and this during a period when the population of the United States has increased from 23 million to 122 million, or *over five times!*

Even in a brief résumé it is hard to resist at least a glance at the three towns:

POMONA. Of first importance to a community is that its members are able to earn a living and provide for their dependents. In Pomona there is little question about this ability among the majority of the people. The average income of its farmers leaves little net profit but is sufficient to provide a comfortable living. Business men and wage earners in the village also earn a fair living, wage earners being considerably better off than they are in Beaufield and Sylvania. Nobody is "on the town." Considering the resources of the valley and the economic conditions in agricultural areas, the people of this town show good ability to earn as much as they do.

The homes that Pomona residents provide for their families are, with but few excep-

*Fifth Annual Report of the Eugenics Survey of Vermont: Selective Migration from Three Rural Vermont Towns and its Significance. University of Vermont. Burlington, 1931.

tions, very comfortable. Electricity, running water, and modern plumbing are in nearly every home. Up-to-date books and magazines that are not in the homes are to be had at the fine town library. Some of the old retired farmers are a little concerned about the way in which their sons run the old family farm. In their day it was a matter of work from sunrise to sunset with little thought for anything beyond the immediate problem of earning a living for their families. But their sons do not believe in working such long hours, insist on modern conveniences and comforts, and even take a day off occasionally to motor with their family on a holiday, reflecting in this way the improvement of the farmer's lot and the change in his attitude toward life.

The people of Pomona have undertaken a big task in order to give their children the best advantages of education. The town was one of the first to organize a consolidated school in the village where the children could be sure of good instruction. Even 30 years ago it was possible to obtain two years' high school in town. Now a four-year course is available. The maintenance of such a school has not been without difficulties. The cost and upkeep is greater than anticipated and the taxes, therefore, are high. The difficulty of bringing the children in from remote farms and the effect that a centralization of education has had upon the remote districts which have become depleted since the closing down of district schools, has arounsed criticism among some people. But in spite of difficulties the fine consolidated school continues. * * * *

BEAUFIELD. The ability of the people of Beaufield to provide a living for themselves and their dependents is on a level practically parallel with that of Pomona. For farmers, incomes range even higher in Beaufield than in Pomona, but the average income in Beaufield is a little lower. The wage earners and business men earn somewhat less than do those in Pomona. But this is due largely to the purely farming aspect of Beaufield compared to the urban aspect of Pomona. Homes are maintained on as high a level as those in Pomona except for the fact that fewer homes in Beaufield are equipped with electricity and modern conveniences.

The children of the town receive their education in the eight district schools of the town which provide the elementary school courses. To obtain high school training it is necessary to go to Norton. Outside of the schools, however, the only organization that cares for the interests of the children of the town has been, until very recently, the C. A. R.—Children of the American Revolution. Such an organization, fine as it is, can serve the needs of only a small group in the community and is not particularly suited to bringing together children of old residents and those of foreign-born immigrants. The 4-H Club, the one club which does serve the need of all children of the farming community, has only very recently been organized because of the difficulty of finding a leader willing to carry on the work. There is no organized recreation.

SYLVANIA. Sylvania is different. Its people are not driven by the will "to get on," as are the people on the other side of the mountain. For them "to be content" means much more than "to get on." Earning a living is something of a haphazard affair for most people in the town. Few work at steady jobs or own private businesses such as store, garage or woodturning factory. Few, too, persist at farming. Most of the townspeople have long since decided that to work as hard as the farmers of the town do to earn so little, isn't worth the effort. They prefer rather to raise "just enough for ourselves" and earn their living by other means than farming.

The ways in which these citizens earn their living illustrate the ingenuity of man in competing with nature. In the spring they usually tap maple trees on their "farms" and boil down maple sugar to sell. During the summer and fall many go ferning. This has been quite an industry for many years. Sometimes a "boss" rents a whole side of a mountain and hires men and women to pick a special variety of fern which he ships to florists in Boston, or else people do this "on their own." For every neat bunch of twenty-four fresh, perfect ferns twelve inches long, the pickers earn one cent. On an especially good day fast pickers occasionally earn as much as six dollars but the average daily earning is nearer three dollars. In the winter there is little work to be had except occasionally to cut some timber and sell a few loads of wood.

There are other ways of earning a living. People on remote farms beyond the area served by a school district sometimes have as their chief source of income the money paid them by the town for driving the children back and forth to school. It is said that some people even choose to live at a considerable distance from school so that they may be assured of this steady income. Then there is always work at repairing and maintaining the roads. This is a highly desired job because it means more or less steady employment during the summer and a steady wage of $2.75 to $3.00 per day. Fortunately the flood of 1927 did considerable damage in the town, and the repairing of roads, the building of bridges and the rebuilding of the West River railroad that runs through the town have provided employment for many people ever since. Now that this work is nearly completed it seems uncertain what

the people will do next unless lumbering booms again. * * * *

The immigration into Sylvania appears to be of a different character from that into Pomona and Beaufield. It is not an immigration of people of somewhat inferior stock as is the tendency in Pomona, or of foreign stock as in Beaufield, but rather it tends to be an immigration of able but maladjusted people. The impression that newcomers give is that they are running away from life and that Sylvania has provided them with the means for escape. Some of these are people who have "come in the night" to Sylvania, to find there freedom from interference. Many who come are well-educated people, a few even talented. Of two brothers who have come in with their families, one is an accomplished violinist, the other a clever cartoonist. After wide traveling, they have chosen to make their homes on worthless back farms of Sylvania. Religious extremests find a haven here. Near Grand Falls two families still remain of a former colony of Seventh Day Adventists. These two families maintain a church in which room is made for a school. Here five children are instructed without supervision by an Adventist mother who teaches geography according to the location of Missions of Adventists throughout the world. Nearby on a lonely farm lives a Finnish woman of rare character who after years of hard work in New York and other cities has bought herself this piece of land which nothing could induce her to leave. Numerous examples such as these cannot fail to convey the impression that these people have found a means of escape in Sylvania * * * *

But these are instances of maladjusted people, who after all are not many in the entire population of the town. The average citizen of Sylvania is a fine type and a choice character that is all too fast disappearing. Among representatives of well-known families in town are direct descendants of Vermont's hero, Ethan Allen, and representatives of the families who in the past have given to the country Presidents Pierce and Taft. The descendants of these and other illustrious families are still among the outstanding people in town. Only a detailed study of each of these families might tell the extent to which the members who have remained in Sylvania stand above or below the average for all the members of each family, in ability and energy.

The people of the three communities are all "different" in a different way, are they not? Why? Were they to be interchanged would they remain the same? Probably not, but there is no way to get a definite answer to this question which is so fundamental in studies of heredity and environment. A few sets of identical twins, or better, triplets, distributed in the three towns, and observed for twenty years might give us a positive answer of great value.

In conclusion it was found that the emigration has largely been made up of young people, and by changes due to the type that immigrated to replace them. The declining population of the communities has tended to increase consanguineous marriages, not necessarily an evil. The fact that the immigrants average of a rather poorer stock is unfortunate. The size of family in these towns today is small—2.5 children the average for the three towns, about a child less than the number necessary to maintain a stationery population. Thus a still further decrease in the size of these towns is to be expected.

The following recommendations conclude the report:

To maintain people of fine stock in the rural communities two conditions are most essential. One is that in the rural sections which are fertile and well suited to cultivation conditions be so improved that people who really love the land are encouraged to remain. The other is that in the rural sections where the land is poor and little suited to cultivation the people be encouraged to leave for more progressive communities, lest deterioration in the quality of the stock of future citizens occur.

One line of action that is necessary to holding a fine class of people in the rural communities is that both the economic and social conditions of the farmers in the good rural sections be improved. Economic improvement for Vermont farmers can be brought about by greater cooperation in marketing, and by producing and selling products of only first class quality—for Vermont's future prosperity lies not in the quantity but in the quality of its products. Amelioration in the social life can come only through better rural schools, improvement in rural health conditions, and in means to enjoy comforts and modern conveniences. Such improvements will do much to encourage people of fine character, who really love the land, to remain in the rural communities; and only when such improvements have been made can the people be encouraged to raise larger families so that the same fine quality of stock may always be represented in the rural towns of the state.

The second line of action is, if anything, more important than the first. It is that the

State encourage people who live on marginal land to move to the more progressive communities of the State by taking over all marginal land. This of course would demand first a careful study of each township to determine reorganization on the basis of the uses for which land is best suited. It may appear at first a costly program, but in the long run it will repay a hundred fold in human values even more than in land values. Deterioration can take place only in poor isolated communities where the potential capacities of the people are not challenged into use. If then Vermont wishes its future citizens to have the same fine qualities of character that marked the early builders of the State, it must pursue a line of action that will prevent deterioration from taking place by providing a social environment that will continue to bring out all the fine qualities in the character of the people.

The recommendations are almost truisms—and yet, are they in the best interests of the race? Is it possible that instead of discouraging the marginal groups they should be encouraged? We know from reams of statistics what happens to birth-rates when a people are fattened. People on "marginal" farms made Vermont a great state; people whose economic condition, in many cases, was incomparably worse than that on the marginal farms today. Perhaps these marginal people are the state's last hold on greatness if the drain of the cities is as fatal as some believe. "Sylvania is different" the investigators report. In that "difference" one senses real human qualities—priceless qualities that might be lost by a process of buying out the marginals. Perhaps there is deterioration there too, but is it possible that it is not as rapid under hard conditions as under easy? We do not know, but it may be suggested at least that fostering the marginal ruralist through helping him to help himself, not rooting him out, is the most enlightened proceeding racially. At least it has the advantage of doing no harm. Something keeps some of these people on the farm besides "degeneracy," or "backwardness" even, and this quality may be racially without price. We are doing so much to eliminate our priceless "primatives" that any movement that may hasten the process, especially in the name of eugenics is altogether to be deplored. Economics is, as recent events have proved, bankrupt as a science. To attempt to apply "economic principles" to problems of racial welfare is to invite disaster.

In spite of these doubts as to the wisdom of the elimination of the "marginal" rural dweller, the fifth report of the Vermont Eugenics Survey is an extremely interesting document. Its frank attempt to "straddle" the technical and lay interests is to be commended. We need more research in human heredity reported in a way that is intelligible *and interesting* to a wider circle of people; for a eugenic program, to be effective at all, must go deeper than "encouraging" people to have large families. This never-ending battle of births and deaths which determines whether we go up or down in the scale of culture and life, must be brought home vitally to a much large proportion of our averagely superior people. If this is done perhaps some of the problems will solve themselves. This survey is a document that would be a revelation to many intelligent people who still are blissfully ignorant that such problems exist. There are millions who have never even heard of heredity in an enlightened sense—and who are not morons by any means. That this is the case is something of a reflection on those who do have the knowledge. The tragedy is that this report will probably reach so few.

R. C.

LINKAGE OF GENES FOR CREST AND FRIZZLE

In the Domestic Fowl

A. D. SUTTLE and G. R. SIPE *

THE subject of genetics is constantly being more closely interwoven with poultry breeding as more investigators test the principles of genetics relating to inheritance in the domestic fowl.

Probably the first phenomenon to be studied was that of sex linkage as reported by Cushman[2]. He mated Indian Game males to Barred Rock females and noted that the sons were barred, while daughters were free from barring and largely black in color (see Jull[12]). Cushman did not explain this mode of inheritance, but in 1908 Punnet, Bateson and Spillman[18] explained this type of inheritance of barring on the basis of a sex linked character. Since the publication of the work of these investigators several others[5, 7, 14, 16] have demonstrated this type of barring inheritance, in this and other breeds.

A recent phenomenon to be discovered was that of linkage in poultry, or a tendency for certain characters of one individual to remain together in later generations. As yet there is not much information available on this subject, and little exact data—Goodale[6] and Haldane[8], in connection with poultry genetic studies, showed the existence of linkage in poultry. Jull[11] and Parkhurst[15] concluded from an extended study that there was a positive correlation between early maturity and annual egg production. Hays[9] found early maturity to be inherited. Dunn and Jull[3] in their inheritance studies in the fowl concluded that the genes for hernia and dominant white are closely linked. Landauer[13] and Dunn[2] as well as Serebrovsky[17] found evidence of autosmal crossing over in female fowls.

Linkage of Crest and Frizzle Characters

It seems desirable to report the results of some preliminary work started in 1925, as the stock dealt with herein traced to this source. The summary follows: A frizzle male mated to White Leghorn females and a White Leghorn male mated to frizzle females gave six normal and four frizzle chicks. Another mating of frizzle males with normal females gave one frizzle to one normal chick. Two of the females in the latter group showed a distinct crest. From the progeny of the above matings several pens were made up as follows:

Pen No. 1. Normal F_1 female and frizzle male.

Pen No. 2. Normal F_1 female and male.

Pen No. 3 consisted of frizzle male and females, some of which were the progeny of the above matings and some purchased, one of the chicks purchased being a crested female.

Pen No. 4 was made up of normal females and the same frizzle male which was used in the previous matings.

The purpose of this investigation was to determine, First: whether or not a strain of frizzle fowls could be produced that would breed true; Second: The genetic behavior of the gene for frizzle feathers; Third: The genetic behavior of the gene for crested head; and Fourth: The relationship of

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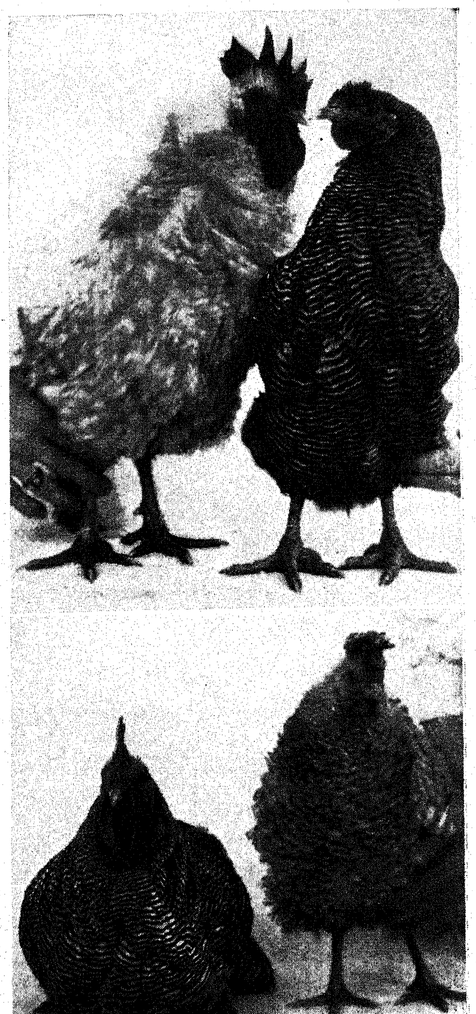
the genes for frizzle feathers and crested head.

The results of these matings is given in Table I.

This count was made while the individuals were rather young and probably is not as accurate as the later counts. As the male birds, except one frizzle-crested male and the two crested males, were sold, the results could not be rechecked. It was observed that at maturity there were twelve crested females from this group which indicates that four of the birds which had been classified as non-crested actually developed crests. Also four females from the group the year before were added to these twelve, making a total of 16 crested frizzle females. This seemed entirely too large a number in comparison with only two crested females with normal plumage. It was therefore suspected that there must be a linkage between the characters for frizzle feathers and crested head. Pen No. 3 gave what was thought to be two kinds of frizzle, namely, homozygous and heterozygous. The description of these two birds will be taken up later.

After breeding some of what were thought to be homozygous frizzles, it was evident that the birds that were heterozygous for the frizzle character could be separated phenotypically from those that were homozygous. All birds were heterozygous for the crested character. Sixteen of these frizzle, crested females were mated with two White Rock males. The two normal crested males were mated with normal hens. The homozygous frizzles were penned separately to insure complete isolation. It was certain that the sixteen females selected and mated to White Rock males were heterozygous for both crest and frizzle genes, the



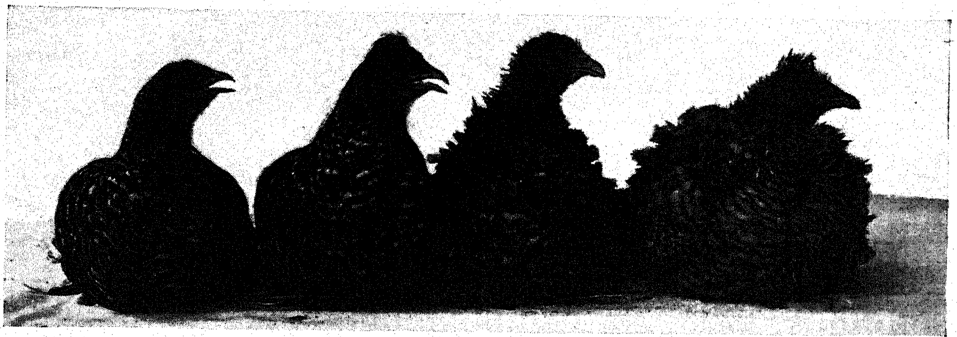
FOWLS USED IN MATINGS

Figure 18

Above Crested heterozygous frizzle male and normal Barred Rock female; *below* a normal Barred Rock male and a crested heterozygous frizzle female. The frizzled fowls could be recognized shortly after hatching, but the crest could sometimes not be detected until maturity.

TABLE I—Results of Crosses Made in 1925

	Normal Non-Crest		Normal Crested		Frizzle Non-Crest		Frizzle Crest		Total
	♀	♂	♀	♂	♀	♂	♀	♂	
Pen 1 (Normal F_1 ♀ x Frizzle ♂)	3	1				5			9
Pen 2 (Normal F_1 ♀ x Normal F_1 ♂)	6	3							9
Pen 3 (Frizzle ♀ x Frizzle ♂)	4	5	1	1	6	4	(Sex?		22
Pen 4 (Normal ♀ x Frizzle ♂)	10	6	2	2	2	3	7	1	33



TYPES OF FEATHERING

Figure 19

1—Normal; 2—Crested; 3—Frizzle; 4—Frizzle Crested.

above mating being equivalent to a back cross. From this group about one hundred chicks hatched, but only thirty survived. The count was very definite in the normal group so far as the crested character was concerned. There was never any difficulty in distinguishing frizzles from normals, but in the early stages of the work there was some difficulty in detecting the crest in the frizzle group, especially in the males. This probably accounts for the discrepancy shown in Table II. This difficulty in detecting crest was largely overcome with experience. The count for this group is given in Table II. *

From the homozygous group selected from pen 3, about thirty individuals grew to maturity; and, with one possible exception, all were true to type, showing that they were homozygous for the frizzle gene. The exception was a red crested bird, which was attributed to an incorrectly marked egg being placed with a group of eggs known to have been laid by a homozygous female. In a cross between crested males and normal females thirty-three chicks were raised, fourteen of which were normal and nineteen crested. The homozygous birds mated *inter-se* gave homozygous exclusively, showing that they were genotypically pure. The heterozygotes are shown in the tables below.†

The distribution shown in Tables III and III*A* indicates that the gene for crest and the gene for frizzle must be in the same chromosome, some 28.48 units apart. The most unusual and interesting feature of the results is the fact that both female and male F_1's show identical percentages of crossing-over. This fact, so far as we know, is the most conclusive evidence of crossing-over in the autosomes of the female fowl. The results indicate that the crossing over in the male and

***TABLE II—Progeny Resulting from mating 16 Crested Frizzle females with two White Rock males.**

NORMAL		FRIZZLE	
Non-crest	Crest	Non-crest	Crest
13	5	8	4

†TABLE III—Heterozygous frizzle crested male × normal females.

	NORMAL		FRIZZLE	
	Non-Crest	Crest	Non-Crest	Crest
Expected	20.25	20.25	20.25	20.25
Observed	33	9	13	26

TABLE IIIA—Normal male × Heterozygous frizzle crested female.

	NORMAL		FRIZZLE	
	Non-Crest	Crest	Non-Crest	Crest
Observed	28	11	12	26
Expected	19.25	19.25	19.25	19.25
Observed	61	20	25	52
Expected	39.5	39.5	39.5	39.5

in the female are identical. The actual counts show a percentage of 27.16 in one instance and 29.87 in the other. The probability for such a deviation from a 1:1 ratio is only one out of 1,923 trials. It is, therefore, apparent that the gene for frizzle and the gene for crest are linked and are approximately 28 units apart.

Characteristics of Homozygous Frizzles

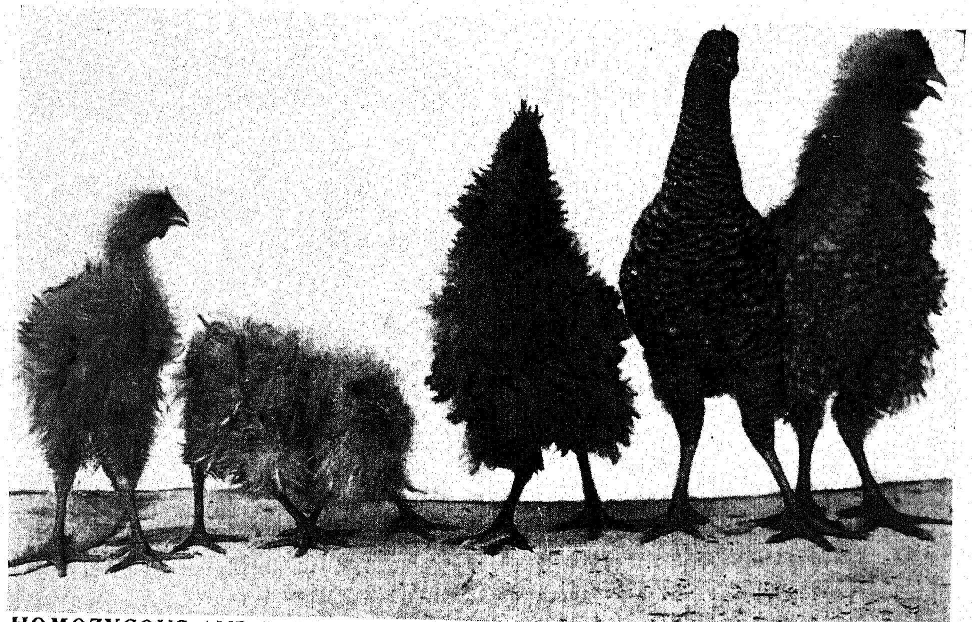
An interesting feature, probably of more practical importance, is that a pure breed of frizzle birds has been isolated and that this strain is both phenotypically and genotypically different from the heterozygotes, or common frizzles, seen in so many negro yards in the South. This agrees entirely with Hutt's report,[10] that homozygous frizzle fowls do not die in the egg, but may be had as a pure strain of true breeding frizzles.

The pure frizzles are much smaller than either the heterozygotes, or the normals, as shown by the weights in Table 4.

The degree of frizzling in the homozygous birds is as far beyond the heterozygous frizzle as the heterozygous frizzle is beyond the normal. The feathers are more intensely curved and the barbs are decidedly more curled than in the heterozygous birds. The absence of barbs in the secondary feathers is more pronounced and the barbs are more intensely twisted about each other. The feathers over the entire body are decidedly smaller, and the barbs are so much twisted about each other that the birds have the appearance of being wet. The feathers resemble wool, or hair. The quills are decidedly more curved and twisted in the homozygotes than in the heterozygotes. Even though the barbs shed more freely from the homozygotes than from the heterozygotes, a careful count shows that the barbs are equally numerous and equally close on the quills in both homozygotes and heterozygotes.

TABLE IV.—Weight of Homozygous Frizzle, Heterozygous Frizzle and Normal Chicks.

Date of Hatch	No. Chicks Homo.	No. Chicks Hetero.	No. Chicks Normal	Average Weight	Ratio Normal to Homo.	Ratio Hetero. to Homo.	Ratio Normal to Hetero.
Feb. 12	2			2.2			
Feb. 12			2	3.2	45%		
Feb. 18	5			1.5			
Feb. 18		6		2.06			
Feb. 18			4	2.50	66%	37%	21%
Mch. 5	4			1.22			
Mch. 5		8		2.20	88%		
Mch. 5			4	2.30		80%	45%
Mch. 12	5			1.04			
Mch. 12		7		1.70			
Mch. 12			5	1.78	71%	63%	4%
Mch. 27	4			1.10			
Mch. 27		2		1.50			
Mch. 27			1	2.20	100%	36%	46%
Apr. 3	8			1.10			
Apr. 3		3		1.56			
Apr. 3			6	1.85	68%	42%	18%
Apr. 10	4			1.10			
Apr. 10		3		1.00			
Apr. 10			4	1.40	27%	10%	40%
Apr. 17	2			.60			
Apr. 17		8		1.05			
Apr. 17			7	1.08	80%	75%	02%
Apr. 24	3			.66			
Apr. 24							
Apr. 24			6	1.03	55%		
Apr. 27			1	1.60			



HOMOZYGOUS AND HETEROZYGOUS FRIZZLES COMPARED WITH NORMAL

Figure 20

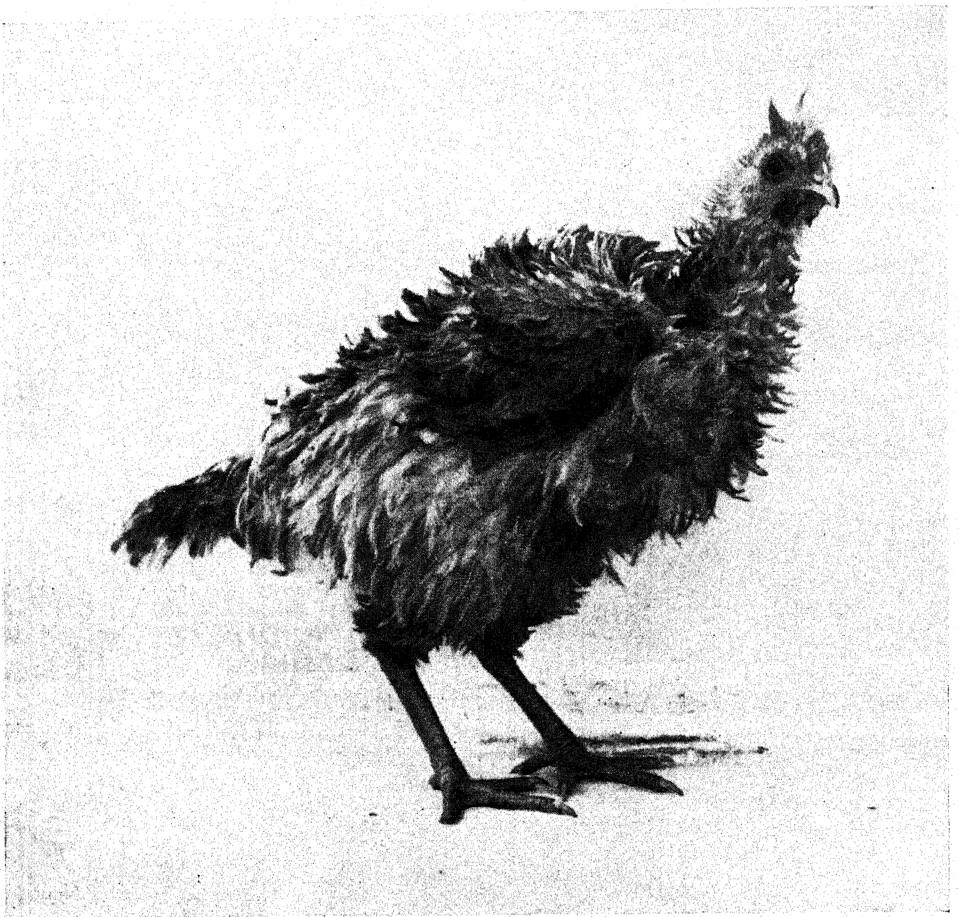
Showing three homozygous frizzles, one heterozygous frizzle crested, one normal, and the one heterozygous non-crested frizzle. Note size and wet appearance of the homozygotes. Also compare size of normal with heterozygotes on each side. The homozygous frizzles are two days older than are the other chickens in the photograph.

RELATIVE SIZES

Figure 21

1: Normal. 2: Heterzygous. 3: Homozygous. Note relative sizes. The Homozygous frizzle is two days older than the others.

The structural differences in the feathers of the different types of frizzle birds are carefully given by Landauer and Dunn, in this JOURNAL.[13] It was noted that the quills of the homozygous birds lost their barbs in summer to the extent of leaving the bodies of the individuals almost entirely exposed. At times, usually in late summer, the quills were so nearly naked of their barbs that the bodies of the fowls were severely burned by the sun. Even during the winter, the coats of the homozygotes are so much reduced that two of the hens actually froze to death in a model Mississippi poultry house; and a grown cockerel, poorly "coated" drowned one night in February upon failing to find his way into the house. So outstanding were the differences between the homozygotes and heterozygotes that almost any person upon entering the yard could differentiate the two kinds of birds.



MATURE HOMOZYGOUS-CRESTED FEMALE
Figure 22
Note the peculiar feathers which more nearly resemble hair than true feathers.

Table IV shows clearly that normal chicks with the same feed and under the same conditions are considerably larger than homozygotes. As a matter of fact, they are 66 per cent heavier at the same age. It is also true that the heterozygotes are 49 per cent heavier with the same feed and under the same conditions. The normals, however, were only 25 per cent heavier than the heterozygotes under like conditions. It is hardly accurate to say that the entire 66 2/3 per cent increase in weight of the normal birds over the homozygotes was due entirely to the gene for frizzle, because the male bird which sired the normal chicks and the heterozygotes was a well-bred Barred Rock, much larger than the homozygous males. It is safe to say that he was twice as large as the homozygous males. The crested frizzle male in Pen 4 was also a large bird and was within a pound or two of the Rock male. A safer basis for comparison of the size of the heterozygotes and homozygotes is the weights of the hens in Pen 13 and Pen 14, where the parentage was identical. Here the average weight of the 14 heterozygous hens was 4.60 pounds, while the weight of the homozygous was only 3 pounds. It is evident that the heterozygotes were 55 per cent larger. Such a difference is just what one would predict from ob-

serving the two types of birds.

Table VI shows that the homozygous eggs gave a 58% hatch, while the heterozygous eggs gave only a 44.31% hatch. The eggs for the two types were placed in different incubators. In the settings of heterozygous eggs of March 27 the contact points of the incubator stuck, causing improper heating: only three chicks hatched from 36 eggs, fewer than would have hatched otherwise. Under normal conditions the percentage of hatch for the heterozygotes would still have been eight per cent less than for the homozygotes. On the other hand, the percentages of birds raised from the hetrozygotes were slightly higher than from the homozygotes.

Summary

The work on this problem has some outstanding features:

1. That frizzle is a simple Mendelian factor incompletely dominant to normal feathers.
2. That crested head is a simple Mendelian dominant to normal head.
3. The results herewith reported show that the genes, for crested head and frizzle feathers are in the same chromosome about 28 units apart.
4. That there is crossing over in the autosomes of the female fowl.
5. That the gene for frizzle is semi-lethal, reducing the individual from 25% to 50% in size and a considerable amount in vigor.

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TABLE V.—Weight, in pounds, of Heterzygous and Homozygous hens of the same parentage.

	Heterozygous (Pen 14)	Homozygous (Pen 13)
	5	2.6
	5.2	2.6
	4.1	2.8
	4.6	3.6
	4.0	4.8
	5.4	2.2
	5.6	
	3.6	
	3.4	
	4.0	
	5.0	
	4.6	
	5.2	
Average	4.6	3.0

TABLE VI—Hatchability of Eggs of Homozygous and of Heterzygous Frizzle Fowls.

	Pen 13		HOMO.	Pen 14		HETERO.
	Eggs set	Chicks hatched	Chicks raised	Eggs set	Chicks hatched	Chicks raised
Feb. 12	11	5	2	5	2	2
Feb. 18	17	9	5	18	13	11
Mch. 5	14	9	4	24	14	13
Mch. 12	16	8	5	26	17	11
Mch. 27	15	6	4	36	3	3
Apr. 3	10	8	8	31	13	9
Apr. 10	7	5	4	23	10	7
Apr. 17	11	9	2	39	17	16
Apr. 24	9	5	3	35	16	5
**				31	20	
	110	64	37	268	125	77

** Note: Eggs from Pen 4, my yard gave this hatch, hence, not comparable to the others.

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Genes of Mice

THE LABORATORY MOUSE, *by* C. W. KEELER. Pp. viii + 81. Harvard Univ. Press. Cambridge, Mass. 1931.

IN an interesting booklet of some seventy-five pages, Dr. Keeler presents the main facts of the genetics of the domestic mouse, *Mus musculus.* The author sets himself the task of presenting his data in "a usable form" and succeeds admirably in so doing.

A discussion of the geographical distribution of the house mouse brings out the difficulty in obtaining any degree of isolation approaching completeness. A chapter on the history of the fancy mouse is of unusual popular interest. It will be a surprise to many to learn that a polychrome pottery image of one of the color varieties of domesticated mice dating from approximately 2000 B. C. was discovered in Egypt. The possession of fancy mice was widespread by 300 B. C. A list of the more important mendelian unit characters of mice shows that out of eighteen, eight have been first recorded since 1900. The earliest character, dominant spotting, traces back to 1100 B. C. The most recent listed is "dwarf," described by Snell in 1929.

The third chapter provides a more complete discussion of the various unit characters and concludes with a useful list of 26 varieties in each of which the fancier's terminology, scientific terminology, and genetic formulae are compared.

This is followed by a brief discussion of "normal" and "abnormal" inheritance in turn. By "normal inheritance" he includes, normal overlaps in which there is marked discrepancy between the genetic constitution of an individual and its somatic appearance; dominant lethals in which one of the two types of homozygotes in a given mendelian combination die; and hereditary sterility in the two types of hairless mice.

The final chapter is on the breeding of mice in laboratories. It gives a brief résumèé of the more important physiological facts of the life history and breeding habits of mice. This is followed by a discussion of the more common diseases and parasites and by directions for food and housing. Twenty-four photographic figures and a bibliography of 184 references bring the volume to a close.

In such a brief and popular publication it is, of course, natural that many subjects cannot be included. It would seem, however, that in future editions consideration might be given to the interesting isolated types, M. *muralis* and *M. musculus faeroensis* which occur in the confined areas of the Island of St. Kilda and of the Faeroe Islands, respectively. Similarly, the dominant lethal, short tail, described by Lang in 1912, and later by Dobrovolskaia-Zavadskaia, is an additional unit character to be listed.

The booklet is attractively prepared and is likely to have a well-deserved wide distribution.

C. C. LITTLE.

The

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COMING

A "Wingless" Guinea Fowl

The "Penguin guinea" would perhaps be a better name, as flight feathers rather than bones are affected. Caused by a gene mutation.

A One Celled Cow

Photographs of the first bovine egg to be discovered, and an attempt to visualize the small on a vast scale which characterises the single-cell phase in the perpetuation of life.

The Need of Naturalists

David Starr Jordan as an example of an essential and too rare a type.

Wooly-Haired Nordics

A dominant mutation, producing in a blond race a frizzle-haired condition characterizing the negro races.

Observations on Teaching Eugenics

"Why not let the public in on our secrets?"

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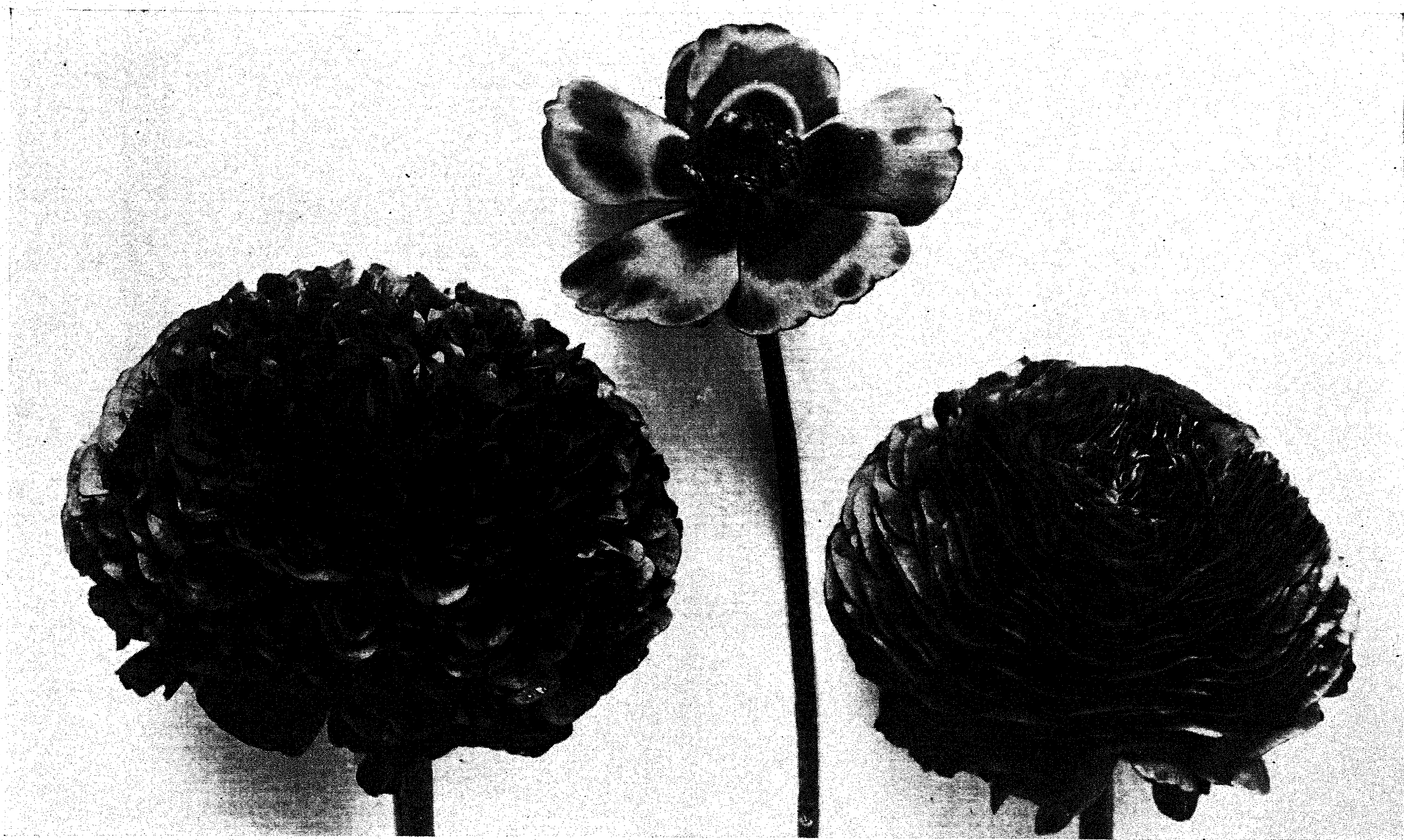

DOUBLE BUTTERCUPS FROM ELEVEN YEARS OF SELECTION

Frontispiece

In the center is shown the single-flowered wild form of Ranunculus from Palestine. The double forms below it have resulted from eighteen years of selection. The number of petals was progressively increased from one supernumerary petal until the present form has as many as 257 petals. At the same time fertility has greatly decreased. Still further improvement in size and color of flowers has been noted since this photograph was made. (See *A New Double Ranunculus,* Page 157.)

PARETO'S "RESIDUES"

Reason and Emotion in Human Conduct

G. H. Bousquet,
University of Algiers, Algeria

Extracts from *Précis de Sociologie d'après Vilfredo Pareto*

(*Freely translated from the French by* Marian H. Bell Fairchild)

Editor's Preface:—In a narrow sense it might be held that this epitome of Pareto's views on reason and emotion in human affairs has little to do with the subject covered by this Journal. Nevertheless, it does have great significance in the study of heredity, and for the following reasons:

First, it emphasizes the amazing persistence of the emotional characteristics that are a part of the hereditary equipment of every human being.

The other point is that in this "scientific" age it is increasingly evident that many of our difficulties lie in not applying in any adequate way the knowledge we have. This is especially true of biological knowledge. The obvious answer is that if we are to progress in the utilization of what science has to teach, it may be necessary to take cognizance of Pareto's "Residues" in interpreting this knowledge. Is it not particularly in scientific fields that the influence of emotion has been ignored? We have based our presentation of scientific knowledge almost entirely on reason—and have been surprised that political and religious demagogues, pseudo-scientists and anti-scientists have been more successful in gaining acceptance of their views than we of ours. Might not a study of the emotions which need to manifest themselves by external actions be very valuable?

The skilful navigator does not ignore the currents of sea and air, but recognizes and allows for their inevitable effects. Where possible he lays his course so that forces he cannot alter will help him toward his goal. It may prove to be no easier to change a "residue" than to shift the Gulf Stream. Science, which is a systematized regard of *facts*, can no more ignore the one than the other.

EMOTION, not Science is what counts with man. We see this as soon as we consider all the activities of the human race. Whether it is a question of art, religion, morality, love of country or love of family, it is emotion that we always find: science plays an entirely subordinate rôle. It is incapable of stirring man to action as emotion does and must therefore be put far behind as a motive force.

In examining the facts we are led to classify man's actions, or rather, the forces which induce them, into two groups, logical ones and others, the immense importance of which have never before been brought to light, which we will call non-logical. It is with the non-logical actions only that we shall deal in this chapter. Within the main group we seem able, in certain particular cases, to recognize two categories of the non-logical action-inducing emotions: (*a*) a constant category, by far the more important, which corresponds to certain human instincts and emotions, and (*b*) another category which aspires to justify the former, and which is extremely variable. (*b*) satisfies man's need for reason and logic, his need of explanation, and it is sensitive to the most diverse influences, whereas (*a*) remains relatively constant.

Before proceeding further it will perhaps be well to designate these categories more conveniently than by letters of the alphabet. Since (*b*) is derived from (*a*) we have called it "derivation"; as for (*a*), our search has shown it to remain constant while other phenomena vary, and we shall

therefor call it "residue" in the future. But if the reader prefers to call it "(*a*)," or "manifestation of instinct," or "psychological constant" or use any other term he fancies, our theory would remain unchanged, for we are reasoning about facts and not about words.

The residues may be classified, so far as we now know, in six great classes, further divided into sub-heads.

Class I. Instinct of Combinations

A very powerful instinct impels us to combine certain things with certain acts, and this is, without doubt, one of the factors that has contributed the most to the advancement of our civilization, because it leads to positive results. The average man makes, or accepts combinations guided by analogies that are fanciful, puerile and absurd. The scholar, the technician, the engineer, the business man under the influence of the same instinct may perform logical actions, although this is not always the case. It would be interesting to study in detail how with practical men logical actions and non-logical ones can be united, because, generally speaking, we here separate by analysis elements which are really composite.

I-*a.*—*Combination in general.* This instinct explains, perhaps, how it is that man has been able to make important and useful discoveries among the great number of combinations instinctively noticed. Up to our own day endless combinations have been suggested as medicaments, not one of which has survived experimental evidence, but which this instinct impels us to accept and investigate. Pliny cites sixty-nine combinations for the cure of epilepsy. Cardinal Richelieu was treated for it with horse dung steeped in white wine. It may seem ridiculous to notice such stories when we are studying the form of great social developments, but the instinct of combinations is one of the most powerful forces determining the equilibrium of societies, and if it manifests itself in absurd phenomena that does not in the least diminish its importance.

As a special illustration of this instinct one might mention the urge to connect certain feelings or certain actions with certain days. This is one of the most fundamental instincts of the human race and has been so from antiquity down to our own day, both in public and in private life. Birthdays were celebrated of old as they are today. The 18th of June, 1855, the anniversary of the battle of Waterloo, was chosen as the day on which to send the French and English troops to the assault of Sabastopol (which, by the way, failed completely). Similarly the Bavarian counter-revolution of November, 1923, took place on the same day as the revolution of 1918. In the one case as in the other there was no logical reason for choosing the date, but the fact explains itself when we recognize in the actors in these events and in the public at large an instinct which tends to link certain acts with certain days, the same instinct which was at the base of the *religiosi dies* and the *dies nefasti* of the Romans.

I-*b.*—*Combination of things that are alike, or opposite.* A sentiment about contrasts is at the base of many deeds and words. At Marseilles, Petronius tells us, it used to be the custom to allow some particular individual to enjoy all the delights of life for one year, at the end of which time he was put to death in an ignominious way. Beart and Raville report something quite similar in ancient Mexico. There is no imitation in this; it is a manifestation of the same instinct to combine opposites.

In regard to resemblances the facts are equally obvious. In Theocritus and in Virgil we find a resemblance between the melting of a wax figure and a man burning with love. The resemblance undergoes further development by the practices of incubism, "explained" by the intervention of the devil or some other demon. The principle of homeopathy shows this same residue.

Rare things and exceptional occur-

rences. One rare thing is united to another by the instinct of combinations. Heroes and great men have a divine or extraordnary birth, this is found consistently through all ages in all literature; these legends, built up by residues, are afterwards explained by derivations. The death of an eminent personage is announced by prophecies; those predicting the death of Caesar are celebrated, nor were they lacking when Charlemagne died. Of the death of Christ we read in the *Gospel according to St. Matthew* "And, behold, the veil of the temple was rent in twain, from the top to the bottom; and the earth did quake and the rocks rent;".

It is even more singular that the biographers of Beethoven mention a terrible storm that broke out on the 26th of March, 1827, at his death. In this case the fact has been established, but why should it be chronicled if there did not exist in the minds of the authors and their readers an instinct suggesting a certain connection between the two events? These are some of the more unusual types of this sub-division.

Terrible things and events. Sallust writes: "There are those who say that Catiline, wishing to unite his fellow-conspirators by an oath, passed around a cup filled with wine and human blood." Whether this be true or false, the fact remains that here two terrible things are united; drinking human blood and conspiring against the Republic.

Other combinations instinctively believed in are that:

Happy conditions are associated with good things and unhappy ones with evil things, and

Things assimilated produce effects of a corresponding nature.

I-*c.*—*Mysterious powers of certain objects and of certain acts.* This residue is found in many magic rituals, in amulets, vows made on certain objects, ordeals, etc. It is also the major factor in the phenomena of taboo, being correlated with a feeling that particular objects and actions are invested with occult powers.

I-*d.*—*Mysterious potency in general.* As usual, the residue shows itself both in acts and words.

Zeller, in his *Histoire d'Allemagne,* reports that two bishops having made a vow over empty reliquaries did not consider themselves bound by it, thus showing that in their eyes the relics which should have been there possessed a mysterious power.

On the occasion of the baptism of the Crown Prince at Belgrade, in 1923, he was baptised with water from the Adriatic and from the principal rivers of the kingdom of Yugo-Slavia; in this there can be no reason drawn from experience. King Charles I caused the Great Seal to be removed from Parliament, "rumor was rife in London because the possession of the Great Seal seemed a necessary adjunt to legal government." Here the residue is extremely clear.

Names mysteriously united to things. This residue is so frequently met with that it could in itself be considered as a sub-head. Innumerable derivations regarding perfect numbers and their mysterious properties, from the followers of Pythagoras down to Auguste Comte are based on this residue.

Think of the prestige which in all times has been attached to diplomas, to honorary titles, university degrees, and so forth, a prestige which certainly is not derived from any experience indicating that the holders of such degrees are always more remarkable or more intelligent than other men.

I-*e.*—*The need to combine residues.* Man feels the need of uniting his residues because his mind is synthetic; analysis only comes through a special effort of which few are capable.

I-*f.*—*Need of logical development.* Men are equally well satisfied by rigorous logic or by pseudo-logic. At heart they want to reason, whether well or ill matters little. We have only to notice the fantastic discussions that have taken place, and still do, about such incomprehensible subjects as the various theologies and metaphysics, the divergation about man's end, etc., to get

some notion of the insistent need which is satisfied by these productions.

I-*g*.—*Faith in the efficacy of combinations.* In general the ignorant man is guided by a pre-conceived faith in the efficacy of non-logical conclusions, reinforced by the fact that other combinations really are efficacious, but this faith arises spontaneously, as one can see in a child who amuses himself by trying the strangest combinations. Cato the Elder propounds with the same faith, magic remedies and agricultural operations. This faith antidates experience.

The belief that *A* must necessarily be united to *B* which characterizes the residues of the first class is reinforced, becomes stable and constant, thanks to the residues of the persistence of the aggregates which we shall study in the second class.

Class II. Persistence of the Aggregates

As we have seen, there is an instinctive propensity in the human mind to combine certain objects, certain acts, certain emotions, either within the same group or between different groups. Some of these combinations form very powerful and persistent emotional aggregates.

When the aggregate is once formed an instinct to oppose any separation of the units thus combined is often aroused. Hence the great sociological importance of residues of the second class. It must be clearly understood that even if the aggregate has no objective existence it may have a subjective one that is very important. Let us suppose that a certain people have made a divinity of a river; this fact could be explained in many ways. First, we might say that from the tangible river they have abstracted an ideal river which they regard as a "natural force" and which they adore as such. Second, we might say that, attributing a human resemblance to the river, they have endowed it with a human soul and that this soul has been deified. Third, we might say that the river has produced vague but very powerful emotions in them and that these emotions persist and constitute *subjectively* a whole to which they give a name, as they do to so many other subjective things that are remarkable. Eventually this entity takes the same place in the Pantheon of the people in question that the flag does in the political aggregate (witness the German Rhine), or, more modestly, is found in the baggage of the poets. The third interpretation explains several facts not covered by the other two and the residue to which it corresponds is therefore much more usual.

An added proof of these assertions is apparent in our own time. The cult of the Unknown Soldier as it is practiced today, among several nations has its origin in very profound emotions that were stirred during the Great War. The literature which seeks to explain this cult by exalting it, is another manifestation of these residues and is not the cause of the cult. These very interesting phenomena show most vividly what are the causes dominating the birth and development of a religion. It is to be noted that in France these emotions have been so strong that alongside this cult another one has grown up, that of the Eternal Flame, which has acquired the significance of a new rite. Here again there is no metaphysical theory in its origin, but it shows the persistence of the aggregate of strong emotions which makes up the patriotism of France, just as the cult of Vesta, with fairly analagous ceremonies, expressed the emotion of the Roman people for their city. As usual the theologies, the *derivations,* differ—exalting in the one case the virtues of ancestors, the power of the protecting gods of Rome, etc., in the other, Liberty, Justice, the Rights of Man, French Blood, etc. . . . . We shall fail entirely to understand these phenomena if we go no further than the literature, which no more than indicates the forces determining social equilibrium.

When we understand the instinct to preserve aggregates we can see why changes are more easily made in the form than in the substance, in the de-

rivations than in the residues.

Briefly, we might say that the farther religious and other practices of the same nature are removed from the residue of a simple association of ideas and acts, and the greater the proportion of theological, metaphysical, logical concepts (derivations) they include the easier it becomes to change them. And so we see why it is easier to change the form of government of a people than its religion, its usages, its customs and its language.

As a matter of fact, the residues of the persistence of aggregates correspond to "religious" feeling in the broadest sense.

II-*a.—Persistence of the relations of the individual with other men and with his surroundings.* These residues form the substratum of family feeling, of patriotism, of sentiment for the mother-tongue, for religion and friends.

If we look at things superficially we might believe that modern patriotism is only concerned with the territory of one's own country. In reality, to arouse the emotion of patriotism the name of the country must suggest an aggregate of emotions of race, language, religion, history, and so on believed to be held in common.

Patriotism cannot be defined with precision, any more than can religion, morality, justice, the good, or the beautiful. All these names evoke a complex of intricate emotions, vague and undefined. These wholes are kept together by the residues of the persistence of the aggregates.

Social class relations. The fact of living in a given community implants certain ideas in the mind, certain ways of thought and action, certain prejudices, certain beliefs which persist and acquire a psuedo-objective existence as do so many other analagous entities. As an extreme example, the associations known as sects, built up by strong and exclusive emotions, have always exhibited well-recognized characteristics. The very general idea current among barbarians that against strangers or enemies anything is permissable, that in dealing with them the moral rules in force among fellow-citizens need not be applied, is another instance of the residues we are discussing. This notion which existed among a people so civilized as the Romans, has not entirely disappeared in our own day.

II b.—*Persistence of relations between the living and the dead.* The aggregate of the relations of one man with other men persists by abstraction even during the absence or after the death of this man. This forms the residue of a great number of phenomena. Partly analagous to the residues of the relations of the individual with other men and with his surroundings, it is almost always found associated with them in the concrete phenomenon. Associated with certain residues of the fourth class, (in relation to sociality), it is at the base of the complex phenomena of the cult of the dead, of the feasts and sacrifices in their honor, and so on. We try to explain these beliefs logically, deducing them from the idea of the immortality of the soul, but this idea is only the derivation; among innumerable proofs that might be cited it will be sufficient to say that in London and in Paris there are cemeteries for dogs, and these animals are buried there by people who most assuredly do not believe that dogs have immortal souls.

On serious reflection we see that the idea of survival after death is the prolongation of another very powerful idea, that of the unity, or integrity, of man during the course of his life. Although physically and psychologically he undergoes ceaseless transformations we nevertheless assume that within him there is an abiding unity.

II-*c.—Persistence of the relations between the dead and the things that belonged to them during their life.* This belief gives rise to the very general custom of burying or burning these objects with the corpse, or of destroying them, of killing his slaves, his wives, his animals. As usual a logical explanation of these customs has not been lacking, they have been considered

as a necessary consequence of a new life for the dead, a life in which his needs should be satisfied. These beliefs undoubtedly exist, but they are derivations, that is to say they are essentially variables, whereas the constant factor in the phenomena is the idea of the persistence of the relationship of the dead with his belongings.

II-*d.—Persistence of an abstraction.* When a complex of intricate relationships of the kind we have been studying has once been constituted, a corresponding abstraction may arise, and if it persists a new subjective entity is created. These residues are the foundation of theologies and metaphysics, which we might define as an aggregate of derivations from such residues.

II-*e.—Persistence of uniformities.* A general character is often imputed to a particular uniformity, or even to a single, unique, isolated fact, and from that beginning we proceed to reason. It is useless to cite examples, the reader can find all he wants for himself.

II-*f.—Emotions transformed into an objective reality.* There is such an infinite number of these residues that they are rarely lacking in any discussion that is not rigorously scientific. The "auto-observation" of the metaphysician, the "experience" of the Christian and similar conditions transform emotions into objective realities.

II-*g.—Personifications.* The residues by which personifications are built up are of this class; at first a name is given to an abstraction, to a uniformity, to an emotion, thereby converting it into a subjective entity; little by little it becomes an anthropomorphism. Objects and places can also be personified without necessarily deifying them. The sentiment in regard to Rome has followed an undulatory movement beginning with a simple personification, then growing in intensity until it became a deification and finally subsiding into the poetic and literary admiration which still persists.

II-*h.—Need of continual abstractions.* When certain abstractions fall into disuse for any reason whatsoever there must be new ones to take their places, since the need for them still persists. Popular mythologies are replaced among the cultivated classes by subtle and sophisticated ones. Ingenious theologies arise, researches into cosmologies and the primitive state of man. Proceeding further along the line of supernatural abstractions we arrive at metaphysical abstractions on the essence of things, and speculations couched in incomprehensible language about even more incomprehensible things. Finally pseudo-scientific abstractions are added, such as evolution, whether creative or otherwise makes little difference. They manifest the continual need for abstractions.

In regard to residues of the present class there are several interesting things to notice:

Like the instinct to make combinations, these emotions have always been as much a part of human nature as the organs of the body, and expressions of them are found in the most remote epochs of history and pre-history; what have varied since have been simply the explanations, the derivations.

The analogy between religion, in the strict sense, and phenomena of social life with the same residual bases, is beginning to be admitted in enlightened religious circles as well as by erudite "materialists." To prove this we may turn to the authority of Mr. Gillouin, who is all the more above reproach because he is a convinced Bergsonian; "It is a fact no less striking," he writes, "that outside Christianity we see a rank growth of all sorts of ways of living that can only be called religious. It seems as though the less Christian our time becomes the more it becomes religious," and he cites the domain of art and that of politics: "Democracy began by being mystic . . . . . , Quinet in fact appeals to Christianity in behalf of democracy and stands forth as the prophet of a truer and more authentic Christianity than that of the churches . . . . . . How can we fail to be impressed by the truly religious fervor which is aroused in many souls by

the Socialistic gospel? Nationalism can also proclaim its own divinity . . . . ."

The author has described under the form of derivations what is true of residues, but he is a good observer and confirms our theories point by point.

That these sentiments are not only analogous, but that genetically they are all transformations of a primordial emotion, filial affection, is the very interesting thesis of a psychologist, M. Bovet, from whom we may quote some interesting passages; "We are in the habit of speaking of a child's adoration of its mother; the word has a deeper significance than is at first apparent, the child ascribes the same perfection to its parents as it does to God. . . . . . Later the same faith is projected further; among primitive races to the chief of the tribe, elsewhere to the monarch, as in the deification of the Roman emperors. Patriotic sentiment partakes of this filial love. Throughout history we see a close connection between patriotism and religion. . . . . . These two emotions can become completely identical."

Class III. Need of Manifesting Sentiments by External Actions

Powerful emotion is generally accompanied by action of some kind, usually having no direct relation to the emotion, not its logical consequence, but satisfying the need to act. A dog satisfies the same need when he wags his tail on seeing his master. As usual, to understand this behavior we must consider the residue, not the manifestation itself, and above all not the logical explanation of it, the derivation.

On entering a church, a synagogue, a mosque, one shows respect by uncovering the head, by covering the head, by taking off one's shoes. The essential element is the need to express respect, the accessory element is the method employed, the negligible element is the theological derivation which justifies it afterwards. This also explains why during the hey-day of anticlericalism, free thinkers copied Catholic ritual, having lay baptisms, burials to the tolling of a bell, even the idea of a lay first communion was seriously discussed, and today socialists are full of enthusiasm for ceremonies of an undeniably militaristic character.

Religious exaltation. Religious enthusiasm carried to the point of delirium is found among all peoples at all epochs. To particularize, the description given by Williams of what he saw in Fiji, and Henri Bois's description of "revivals" in Wales, are almost identical, in spite of the fact that the derivations had nothing whatever in common.

Class IV. Residues In Relation to Sociality

Private associations. Most people feel the need of some form of exclusive association, though it may vary greatly as to kind. Let us see what residues are generally to be observed. Remarking on the spread of funeral colleges in Rome, Renan notes that their members became closely united and like relatives. This indicates what powerful emotions originate from associations of this sort. Renan found this same feeling among the Oriental Christians of his own time. Moreover they differ little from those we can easily see for ourselves among religious sects, political associations and secret societies. For the most part the activities of the members have been the same and include banquets; it is one of the very numerous cases in which we see the form change while the foundation remains the same. The derivations change, the residues persist.

Need of uniformity. This varies among different peoples and presents various sub-classes.

IV-*a.*—*Uniformity obtained by making oneself do certain things.* Imitation should be mentioned here; it plays an important rôle in social life. It shows itself in simplest form in the uniformity imposed by fashion. It is often impossible to discover that it serves any useful purpose whatever. To say that "one follows the fashion to be like the rest of the world" is only to say that "one imitates because one imitates."

IV-*b.*—*Uniformity imposed on others.* This is one of the most universal urges

of social life, although its manifestations seem to be extremely variable. We see it with great clearness when we study history or observe what is going on among neighboring peoples, because when personal feeling does not blind us we can appreciate the non-logical characteristics of an act. In the time of Louis XIV it was unthinkable that subjects should have any other faith than that of the monarch, but the Great King was not in the least disturbed by the fact that in his various provinces there was an almost chaotic diversity of administrative form. Today all the world, or almost all, approves of liberty of conscience, but there are statesmen who have insomnia at the mere idea that Alsace Lorraine is not in all respects governed by the same institutions as the rest of France and who consider that this is wrong "in itself." As usual this is explained, by pleading the "necessity of a return to the fold of our holy Mother Church" or to "la France eternelle," while as a matter of fact it is just the other way around and, as always, the residue was there before the derivation.

IV-*c.*—*Neophobias.* This emotion hinders any innovations that threaten uniformity. It is very strong among primitive peoples and is noticeable in our own day, being only overcome by the instinct of combinations. In Paris, in 1911, women appearing in divided skirts were mobbed on the streets. The same thing happened in connection with big hats or other innovations of fashion. It should be noticed that among persons affected by these neophobias there are many to whom on the other hand anything new in politics or in religion must be good for the sole reason that it is new. Horrified at the innovations of tailors and milliners, they are indignant because the Pope will not accept the innovations of the *Modernists,* and they blame the government for any delay in accepting social innovation . *Contradictory residues can exist together in the mind of the same man.*

IV-*d.*—*Pity and cruelty.* These feelings belong in this class, but for want of space we shall simply mention them in passing.

IV-*e.*—*Tendency to impose hardships on oneself for the good of others.* These are the feelings of altruism which social life engenders and which impel man to share his belongings with his fellows, or even to sacrifice himself for them .

IV-*f.*—*The feeling of hierarchy.* This is felt as much by those who are the inferiors as by the superiors. Observable even among animals, it is very widespread in human societies. It even seems as though simple societies could not exist without these distinctions of class. The hierarchy may become transformed, but the sentiment persists, even in social groups which in outward appearance proclaim the equality of its individuals. Thus to understand the mechanism of social life it is far more important to give attention to these residues than to the derivations from them. To imagine that the ancient feudal system of Europe was imposed solely by force is absurd; it was partly maintained by those mutual feelings of affection which are also observable in other countries having a feudal system, as for example in Japan. The sentiments on the part of the superiors are sentiments of protection and benevolence, mingled with those of domination and pride; on the part of the inferiors they are those of subjection, of affection, respect and fear.

IV-*g.*—*Need of having the approbation of the collectivity.* The desire which every individual feels to be well thought of in the community represents a very powerful emotional force and is the real foundation of human society. It operates tacitly however, often without being expressed, and the individual himself is sometimes unconscious of it.

IV-*h.*—*Asceticism.* In man we see a special type of emotions not found among animals, which makes a person inflict suffering upon himself for no utilitarian reason whatever. This is the kernel of the phenomenon of *asceticism.* If we were familiar with only one class of manifestations, the asceticism of the

Roman Catholic church for example, it would be difficult to distinguish the residue from the derivation. A man does penance because he believes it pleases God and because he wants to make honorable amends for his sins. This religious feeling might be the residue and the penance the consequence of this residue. In fact, in certain instances it seems as though this were the case. But there are ascetics, like the ancient Cynics, devoid of all religious ideas whatever. We see the Spartans, ascetics solely for the purpose of maintaining a strict discipline; Buddhists, who want to curb all vital energy; and finally, among our contemporaries, we have people who make ascetics of themselves in the name of holy *Science,* which, they say, condemns the use of alcoholic beverages.

It is therefore clear that the constant element is to be found in the sufferings men choose to inflict upon themselves, the variable element lies in the motives they have, or claim they have, for so doing.

Class V. Integrity of the Individual and of His Dependents and Possessions

V-*a.—Emotions which oppose any alteration of the social equilibrium.* The individual sets up certain ideals regarding his relationship with other men and with community life as a whole. When these ideals are interfered with he experiences unpleasant emotions and tries in various ways to re-establish the equilibrium. Such emotions are, as usual, vaguely defined; in this case they are called feelings of "justice" and "injustice." The man who says "This thing is unjust" means that it offends his sensibilities, those pertaining to the equilibrium of the society in which he lives. Where a certain kind of property exists, it is "unjust" to take it away from a man, where it does not exist, it is "unjust" to give it to him. Soldiers who share the booty taken from an enemy call "unjust" any alteration of the rules usually followed in such distributions.

The residue we are examining gives rise to an observation of great importance. Imagine a collectivity where homicide is becoming frequent; evidently it is in process of disintergration. To counteract this dissolution it is not necessary that the present residue should operate, the immediate interest, the instinct of self-preservation would be enough. But long before there is a probability of any particular individual being assaulted, the feeling that one should resist any disturbance of the existing social balance becomes operative.

We can see why the residues of this class, combined with those of the persistence of the aggregates make emotional wholes of great social importance, designated vaguely as "ideals of justice."

It is noticeable that actions designed to prevent this kind of alteration are usually non-logical ones, as the following example will typify: In March, 1786, two members of the States General of Holland wanted to drive through a certain gate at The Hague, a privilege until that time reserved exclusively for the Stadholder, but which privilege had just been taken from him by the States who were in open conflict with him, to show their sovereignty. The common people of The Hague upheld the rights of the Stadholder and made a demonstration. The different residues are clear: the instinct of combinations, uniting in some mysterious way the governing power to the privilege of a special gate, those of the persistence of aggregates, making this association persist even in the absence of the titulary head, those concerning the integrity of the individual, opposing any alteration of the disturbed equilibrium, and lastly the need to express action, manifesting itself here in an illogical way, as the trouble makers, who had no concerted plan, were severely punished.

V-*b.—Feelings of equality on the part of inferiors.* These feelings often help an individual of an inferior class to maintain the sense of his own integrity and they tend to lift him into a higher class.

V-*c.—Re-establishment of integrity by operations relating either to those who*

have suffered changes or to the disturbing agent. Under this heading come the purifications so customary in the social groups of antiquity, giving rise as ever to innumerable derivations. In our day these have not disappeared; quite the contrary. Protestants have put up an expiatory monument to Servet, burned at Geneva by Calvin's command: an analagous feeling impelled the ancient Greeks to purify themselves when they had committed even an unintentional murder; notice the residue of the persistence of an aggregate, by virtue of which it is believed that an integrity altered in the 16th century remains so for three and a half centuries, exactly as the vengeance of the Gods of Greece could be visited on descendants to the third and fourth generation.

In concrete phenomena of this group we generally find the following elements: first, there are the residues of the persistence of the aggregates, which enable one to believe that an abstract or imaginary subject is real; second, it is necessary that there should be some real or imaginary happening which makes one believe or suppose that the integrity of a given subject has been attacked; third, the residue to re-establish integrity comes into play to initiate action to compensate for the attack sustained; fourth, the residues opposed to any alteration of the social equilibrium are added. In 1923 Or, a French novelist, was dropped from the Legion of Honor for having written what was considered to be an "immoral" book. Let us see how this corresponds with the theory just stated. First, the Legion of Honor is not a tangible entity whose integrity can be altered, but the emotions which its members and a part of the public feel regarding it are transformed into an objective reality; second, this integrity was felt to be injured by the writing in question; third and fourth, integrity was considered restored and the social order strengthened by the expulsion of the author. If we admit that in this case it is not a question either of conscious vengeance on the part of his enemies, or of advertisement desired by the novelist himself, the non-logical character of the act is plain, because the only tangible result—any re-establishment of the integrity of an imaginary entity being purely supernatural—was that editions of the book were multiplied in France and abroad. It is the sort of thing that always happens when the residue of sex is aroused.

Class VI. Sexual Residue

This is at the base of innumerable verbal and other expressions of man. Literary works in which it does not appear are excessively rare. It is present in most amusements and diversions of a public character. It is even found in works written for the express purpose of preaching chastity and the repression of manifestations of the sex instinct; under this form it has engendered in our day a sort of "virtuist religion," with taboos and interdictions, defended by people who often say that they are free of all prejudice, while in reality their intolerance is that of all fanatics.

The Sociology of Vilfredo Pareto, translated by Professors J. Harvey Rogers of Yale, and Andrew Bongiorno of Oberlin, will appear in the near future. It is to be published by Harcourt, Brace and Co. The Précis, prepared by Professor Bousquet, from which we print an exerpt in this number of the Journal was published by Payot, in Paris, in 1925. The extract here printed will give some idea of "the wealth of collected material, the genius shown in its interpretation, the rigorous application of the method of logico-experimental research" which makes Pareto's Sociology one of the great books of the century.

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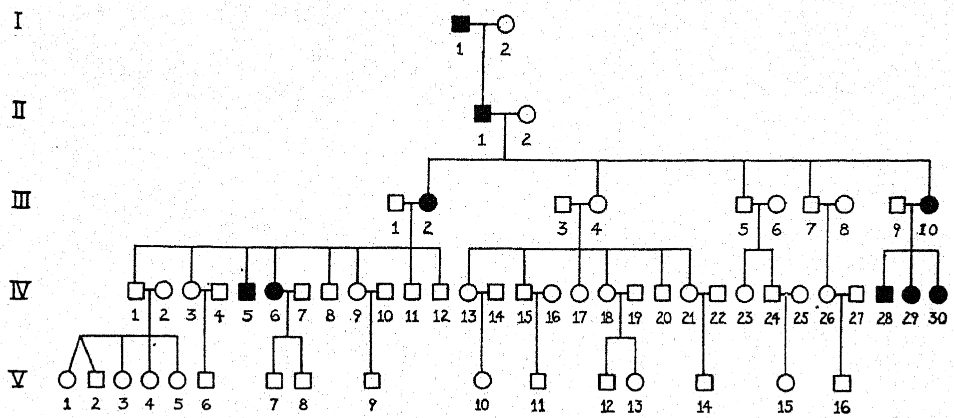
Besides numerous articles, Dr. Bousquet has published the following books:

L'Evolution Sociale aux Pays-Bas, Rivière, Paris; *Précis de Sociologie d'après V. Pareto,* Payot, Paris; *La Restauration Monetaire et Financière de L'Autriche,* Riviére, Paris; *Essai sur l'Evolution de la Pensée Economique,* Giard, Paris; *Introduction aux "Systèmes Socialistes" de Pareto,* and *Introduction au "Manuel" de Pareto.* Giard, Paris; *V. Pareto, Sa Vie et Son Oeuvre,* Payot, Paris; *Cours d'Economie Pure,* Rivière, Paris; *The Work of Vilfredo Pareto.* Sociological Press, Hanover, N. H; *Vol. I. Institutes des Science Economiques,* Giard, Paris.

INHERITANCE OF RINGED HAIR

George D. Snell
Brown University
and
Frank Foley
Princeton University



FOUR GENERATIONS OF RINGED HAIR

Figure 1

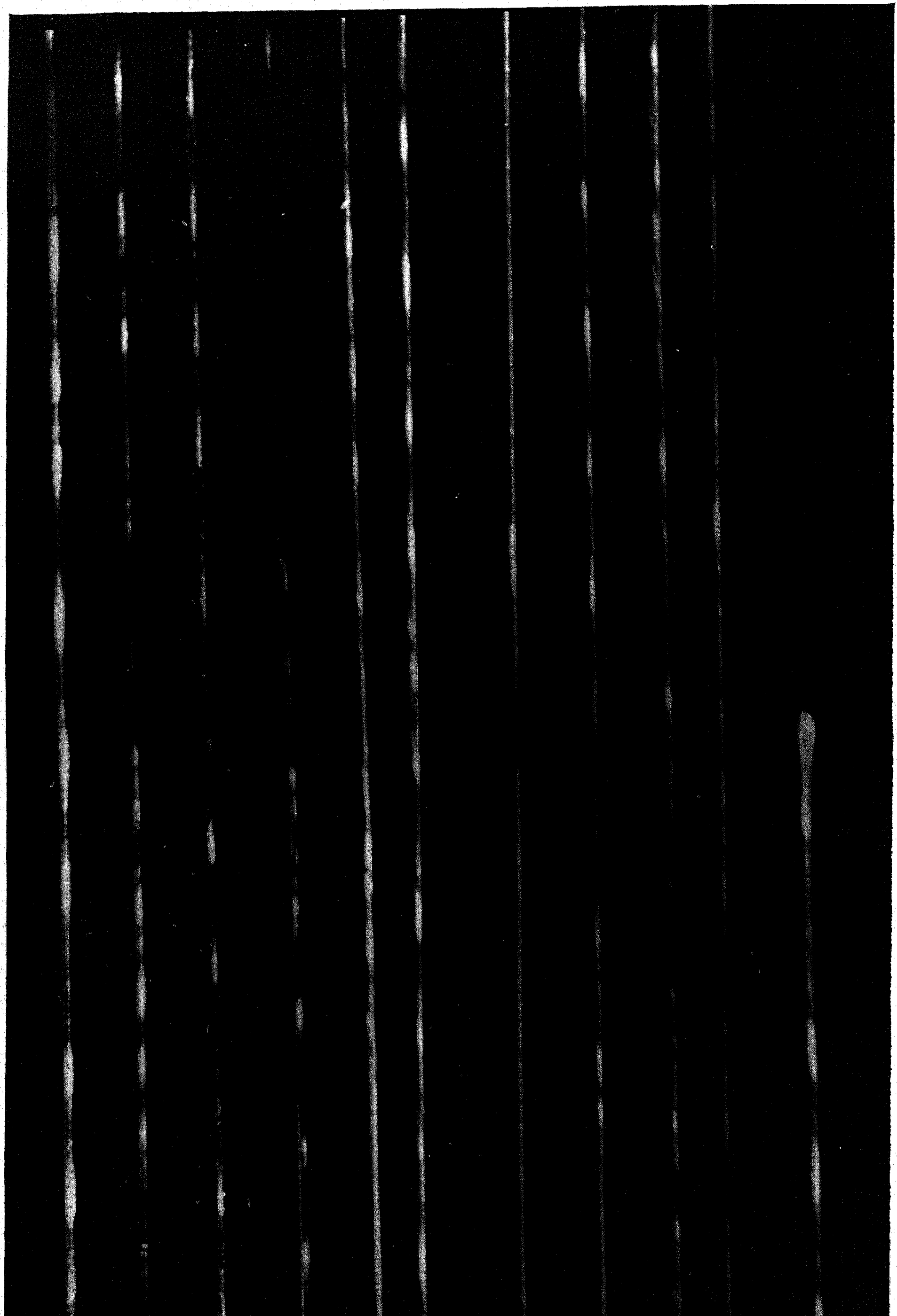
Inheritance chart of ringed hair, showing that this character is inherited as a Mendelian dominant. The "rings" are alternate lighter and darker bands, as shown in Figure 2.

A NUMBER of authors have reported cases of ringed hair, an hereditary marking of the head hair in man. The most thorough study has been made by Cady and Trotter* who have published three pedigrees and a full description of the character. A new pedigree showing transmission through four generations is illustrated in the accompanying chart (Figure 1).

The trait is usually not a conspicuous one, the first impression on observing the hair of an affected individual being merely that it is somewhat sandy and speckled. If single hairs are examined, however, most of them are found to show distinct bands of alternating lighter and darker shade. The width of the dark bands is about 0.5 mm., that of the light bands usually somewhat less. The rings are present only in hairs from the head; hair from other parts of the body is entirely normal. According to Cady and Trotter the light areas are caused by the presence of gas in the interstices of the medulla and cortex. As evidence of this they cite the fact that when seen under the microscope with transmitted illumination the ordinarily light areas appear dark and the dark areas light. This is because the gas filled regions refract the rays of light so that they do not enter the microscope. If reflected instead of transmitted illumination is used the bands appear under the microscope just as they do to the unaided eye.

The classification as affected or not affected of most of the persons figured

*Cady, D. D., and Mildred Trotter. A study of ringed hair. *Arch. of Derm. and Syph.* 6:301-317. 1922.



RINGED HAIR

Figure 2

Enlarged photograph (about 20 times) of the "ringed" hairs. The light areas are caused by the presence of gas in the interstices of the medulla and cortex. Not all the head hairs are affected in this way, and the body-hairs are not ringed. One of the hairs near the center of the picture is normal except for a single light spot. The hairs are actually cylindrical in outline, the greater breadth of the light areas being due to halation of the photographic negative. At the right is shown the base of one of the hairs.

in the chart is based on reports by members of the family. Samples of hair from two individuals, however, III-2 and IV-28, were microscopically examined by the writers. In the case of III-2 slightly less than one-third of the hairs show the ringing, but in some of these it is quite pronounced. A somewhat larger proportion of hairs are ringed in the sample from IV-28. It is possible that as some of the individuals in generation V grow older the rings may appear in their hair, but this seems unlikely, as in the case of V-28, 29, and 30 they are known to have appeared at an early age.

This pedigree, like those reported by Cady and Trotter, indicates the character to be inherited as a Mendelian dominant. According to Mendelian theory half the offspring, on the average, of parents of whom one shows ringed hair should themselves show ringed hair. In this case the ratio is 7 with ringed hair to 11 with normal hair, a satisfactory approximation to the expected ratio of 1 to 1. II-1 is omitted from the total as information is lacking concerning several brothers and sisters who consequently have been omitted from the chart.

A NEW DOUBLE RANUNCULUS

Produced by Selective Cultivation

Dr. Attilio Ragionieri
Castello, Italy

IT is common knowledge that the passage of a wild plant into cultivation is a cause of morphologic and physiologic variations in its organic structure. These variations appear more or less promptly and easily produce new forms of plants. The breeder often profits by this phenomenon for his designs.

One of the first organs of the plant to be affected is the flower, and it sometimes happens that its constitution may be deeply affected and its power of reproduction much reduced, as in the case of double flowers. A fine example of doubling produced by means of selective cultivation is given by the Ranunculus flowers figured in the accompanying photograph (Frontispiece). The following is their history:

In 1911 I had from the kindness of Mr. Herb, nurseryman at Naples, a few roots of a wild single flowered scarlet Ranunculus he had received from the Holy Land. I grew these roots carefully with plenty of food and space. In the second year of cultivation I noted a flower with a supernumerary petal and this fact suggested to me the opportunity of helping the tendency to doubling. Therefore I removed all the other flowers produced by this plant, with the object of drawing the maximum of nourishment into the mutant, and I carefully gathered the seeds. I had from these many plants showing some supernumerary petals, and persevering with this method after a few years a collection of semi-double flowers had been produced. Full double flowers came only in the tenth year.

In the photograph the upper flower is the single wild form. The lower are improved double varieties of the 1929 cultivation. In 1930 the improvement advanced remarkably and much more in the last blooming (1931) when I have measured a flower 15 centimeters across carrying 257 petals.

This new race of Ranunculus must be not confounded with the old strain known under the name of Roselline di Firenze, or Giant florentine Ranunculus, that has been grown and selected by many members of my family since the year 1790, and by myself

since the year 1872, and which therefore is named Ragionieri's strain.

My new race in which the reds of all shades predominate and the other colours are more lively than in the old one will be given the name of Ranuncoli di Castello (Ranunculus of Castello).

Chaplain and Biologist

BIOLOGY AND MANKIND, *by* S. A. McDowall, B. D., Senior Chaplain and Senior Science Master at Winchester College. Pp. 229. 9 Chapters. 46 Illustrations. Price, $2.50. New York, The MacMillan Co., 1931.

THERE are colleges, even in this so-called scientific age, in which the Chaplain and the Professor of Biology never speak cordially to one another. At Winchester College they speak with the same voice, this apocalyptic situation being brought about by the fact that our author serves there both as Chaplain and Senior Science Master. *Biology and Mankind* is an attempt to present briefly some evidences and theories of evolution, and to follow this with a rather more complete discussion of Mendelian heredity. Then the author takes up, in a "frankly propagandist" manner, the social implications of human inheritance.

The book is a somewhat abridged compilation of the data presented in one term of a course in general biology for classical students. Since it involves a study of regression, the effects of single and multiple factors, linkage, sex-linkage, lethal factors, and the cytological basis of heredity, besides the by no means stinted material upon evolution and eugenics already mentioned, the mental keenness of the young classical scholars must receive a thoroughgoing test.

The author has felt a little anxious concerning the rapid progression of the subject matter of his text, and has supplemented it with a glossary which is explanatory as well as definitive, and contains a full page illustration (Agar's meiosis in *Lepidosiren*). The other figures, illustrative largely of the discussion of evolution, are clear and appropriate. The frontispiece is a presumably original group of photographs showing the interesting convergence in skull characters to be found in a comparison of differently adapted placental mammals and their remarkably similar marsupial prototypes.

To appease old Argus and maintain traditions the reviewer must find an error, and he feels that the author has made one in supposing that sex-reversal must be accompanied by the "alteration of one sex chromosome." Breeding data make it probable that reversed animals merely put old chromosomes into new gametes.

The culminating portion of the book is given over to material upon human inheritance and the socio-political inferences which must come from observation of the present differential birth rate. The reviewer was, here, particularly impressed with the appropriateness of having the information presented by a clergyman. Biology has always interested churchmen. Indeed, the subject is heavily indebted to parson-naturalists—including Darwin—for outstanding contributions. These traditions, together with the possession of that virtue which suffereth long and is kind, makes the parson an ideal eugenist. Eugenic recommendations presented by scientists are resisted because they appear cold and inhuman, and, moreover, the scientists are still bogged down in the minutiae of their own data and are striking at one another wildly. Chaplain McDowall shows us how a well-informed padre can, in this contretemps, come over to Macedonia and help us.

R. R. Huestis.

University of Oregon

A PROBABLE CASE OF DOUBLE SUPERFETATION IN THE EWE

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"TRIPLETS" BORN SIX WEEKS APART

Figure 3

The four-year-old grade Shropshire ewe in which superfetation occurred. These lambs were in the uterus at the same time as embryos in different degrees of development. The large lamb was born May 5, 1931, and the twins 81 days later on July 25. The lambs are fully developed and apparently normal. Photographed July 27, 1931.

CASES of superfetation have been reported at various times among farm animals. In mares, cows, and sows, the instances have been rather frequent. In the ewe the cases reported are few. Winters† reports an occurrence in an ewe but in no case, as far as the author is aware, has a double superfetation been reported. A brief account of such an example follows.

A flock of 20 grade Shropshire ewes, owned by Mr. E. A. Hepler, Manhattan, Kansas, was allowed to run with the ram. This ram was a cross bred Shropshire × Hampshire. The ewes and ram were run together and breeding was promiscuous, consequently, the dates of service could not be ascertained.

During the course of lambing in the flock, the winter of 1930-31, the performance of a four-year-old ewe was noteworthy. A record of her lambing follows:

December 25, 1930—1 ewe lamb—8 pounds.

May 5, 1931—1 ewe lamb—8 pounds.

July 25, 1931—2 ewe lambs—6½ pounds each.

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*Published as Contribution No. 95 from the Department of Animal Husbandry.

† Winters, L. M. 1925. Animal Breeding, by John Wiley and Sons, Inc., New York.

All the lambs were normal. The ewe was normal in every way and had a good flow of milk after each lambing. Previous to the births noted above, the ewe had lambed three times, dropping three fine lambs. These births were normal, occuring in the normal season of each of the three preceding years.

An analysis of the births from December 25, 1930, to July 25, 1931, shows that three parturitions occurred in 211 days. The normal period of gestation in Shropshire ewes is about 147 days. The reported range in length of gestation periods for a number of normal births is 143 to 156 days. There were 131 days intervening between the births of December 25 and May 5. This is less than the normal period of gestation. This indicates the first case of superfetation and is particularly interesting because of the length of time between the births. However, there is a possibility that the May 5 lamb may have been slightly premature. But considering that the lamb was normal in weight, this is unlikely. Even if the May 5 birth was premature, it is unusual that the ewe accepted service so soon after parturition on December 25, for ewes after lambing do not usually come into heat and accept service until the autumn following parturition. It is possible these births may not constitute a case of superfetation, but at any rate two normal births occurred well within the time of a single gestation period.

The births of May 5 and July 25, give distinct evidence of superfetation. The July 25 twin lambs were dropped 81 days after the May 5 lamb. Again a large number of days intervened between the births. To produce the July 25 lambs, breeding must have taken place about February. This breeding date is out of season and occurred when the ewe was already in gestation with the May 5 lamb.

Whether or not the three births within a period of 211 days may be interpreted as an instance of double superfetation is questionable. At any rate, a single case is established in the May 5 and July 25 births and adds another example to the list of mammals in which the uterus contained at the same time, embryos of widely different degrees of development.

Heredity and Cancer

Most of the major problems of medicine that can be solved by the study of human material alone have been settled as far as available methods and materials permit. Most of our remaining problems apparently will have to be settled by work with animals, which furnish the essential possibility of control of observations and results, a feature difficult, often impossible, when human material must be used for our investigation. This is particularly true of cancer, whether we are seeking light on the cause or the treatment. Cancer is a disease of such protracted development and course, so variable in its manifestations and duration, often so difficult of diagnosis and differentiation, that satisfactory study of many fundamental problems on the basis of clinical observation is almost or quite impossible. At the present time it seems safe to maintain that the existence of an hereditary influence on the susceptibility and resistance to cancer has been established both for man and animals. The exact mechanism of the hereditary influence has yet to be determined. The evidence offered by human material is conflicting, and inadequate both in amount and character to permit of satisfactory analysis. The studies so far made with spontaneous tumors occurring in laboratory animals show the possibility of modifying the occurrence of cancer in these animals in marked degree, and of determining by experimental genetics the site and character of the tumors that will arise. Therefore, we may reasonably look forward to the eventual finding of a definite explanation of the genetic mechanism which determines susceptibility and resistance to human cancer.—Wells, H. Gideon: The Nature and Etiology of Cancer, *Am. J. Cancer* (supp.) 15:1919 (July) 1931.

DID SHAKESPEARE KNOW PLANT HYBRIDS?

W. E. PRAEGER
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THE credit of first recognizing sex in plants is usually ascribed to Camerarius in 1694, and the earliest use of this knowledge to produce a hybrid to Fairchild prior to 1719, when he used the pink or gillyvor, *Dianthus caryophyllus,* in his experiment. But there is a very interesting passage in Shakespeare's *A Winters Tale* that suggests the possibility that sex may have been known and hybridization practiced by some gardeners in England much earlier, the play having been brought out in 1611. This passage, in Act IV Scene III, is as follows:

Perdita. ——the fairest flowers
o' the season
Are our carnations, and streak'd gillyvors,
Which some call nature's bastards: of that
kind
Our rustic garden's barren; and I care not
To get slips of them.
Polixenes. Wherefore, gentle maiden,
Do you neglect them?
Per. For I have heard it said,
There is an art which, in their piedness,
shares
With great creating nature.
Pol. Say, there be;
Yet nature is made better by no mean,
But nature makes that mean: so, over that
art,
Which, you say, adds to nature, is an art
That nature makes. You see, sweet maid,
we marry
A gentler scion to the wildest stock;
And make conceive a bark of baser kind
By bud of nobler race: this is an art
Which does mend nature,—change it rather:
but
The art itself is nature.
Per. So it is.
Pol. Then make your garden rich in
gillyvors,
And do not call them bastards.
Per. I'll not put
The dibble in earth to set one slip of them:
No more than, were I painted, I would wish
This youth to say, "twere well; and only
therefore
Desire to breed by me.—

The quotation is difficult of scientific interpretation. Perhaps Shakespeare was thinking of grafting and nothing more. He may have had even more confused ideas of this art than had Virgil, who grafts apples on plane trees and pears on ashes. The implication of sex in plants and of "bastards" would then be mere fantasy. This would be an easy explanation and yet hardly satisfactory.

Shakespeare was often inaccurate in details. He depended much on what he read or heard and was quite uncritical in any scientific sense. In this play he speaks of the non-existent seacoast of Bohemia because he knew of seacoasts and had heard of Bohemia. Had he heard of the crossing of plants he might easily have confused it with grafting. Omitting the few lines that seem to refer to this old art, the whole passage would imply that both Perdita and Polixenes were familiar with hybrids and knew how they were produced. Gillyflors were not likely to be grafted. Where "slip" is used it is in the sense of cuttings, and does not mean scions, as elsewhere in Shakespeare, for they were planted with a dibble. Perdita's prejudice, which seems absurd to us, was that of the contemporaries of Fairchild, who 108 years later, considered the results of his work against nature and perhaps the whole process immoral.

Is it not possible that some English gardeners, as early as the sixteenth century, were using crossing to produce varieties of carnations? Gerarde in 1597 said of this flower—"that a great and large volume would not suffice to write of every one at large and in particular." It would be easier to conceive of this if deliberate hybridization had been practiced to produce the varieties. In 1702 John Ray catalogued 360 distinct kinds. In those days all that men knew did not, as with us, get quickly into print. The practice might have

been local, or, perhaps, the secret of the method guarded as has frequently been the case in the arts.

Does not this garden scene make it possible, or, indeed, probable, that sex in plants and the practice of hybridization were known to the contemporaries of Shakespeare?

BACON PREDICTED TRIUMPHS OF PLANT BREEDING

PROFESSOR PRAEGER'S interesting quotation from Shakespeare and the questions which he raised suggested that further light might be thrown on Seventeenth Century views concerning this subject by an examination of the writings of Sir Francis Bacon. In many places in Bacon's writings a deep interest in horticulture is evident: His essay on *Gardens* opens with the understanding phrase "The Lord Almighty first made a garden," and it shows a detailed knowledge of many plants. Thus we might expect to find in Bacon's scientific writings a fairly complete summary of the ideas about hybrids current among horticulturists at the time of Shakespeare. In this we are not disappointed: in his *Sylva Sylvarum; or a Natural History* we find in the many pages devoted to a mixed collection of observations, theories, and superstitions about plants, specific references to this matter. Much of the material there presented, he admits is absurd,—"But these are idle and ignorant conceits and forsake the true indications of the causes." He says elsewhere, "Let no one be concerned if this history has its errors." He considered the advancement of science more important than his reputation, which he felt would have been enhanced by the suppression of this work, which was published posthumously in 1627 shortly after his death (April 9, 1626).

His own interpretations of the observed facts are colored by current concepts, which included the constitution of all matter from the four elements, earth, fire, water, and air, and the spontaneous generation of at least the lower forms of life. Nevertheless, he observed widely and keenly, and his conclusions sometimes fit remarkably into modern interpretations. The practical nature of his experiments, and his skepticism of some of the wonders he records, is indicated in the following quaint statement: "Our experiments we take care to be, as we have often said, either 'experimenta fructifera,' or 'lucifera'; either of use, or of discovery for we hate impostures, and despise curiosities. Yet because we must apply ourselves somewhat to others we will set down some curiosities touching plants." ("Pure" science, in its extreme modern form, which scorns "fructifera" and sometimes neither bears, nor is intended to bear fruit, would appear to have little appeal to Bacon.)

It also seemed possible that another matter in relation to Bacon might be touched on. As was pointed out in this Journal in 1927,* the question of the authorship of Shakespeare's plays is of no little interest to eugenists. If the random Stratford boy, abandoning his wife and children at twenty, was able to absorb in the next ten years of a roving player's life the learning set forth in these plays, he was a unique phenomenon in the human race, more amazing than is generally considered. Robert Louis Stevenson railed at those who wished to roll Shakespeare and Bacon into one "To have a greater wonder to gape at," but he is mistaken in this The greater wonder would be the conventional Shakespeare. The authorship of the plays by a person with Bacon's breadth of interest and literary endowments, is much more explicable biologically.

It was further pointed out that the question of the authorship of the plays has been argued very largely on a purely literary basis. Style can be con-

*An Age of Genius, JOURNAL OF HEREDITY 13(8):343. 1927.

sciously altered, as several outstanding instances prove, but the *content of mind* behind the style remains the same. If what the founder of modern science thought and recorded illuminates what Shakespeare's characters have to say in biological matters we may consider it merely a coincidence, but it would suggest that the method has possibilities. The following quotations are very helpful in understanding the passage from *A Winter's Tale*. Bacon's views, though by his own admission he knew of no plant hybrids, are those of Polixenes, who saw in hybridization a natural process of great potential value to mankind.

Sex in Plants

The concept of plant hybrids involves as a corollary the idea that plants are sexual organisms. Writers on this subject have often pointed out the fact that sex in the date palm was known in very ancient times, centuries before Camerarius proved the sexual nature of pollen and stigma. Bacon considered sex in plants such an obvious matter that it might be considered axiomatic, though his views were confused by the acceptance of the doctrine of spontaneous generation. The following quotation shows that he unquestionably considered sex in plants as a literal and observed fact, even more literal than the reality:

> For the difference of sexes in plants they are oftentimes by name distinguished, as male-piony, female-piony, male-rosemary, female rose-mary, he-holly, she-holly, etc., but generation by copulation certainly extendeth not to plants. The nearest approach of it is between the he-palm and the she-palm, which, as they report, if they grow near, incline the one to the other, insomuch as that, which is more strange, they doubt not to report that to keep the trees upright from bending, they tie ropes or lines from the one to the other, that the contact might be enjoyed by the contact of a middle body. But this may be feigned, or at least amplified. Nevertheless I am apt enough to think, that this same binarium of a stronger and a weaker, like unto masculine and feminine, doth hold in all living bodies. It is confounded sometimes, as in some creatures of putrefaction, wherein no marks of distinction appear: and it is doubled sometimes, as in hermaphrodites: but generally there is a degree of strength in most species. (BACON, FRANCIS. 1627. *Sylva Sylvarum, or a Natural History.* ¶ 608.)

It is noteworthy that neither here nor elsewhere does he recognize the part played by pollen in the fertilization of the flowers. Evidently his information about the date palm, doubtless obtained from Classical sources (possibly from Virgil) did not include the fact that a branch of a male flower is tied to each female inflorescence to furnish an ample supply of pollen to insure fertilization. Doubtless the rope was considered to serve the purpose, as well as furnishing that vicarious thrill afforded by the "bark you love to touch."

Hybrids Predicted

A section of the "History" is devoted to the problem of the modification of species, but without including among them hybridization. A number of experiments are suggested for producing changes in the form of plants, and many of the views are colored by the existing concept of plants arising *de novo* through "putrifaction." Planting of mixed seed is believed to be a way to influence plants to resemble each other. Raising plants ordinarily found at sea level in an alpine environment is a suggested experiment only recently attempted. The reversion, or, as Bacon calls it, "degeneration" of seedlings of plants ordinarily propagated vegetatively is noted and discussed. The production of graft hybrids is also given extended consideration, and much space is devoted to a discussion of the effect of stock on scion, and *vice versa*. "—But these are but imagination and untrue. The cause is, for that the cion over-ruleth the stock quite, and the stock is but passive only, and giveth aliment, but no motion to the graft." (¶ 421)

If hybrids were being produced in England it is unlikely that Bacon would have been ignorant of the fact, were it generally enough known to be included in Shakespeare's plays. That this is not the case is proved by the following prophetic passage in which he discusses the possibilities of their production and

predicts their value to the plant breeder:

We see that in living creatures, that have male and female, there is copulation of several kinds; and so compound creatures, as the mule that is generated betwixt the horse and the ass, and some other compounds which we call monsters, though more rare; and it is held that that proverb, *Africa semper aliquid monstri parit,* cometh, for that the fountains of waters there being rare, divers sorts of beasts come from several parts to drink, and so being refreshed fall to couple, and many times with several kinds. The compounding or mixture of kinds in plants is not found out; which, nevertheless, if it be possible, is more at command than that of living creatures, for that their lust requireth a voluntary motion; *wherefore it were one of the most noble experiments touching plants to find it out: for so you may have great variety of new fruits and flowers yet unknown. Grafting doth it not, that mendeth the fruit, or doubleth the flowers, etc., but it hath not the power to make a new kind. For the cion ever over-ruleth the stock.* [Italics mine.]

It hath been set down by one of the ancients, that if you take two twigs of several fruit-trees, and flat them on the sides, and then bind them close together and set them in the ground, they will come up in one stock; but yet they will put forth their several fruits without any commixture in the fruit. Wherein note by the way, that unity of continuance is easier to procure than unity of species. It is reported also, that vines of red and white grapes being set in the ground, and the upper parts being flatted and bound close together, will put forth grapes of several colours upon the same branch; and grape-stones of several colours within the same grapes: but the more after a year or two, the unity, as it seemeth, growing more perfect. And this will likewise help, if from the first uniting they be often watered, for all moisture helpeth to Union. And it is prescribed also to bind the bud as soon as it cometh forth, as well as the stock, at the least for a time.

They report that divers seeds put into a clout, and laid in earth well dunged, will put up plants contiguous; which, afterwards being bound in, their shoots will incorporate. The like is said of kernels put into a bottle with a narrow mouth filled with earth.

It is reported, that young trees of several kinds set contiguous without any binding, and very often watered, in a fruitful ground with the very luxury of the trees will incorporate and grow together. Which seemeth to me the likeliest means that hath yet been propounded; for that the binding doth hinder the natural swelling of the tree; which, while it is in motion doth better unite. ¶'s 476, 477, 478, 479.)

This passage may be taken, in view of the encyclopediac nature of Bacon's information, to be conclusive evidence that the production of plant hybrids was not generally recognized in England at that time. Had hybrids been known the statement must have been much modified. It is strange that with the definitely expounded conviction of the importance of hybrids he should not have undertaken to make them artificially, since he had no prejudice against their production. Nothing in his writings indicates that any such attempt was made. His ignorance of the function of pollen is doubtless the explanation.

The statement regarding the production of graft hybrids in the second and succeeding paragraphs are very illuminating if read in conjunction with Perdita's remarks regarding the "bastard gilliflowers." While gilliflowers are not customarily grafted the seeds could be "planted contiguous" as there described, thus assumedly bringing about the production of new forms.

The "bastard gilliflowers," which Perdita takes exception to, are also the basis of another interesting observation. In the following passage we have what is perhaps the first record of two Mendelian phenomena, segregation and linkage. Whether the linkage between purple color and single flower exists today, appears not to have been ascertained. The scarcity of double purple gilliflowers was marked enough when Bacon wrote to deserve notice:

Take gillyflower seed, of one kind of gillyflower, as of the clove-gillyflower, which is the most common, and sow it, and there will come up gillyflowers some of one colour, and some of another, casually, as the seed meeteth with nourishment in the earth; so that the gardeners find, that they may have two or three roots amongst a hundred that are rare and of great price; as purple, carnation of several stripes: the cause is, no doubt, that in earth, though it be contiguous, and in one bed. There are very several juices; and as the seed doth casually meet with them, so it cometh forth. And it is noted especially, that those which do come up purple, do always come up single: the juice, as it seemeth, not being able to suffice a succulent colour and a double leaf. This experiment of several colours coming up from one seed, would be tried also in larks-foot, monks-hood, poppy, and holyoak. (¶ 510).

He also noted the tendency of double-flowered trees not to produce fruit. It is curious that so keen an observer should have believed that many trees bore no flowers:

> There be some plants that bear no flower and yet bear fruit; there be some that bear flowers and yet no fruit; there be some that bear neither flowers nor fruit. Most of the great timber trees, as oaks, beeches, etc., bear no apparent flowers; some few likewise of the fruit trees as mulberry, walnut, etc., and some shrubs, as juniper, holly, etc., bear no flowers. Divers herbs also bear seeds, which is as the fruit, and yet bear no flowers, as purslane, etc. Those that bear flowers and no fruit are few, as the double cherry, the sallow, etc. But for the cherry, it is doubtful whether it be not by art or culture; for if it be by art, then trial would be made, whether apple and other fruit blossoms may not be doubled. There are some few that bear neither fruit nor flower, as the elm, poplars, box, brakes, etc. (¶593.)

One other reference is of interest in connection with sex in plants:

> "It is strange, which is observed by some of the ancients, that dust helps the fruitfulness of trees, and of vines by name; insomuch as they cast dust upon them for purpose. It should seem, that the powdering, when a shower cometh, maketh a kind of soiling to the tree, being earth and water finely laid on. And they note, that countries where the fields and ways are dusty bear the best vines."

Is it possible that this belief in the efficacy of dust to promote fruitfulness is an unconscious recognition of the importance of pollen in the physiology of plants?

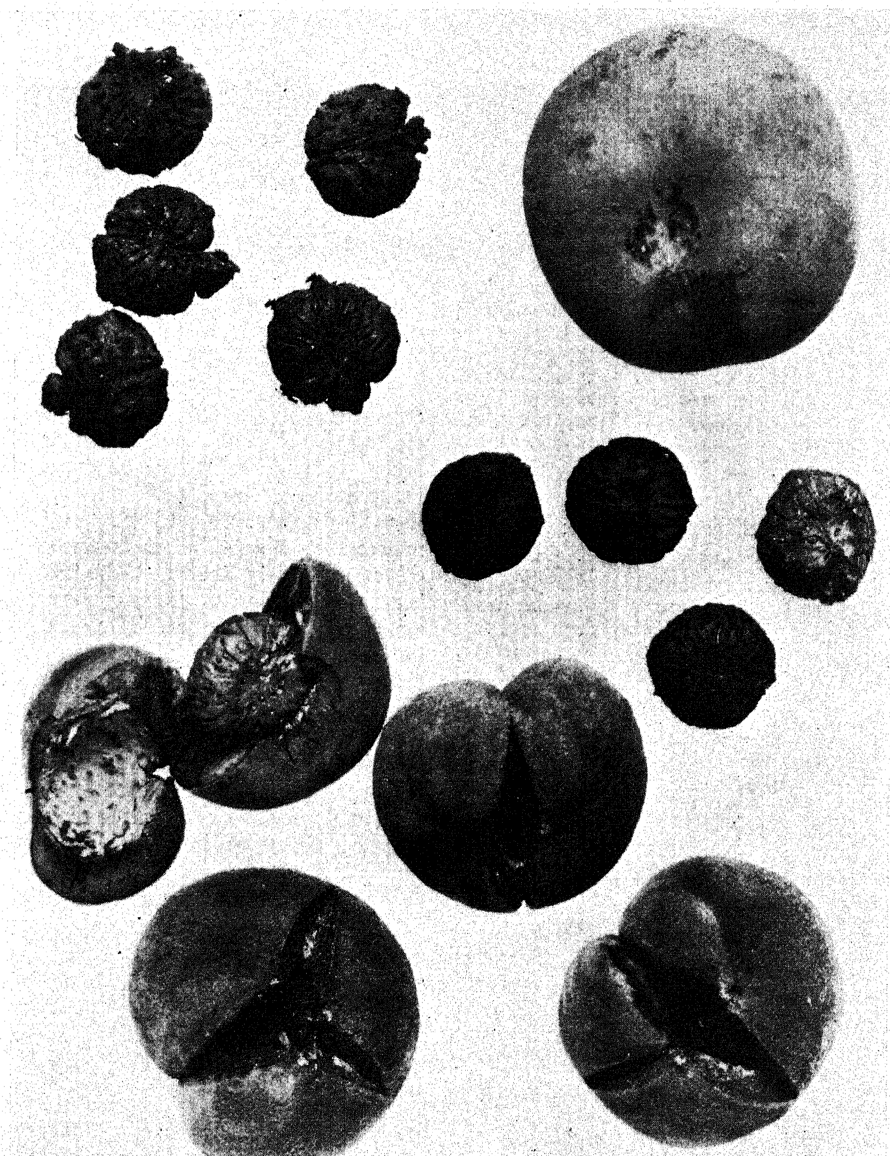
So many peculiar practices and beliefs are chronicled by Bacon that it seems almost a certainty that he would have recorded the practice of hybridization had he known of it. Since he specifically prophesied the part plant hybridization would play in the production of new forms it can hardly be questioned that he was ignorant of the process. It is, of course, possible that he deferred to a popular prejudice in the matter and did not record in print a phenomenon that he clearly understood. Some of his other views are so "advanced" for the times in which he lived that this seems unlikely.

Like so many of other parallels brought into the question of the authorship of Shakespeare's plays, the preceding quotations from Bacon might be taken either way in interpreting the conversation between Perdita and Polixenes quoted by Dr. Praeger. It is clear from what Bacon has to say about graft hybrids that experiments were being made with plants not ordinarily grafted, in which the seeds were planted contiguously and the shoots bound together. This fits exactly the process described by Polixenes. The allusion in the play is evidence of popular notion that the pied gillyvors were graft hybrids, but Bacon's remarks would appear to render it extremely doubtful whether anything more need be read into the text. The reference to segregation in flower color indicates that ignorance of sex physiology of plants on the part of the gardeners did not impede the natural sequence of events. Polixenes spoke even truer that he thought when he said "The art itself is nature."

ROBERT C. COOK.

A New Journal

In February the first issue of the new *Journal of Cellular and Comparative Physiology* appeared. It is published bi-monthly by the Wistar Institute of anatomy and Biology at Philadelphia. The articles in the first issue deal with such matters as the effect of tensions and pressures on various physiological reactions. No particular branch of physiology is included, and articles are preferred on analysis of fundamental physiological phenomena, as other journals are available for publication of articles dealing with such specialized subjects as behavior, developmental mechanics, sex determination, genetics, etc. A volume will comprise about five hundred pages. The subscription price is $5.00 per volume in the United States, and $5.50 foreign.



FRUIT CHARACTERS OF SAUCER PEACH AND HYBRID

Figure 4

Above, fruit and pits of the Saucer (Peen-to) peach; below, fruit showing splitting, and pits of the Almond-Saucer hybrid.

TWO SPECIES HYBRIDS

Hybrids of the Saucer Peach of Possible Value as Ornamentals*

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A RAPID-GROWING TREE
Figure 5
Showing growth of the hybrid of *A. mira* × *A. persica,* with some flowers much reduced in size at the lower right. The tree shows the prolific flowering characteristic of this hybrid.

THE occurrence of hybrids between related species of Prunus is not rare, although Hedrick[1] states that "there appear to be but few clear cases of peach and almond crosses. The almond blooms so much earlier than the peach that crosses could hardly occur in nature." It is a well established fact that the Saucer peach (Peen-to) *Amygdalus persica* var. *platycarpa* Bailey (*Persica platycarpa* Decne.) blooms in southern California early in February, and that it is a few weeks earlier than the commercial almonds. Some of its late flowers, however, would still be open with the first bloom of the almonds (about March 1).

A natural cross between *Amygdalus persica* var. *platycarpa* and *Amygdalus communis* Linn., and another cross between *A. mira* (Koehne) and *A. per-*

* Paper No. 261, University of California Graduate School of Tropical Agriculture and Citrus Experiment Station, Riverside, California.

FLOWERS AND FOLIAGE CHARACTERS OF THE ALMOND-SAUCER PEACH HYBRID

Figure 6

Left, foliage of Nonpareil almond; right, foliage of the Almond-Saucer (Peen-to) hybrid. (Slightly smaller than natural size.)

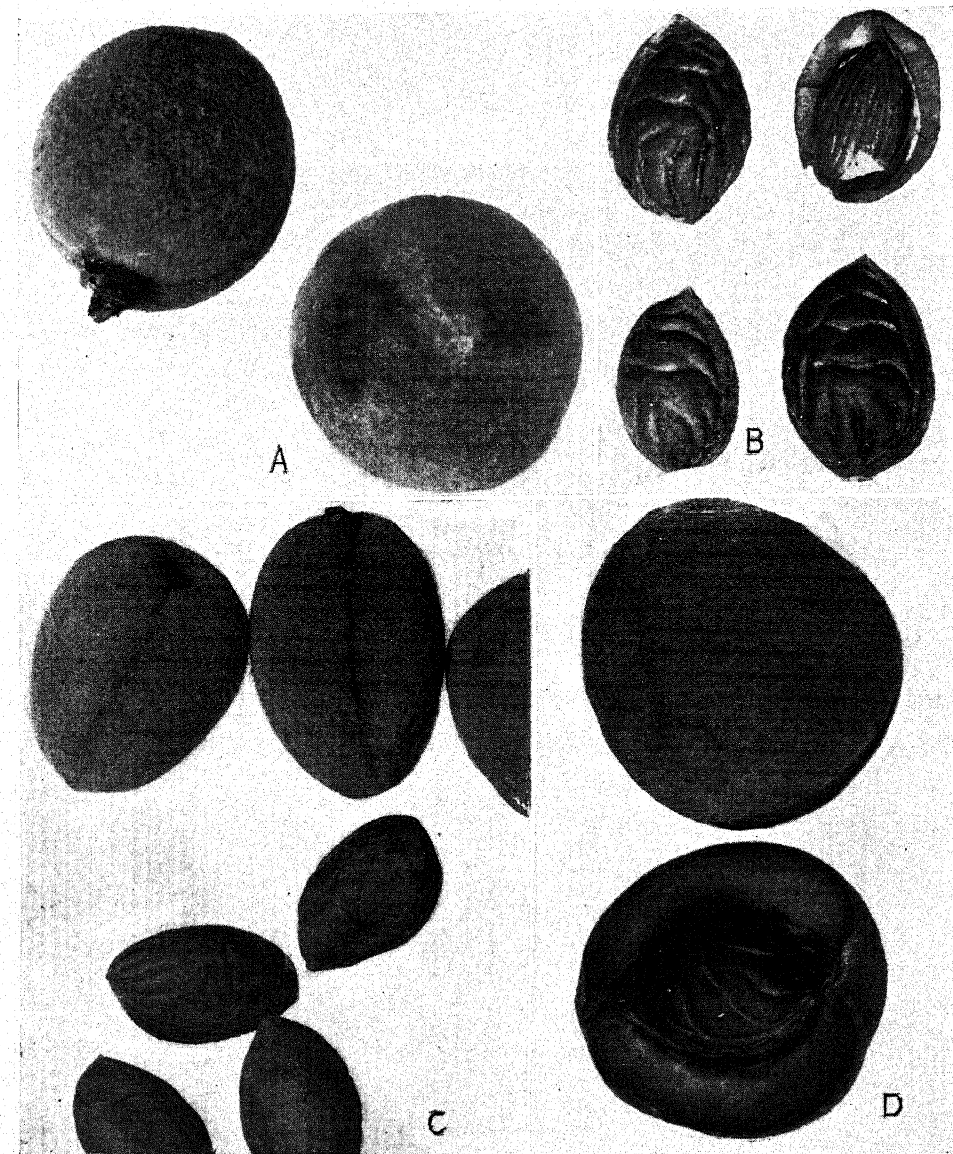
FLOWERS OF LARGE SIZE
Figure 7
Left, flowers of hybrid (*A. mira* X *A. persica*). Right, flower of *A. mira* for comparison.

sica Linn. have been under observation for several years.

Hybrid of Saucer Peach × Almond

The hybrid of the almond with the Saucer peach (*A. communis* ♀ × *A. persica* var. *platycarpa* ♂) was found as a single seedling grown from hard-shelled almond pits that had been purchased from the Claremont Nursery Company, Claremont, California. These seedlings were grown for two years in an experimental nursery planted by the author at the Citrus Experiment Station, Riverside, California. The second year after planting, blossoms and fruit were formed on this hybrid which differed from those of either an almond or a peach.

The flowers (Figure 6) were abundant and were larger than those of the ordinary peach, being 1.5 to 2 inches



FRUITS AND PITS OF PARENT AND HYBRID

Figure 8

A, B, D, Fruit and pits of hybrid (*A. mira* × *A. persica*); *C,* pits and fruits of *A. mira*. Fruits and pits of the other parent (*persica*) are shown in Figure 4.

in diameter. They are large like the almond flowers, but differ in color, being a pink instead of a rose or a white. The petals of the opening buds were deep rose-pink, Ridgway[2], but after opening, the color was a rose-pink (Ridgway) or a slightly lighter shade. The petals of the open flower are .75 to 1 inch long and about 0.5 inch wide, with margins entire except

for a notch at the apex. Sepals are greenish at first but soon they have a reddish color (Vandyke red, Ridgway), especially along the middle of the outside surface. The color of the margins of the sepals is a lighter shade of red or often they remain greenish in color. The almond character is shown in the large flowers and the apical notch of the petals.

The fruit is abundantly produced and has the general shape of the Saucer peach (Figure 4) but is somewhat smaller. It does not fall readily but dries out and may remain on the tree as a mummy. The flesh is greenish, hard, thin (0.25 inch), separates free from pit, becoming soft on ripening but is scarcely edible because of its limited amount and bitterness. It cracks open to the pit at the blossom end while the fruit is still green. This may be limited to a single crack, or two may open at right angles thus forming a cross. This cracking suggests an almond character. It has never been observed in the fruit of the Saucer peach.

The pit has much the same shape as that of the Saucer peach, but is smoother. The depressions and convolutions are shallower. The kernel is bitter.

The foliage of the hybrid resembles that of an almond but differs from it in that the leaves are longer and narrower or more slender (Figure 6). They differ from the Saucer parent in that they are thicker, narrower, more flat, and without the cross wrinkles in the blade of the leaf. After being picked the hybrid leaf will be flat while the Saucer peach leaf folds together and the tips curl toward the underside. The leaf serrations are not distinguishing. In general a leafy shoot of the hybrid is not so stiff in appearance as the almond, and is clearly intermediate in character between the Saucer peach and the almond.

The original hybrid seedling was transplanted to a permanent planting in the orchards of the Citrus Experiment Station. It showed a vigor of growth that is often characteristic of hybrids. It had been under observation for several years and had made a tree 15 feet high, when it became necessary to remove the original tree in rearranging the planting. Buds were established in other peach stock, and the trees now growing are budded stock. These are vigorous, but not so much so as the original tree, doubtless due to their not being propagated on the most suitable rootstock.

This hybrid has the early flowering habit of the Saucer peach and is in blossom in February before the almond and cherries, and at a time when other flowers are not abundant. It has desirable characteristics as an ornamental, such as large showy rose-pink flowers that appear before the leaves. It never suffers from delayed foliation[3] as is the case with some peaches. The foliage is vigorous and attractive.

A Hybrid of *A. Mira* × *A. Persica*

The hybrid (*A. mira* × *A. persica*) was a chance cross from an open pollinated flower of *A. mira,* a single tree among a few seedlings that were grown from pits of *A. mira.* The hybrid seedling showed great vigor from the first. The tree is now about eight years old with a height and spread of top of 15 feet (Figure 5). It has a smooth trunk which in its earlier years sprouted much at the crown and from the trunk that was above ground.

The flowers are numerous (Figure 7), and are a sharp contrast to the sparse bloom that a tree of *A. mira* produces under our conditions. The flowers for the most part do not develop into fruits. The four crops thus far borne have consisted of 6 to 36 fruits each. This lack of fruiting is often a desirable character in an ornamental.

The flowers appear before the leaves and are similar in shape to those of *A. mira,* but are much larger in size: 1.5 to 2 inches in diameter (Figure 7). They have a resemblance to apple rath-

er than to peach bloom. The petals are a slightly lighter shade than rose-pink (Ridgway), and have crenate or crinkled margins, with a small notch at the apex. Sepals are Vandyke red (Ridgway), especially along the middle outside part. The time of flowering is the same as that of A. mira, (about the middle of March in Riverside).

The fruit (Figure 8, *A* and *D*) is greenish, very pubescent with a shallow groove extending around it, medium to small (1.2 to 1.5 inches) in size, and round to oblong in shape. The flesh is 0.25 inches thick, greenish, becomes soft, but is scarcely edible as it is too acid. Its general appearance resembles *A. mira* (Figure 8, *C*). The pit shows characters that are intermediate between *A. persica* and *A. mira,* the peach with a smooth plum-like pit. The hybrid pit has a few shallow corrugations like *A. persica* (Figure 8, *B*). These corrugations near the apex extend around the pit, but at the base they have a longitudinal direction. The shape of the pit resembles that of *A. persica* rather than that of *A. mira* (Figure 8, *C*) and is 1.0 by 0.75 of an inch.

The two hybrids are recommended for experimental trial as ornamentals because of their large flowers, dark green, vigorous foliage and freedom from delayed foliation.

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Inheritance of Blood Sugar Differences in Mice

Differences of the level of blood sugar in mice were found to be inherited, by Cambridge and Howard, in 1930.* Writing in *Nature* (London) for January 23 (Vol. 129, No. 2347) L. C. Dunn calls attention to the fact that, assuming that a large proportion of the dead mice in some of the progenies were hypoglycaemic, we have strong indications that three allelomorphs control the level of blood sugar. The situation is of great interest, aside from its suggestive analogies to the inheritance of diabetes in man, because the gene for normal blood sugar is dominant both to the gene for low blood sugar and to the gene for abnormally high blood sugar. This is a very unusual situation, as in a series of allelomorphs controlling quantitative characters, the dominance tends to be cumulative in one direction. In the same number of *Nature,* Professor R. A. Fisher comments on Doctor Dunn's note, and calls attention to the fact that the condition having the highest survival value is dominant. This case is thus a further link in Fisher's theory of the development of dominance discussed in his *Genetical Theory of Natural Selection,* which postulates that dominance is built up by modifying genes and that the dominance relations of allelomorphs would be expected to be continually undergoing selective modification.

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BUD VARIATION IN APPLES

A Study of the Rôle of Bud Mutation in Deciduous Fruit Improvement

A. D. Shamel and C. S. Pomeroy*

THE subject of bud variation in apples is receiving increasing attention both from the scientific and commercial points of view. During the period of 1909 to 1915 the senior writer found several limb variations in otherwise normal trees of the Baldwin and Ben Davis varieties in Connecticut apple orchards. Subsequently, L. B. Scott, who was then associated with the writers in fruit improvement work, found several abnormal limbs and fruits in trees of the Baldwin, Rhode Island Greening, Northern Spy, and Winter Banana varieties in orchards located in Michigan where he conducted individual-tree performance-record work. Opportunities for carrying out progeny tests of these apparent bud variations were lacking so that final conclusions can not be drawn as to their perpetuation through budding.

During recent years several striking apple strains that originated as bud variations are being propagated commercially, including the Starking, Richared, Red Rome, Black Rome, Red Spy, Red Oldenburg, Red Gravenstein, Red Stayman, and Blackjon. These and other illustrations of bud variations in apples will be briefly described in this article. The main purpose of this paper, however, is to present available information concerning bud variations in apples and to point out the significance of this phenomenon as applied to the improvement of commercial apple varieties.

Published Accounts of Apple Bud Variations

The literature on bud variation in apples is meager. No attempt will be made here to refer to all of the publications on this subject but mention will be made of the first known published records that were found up to the past few years. Until recently only a very few varieties have been recognized as having originated as bud mutations and the probability of commercially valuable ones arising in this manner has been questioned.

Under these conditions it is interesting to find that the Red Russet variety, a russet form of the Baldwin which is still in cultivation, originated about 90 years ago as a limb variation in a tree located on the farm of Mr. Aaron Sanborn of Hampton Falls, N. H. This variety was introduced to notice by S. W. Cole in 1849† and its bud origin was first suggested in 1853.[26]

Another apple variation was described in 1852[22] in France and it was also recorded by Darwin.[17] A branch bearing two kinds of fruit was exhibited to the French Academy of Sciences and this fruiting condition was perpetuated in propagation by grafting.

A red variation of the Gravenstein occurring in Germany about 1855 was described in 1859.[32] This variation was rather widely grown as a standard variety and it was recorded in German and French publications in 1877,[28] in 1881[33] and in 1894.[21]

Another red Gravenstein variation which was named Banks was recorded in 1893.[16] It was first noticed and propagated by C. E. Banks, of Berwick, Kings County, Nova Scotia. It appeared as a limb variation and first came into bearing about 1880.

In *Apples of New York* by S. A. Beach, published in 1905 as a part of the report of the New York Agricul-

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†Numbered references are to Literature Cited, which will appear at the end of the article in the May number.

tural Experiment Station for 1903, the above mentioned Red Russet and Banks varieties, together with two others, the Olympia and Collamer, are listed and described as of bud sport origin.[3,4,5,6]

The Olympia apple originated as a tree in a block of Baldwins which were planted about 1860 on the place of William Shincke, Sr., at Olympia, Wash.[5] The fruit was first sold as Baldwin but it was later noticed to be larger and better colored. Tests proved the variation to be inherent and commercial propagations were made about 1890. The Collamer apple originated as a sporting branch bearing highly colored fruits in a Twenty Ounce tree in the orchard of J. B. Collamer at Hilton, N. Y., and was first propagated about 1900.[6]

In 1909 two apple bud variations, a Red Rome Beauty and an oblong-shaped Grimes Golden were described.[2] The Rome variation occurred as a limb bearing brilliant, solid, dark red fruits in a tree in an orchard of U. T. Cox at Proctorville, Ohio, and was exhibited at a meeting of the Ohio State Horticultural Society at Columbus in 1906. This variation has been commercially propagated and planted. The Grimes Golden variation was a tree owned by B. F. McCoun of Proctorville, Ohio, on which the fruit was oblong in shape instead of roundish or oblate as is typical of the parent variety. Fruit from this variation was shown at the Ohio State Fair in 1908.

The Chesebro-Spy variation which originated as a limb bearing solid, dark red fruits in a Northern Spy tree on the farm of C. C. Chesebro of South Haven, Mich., was first described in 1910.[20] The parent tree was planted about 1870 and the limb variation is said to have always borne dark red fruits.

Another variation of the Twenty Ounce apple was described by S. A. Beach in 1910[7] as the Hitchings. This occurred as an entire tree bearing deep red fruits in the orchard of Grant Hitchings at South Onondaga, N. Y. The fruits were said to be much like the Callamer but of a more solid red color with no stripes and maturing about two weeks later than the Twenty Ounce fruits. At the New York State Fair in 1910 Hitchings fruits were awarded a first premium as the best new apple.

In 1911 another red variation of the Gravenstein was described.[13] It was found as a single limb in an otherwise normal Gravenstein tree in an orchard owned by Van Zandt and Whipple on Orcas Island, Wash. This sport originated about 1907 or 1908 and was being distributed by a nursery company in 1911.

The foregoing list includes only 11 variations that have come to our attention as having been recorded by the year 1911. In the next ten years only five additional variations have been found recorded in literature. One of these, described in 1915[24] was the Anderson Twenty Ounce, a red-fruited variation found in the orchard of E. H. Anderson, at Hilton, N. Y., about 1898, as a limb in an old tree which had been grafted to Twenty Ounce prior to 1890.

In 1916 another Red Gravenstein limb was described[30] as having been found in the orchard of H. O. Mood of Worcester County, Mass., about 1910 and it was propagated to some extent.

Correspondence in 1917[29] told of a Russet Rome Beauty limb variation in a tree in an orchard of U. T. Cox of Proctorville, Ohio, which had produced only heavily russeted fruits from the time that it was first noticed, some time prior to 1905.

Two Red Oldenburg variations were also described in 1917.[18] One of these was located in the orchard of William Bardwell near Excelsior, Minn. This was an entire tree in a Duchess block that was planted about 1902. The fruit of this tree was considered to be so superior to the normal that a nurseryman purchased the tree and all propagating rights in it for $1,500 and it was rather widely disseminated as the Daniels Red Duchess. The article describing the Daniels Red Duchess[18] stated that a similar Red Duchess sport

has been found as a single limb in a tree on the grounds of the Minnesota Agricultural Experiment Station.

In September, 1922, publication was made[10] of the bud sport origin of two English apples, the Red Bramley from the normal form and a strain of the Cox Orange which produces strongly striped fruits.

In October, 1922, announcement was made[31] of the occurrence and purchase of the red-fruited Delicious limb sport which was later named the Starking. This was found in an otherwise normal Delicious tree in the orchard of Lewis Mood, near Mullica Hill, N. J. This tree was planted about 1909 and the variable limb had always produced the deep red fruits. (See Figure 9.)

In November, 1922, a bright red-fruited limb variation[34] was described that was found in 1916 in a tree of the James Grieve variety in the orchard of Hall Jones of Letchworth, Scotland. This variation was propagated and named McCoy.

In January, 1923, a Red Northern Spy sport was described[25] that had been propagated at the New York Agricultural Experiment Station in 1910 from scions received from C. E. Green, of Victor, N. Y.

In March, 1923, another Red Rome Beauty variation was announced[14] by L. J. Clifton of New York State.

In December, 1923, a red sport of Delicious was recorded as having been introduced by Frank Walker of Lounceston, Tasmania[35] and in correspondence he stated that it had been under observation for about 12 years.

In October, 1926, the origin of the Staymared was described.[11] The original tree variation in the orchard of B. C. Moonaw near Covington, Va., had been observed for several years.

In September, 1926, a description was published[36] of the red Delicious tree that was found about 1920 by Lewis J. Richardson in his orchard near Monitor, Wash., which gave rise through bud propagation, to the Richared variety.

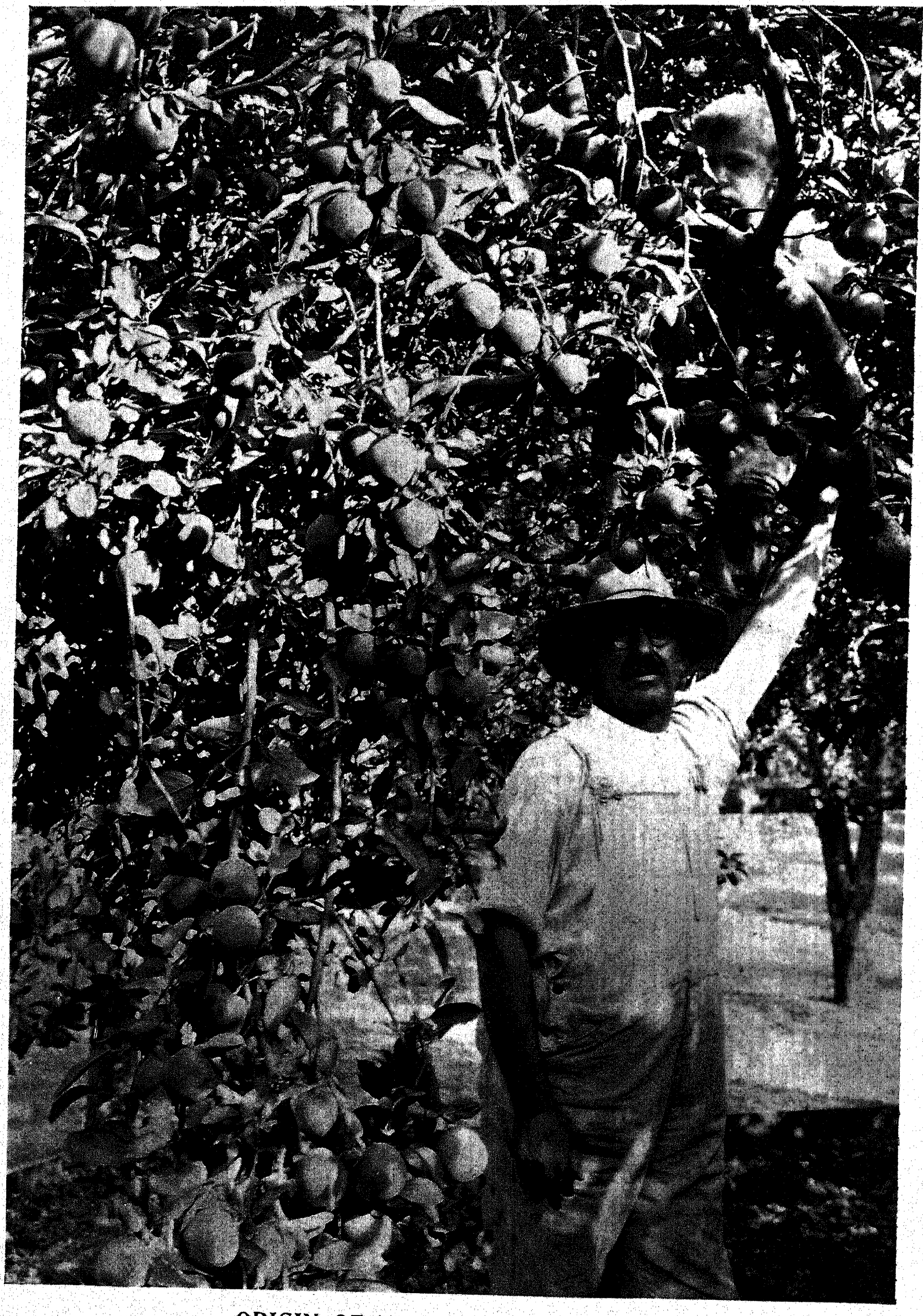
Within the past few years a large number of apple variations have been found and many of them have been mentioned in publications,[23,19,27,12,8,9,3] though detailed descriptions of their origins have been given for only a few.

The Occurrence of Apple Bud Variations

The term "improvement of apples through bud selection" is here used to mean (1) the discovery and propagation of desirable bud variations in order to develop strains in which the trees or fruits have one or more characters that are superior to those of the parent varieties for commercial culture, (2) the conservation of the proved varieties through the systematic selection of budwood from good type and inherently stable parent trees, and (3) the topworking of healthy undesirable trees in bearing orchards with buds obtained from good type trees of the best strains, or the replanting of the unprofitable ones with nursery trees that have been propagated from systematically, carefully selected buds.

The present study of bud selection in apples was begun by the senior writer in 1909 in the Connecticut Valley, mostly in the J. H. Hale orchards at South Glastonbury and Seymour, Conn. Later, individual - tree performance - record studies similar to those made in Connecticut were carried on in apple orchards located near Belding and Ionia, Mich. Recently, a study of the bud variations and the commercial performance-records of apple trees in orchards in the Wenatchee and Yakima districts of Washington was begun, together with similar observations in some of the apple orchards in the Hood River district of Oregon. The results of these studies will be briefly discussed in this report.

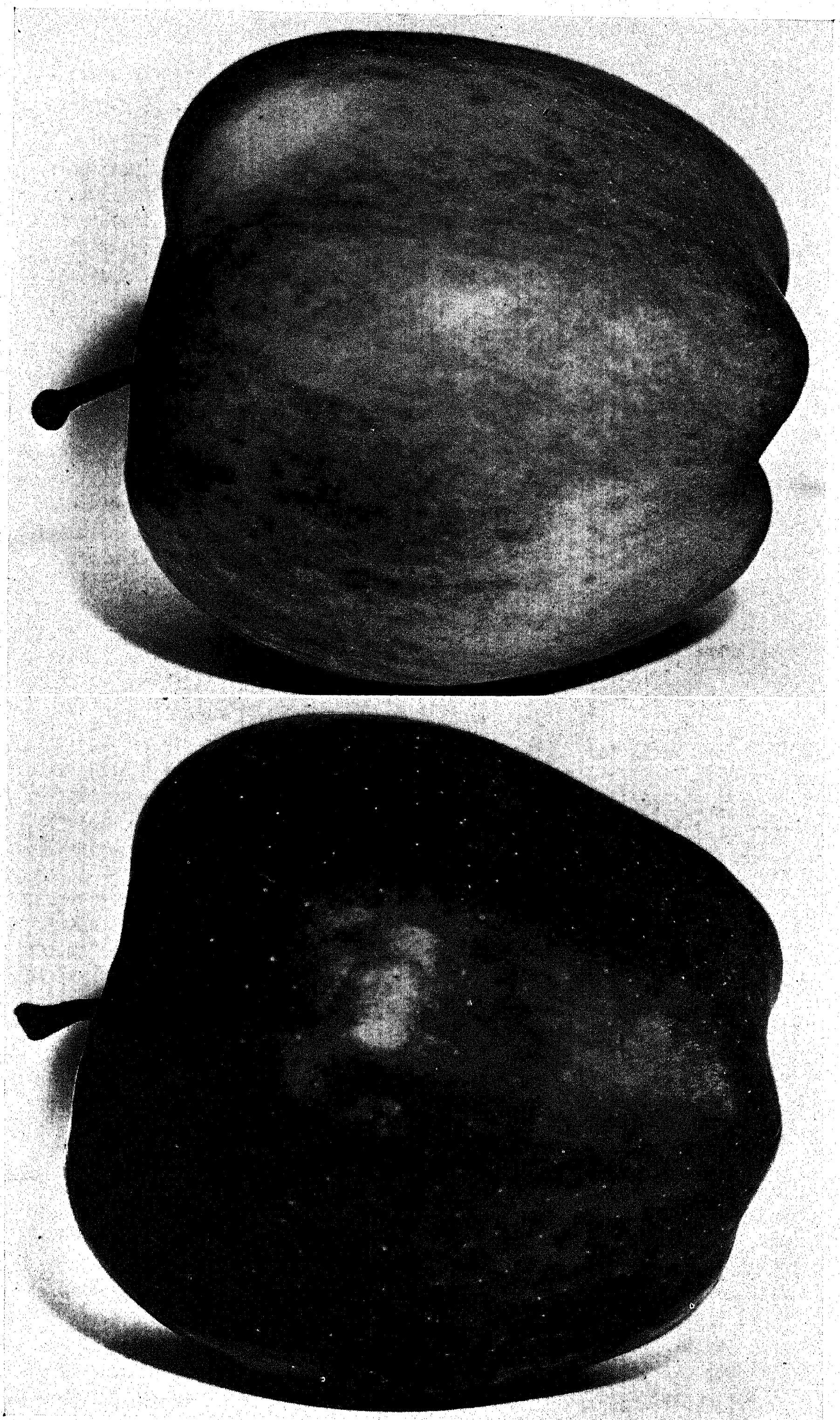
The limb variations in otherwise normal fruit trees that have been discovered by growers thus far are largely those in which the color of the fruits as they approach maturity serves to distinguish them from the normal ones.



ORIGIN OF THE STARKING APPLE

Figure 9

The Starking variety, which is now being widely grown, arose as a bud mutation in a tree of the Stark's Delicious variety in the orchard of Lewis Mood, Mullica Hill, New Jersey. The mutant limb is the one on which Mr. Mood's hand rests. The Starking variation is so marked an improvement over the Delicious that a high price was paid Mr. Mood for the propagation rights of the new form.



DELICIOUS AND STARKING APPLES

Figure 10

Typical Delicious (above) and Starking (below) apples showing the more nearly solid red color of the Starking. Yakima, Washington, December, 1928.

This condition is due largely to the fact that the color-of-fruit character is so easily seen and consequently is the one first discerned by interested observers.

Limb variations in otherwise normal apple trees have also been found in which the fruits differ from the normal in size, shape, time of maturity, number of seeds, and keeping qualities under storage conditions, or combinations of these characters. Doubtless other variations in the characters of the fruits as well as those of the foliage of the trees occur but the limited study of this subject thus far has prevented the accumulation of much definite information as to their occurrence and characteristics.

The number of striking bud variations that have been reported in apple trees from time to time has increased rapidly during the past two or three years as a result of the growing interest in this subject until 173 are now known, as listed in Table 1. Growers and others are begining to learn when and how to look for variations and what to look for. Furthermore, the significance of bud variations for the improvement of the apple varieties is just beginning to be realized. It is believed that the superior color characteristics of the Starking and Richared variations, the extensive advertising given them and the large prices paid for their propagating rights have spurred other growers to intensive searching of their orchards in the hope of finding other variations that may be of equal or greater value. Only 20 of these variations were recorded prior to October, 1922, when the Starking was announced. Hence 153 of them have been brought to notice in the past nine years and a very large part of these were listed during the past three years. With the awakening interest in this subject and the realization of its importance it seems certain that the number of bud variations now known will be largely increased in the near future. The newly enacted Plant Patent Law will also foster an interest in bud variations.

For the most part this list includes those variations that have occurred as limb sports in otherwise normal trees. A few entire-tree variations have been included where conclusive evidence has indicated their origin as due to the propagation of bud variations. In this table the bud variations have been grouped according to the nature of the variation, the parent variety in which they were found to have originated, the number of variations in each variety that are distinguishable in one or more characters from the parent forms, and the total number of bud variations that have been found and studied more or less up to the present time (Aug. 15, 1931).

It will be noted that there are 143 bud variations in which the color of the apples are of a deeper, darker red, or a more solid red than is characteristic of the parent forms, three have a more green or greenish color of fruits than the normal, three have a more distinctly striped appearance than the normal, and in three the fruits are russeted. In ten instances the limb sports bear apples of large size as compared with the normal ones. In four instances the fruits are distinctly flattened in shape, in one case the apples have a much more oblong shape than the normal ones and in one instance there is a tendency for the production of twin fruits instead of single ones from each blossom. There are three variations that tend to bear earlier maturing fruits than the normal, one variation in which there is a tendency for the production of annual rather than biennial crops, and one limb variation in which the apples are seedless.

Some of these bud variations, such as the Starking, Richared, Red Winesap, Staymared, Blaxtyman, Red Rome, and Blackjon are now being propagated in many apple growing regions. Others are being grown in progeny tests in various locations in order to determine their value for commercial culture as

compared with the parent varieties. A few are being tested in propagation studies in order to obtain reliable information as to the transmission of their characters through budding. Most of the remainder are under systematic observation in their original locations.

Isolation of Strains Originating from Bud Variations

The term "isolation of strains originating from bud variations" is here used

TABLE 1.—A list of apple bud variations of which records have been obtained prior to Aug. 15, 1931.

Nature of the variations	Parent variety	Number of bud variations known	Totals
Color of fruits			
Deep or solid red	Baldwin	4	
	Ben Davis	2	
	Bramley	1	
	Chenango	1	
	Delicious	37	
	Esopus Spitzenburg	2	
	Fameuse	4	
	Gravenstein	8	
	James Grieve	1	
	Jonathan	2	
	McIntosh	8	
	Northern Spy	8	
	Oldenburg	4	
	Rambo	1	
	Red Astrachan	1	
	Ribston	1	
	Rome Beauty	12	
	St. Lawrence	1	
	Stark	7	
	Stayman Winesap	8	
	Tompkins King	2	
	Twenty Ounce	4	
	Westfield	1	
	Willowtwig	3	
	Winesap	19	
	Winter Banana	1	143
Green or greenish	Stayman Winesap	1	
	Winesap	2	3
Striped	Black Rome	1	
	Cox Orange	1	
	Williams	1	3
Russeted	Baldwin	2	
	Rome	1	3
Size of fruits			
Very large size	Delicious	1	
	Fameuse	1	
	Grimes Golden	3	
	Hyslop	1	
	Jonathan	1	
	Stayman Winesap	1	
	Winesap	2	10
Shape of fruits			
Flattened	McIntosh	2	
	Northern Spy	1	
	Jonathan	1	
Oblond	Grimes Golden	1	
Twin fruits	(unknown)	1	6
Earlier production	Baldwin	1	
	Northern Spy	1	
	Rhode Island Greening	1	3
Annual cropping	Yellow Transparent	1	1
Seedless fruits	Porter	1	1
Total			173

to mean the propagation experimentally or commercially of bud variations that are clearly distinguished from the parent forms in one or more fruit or foliage characters that are found to be transmitted through bud propagation. The variations may occur either as limbs or entire trees and are the result of somatic variations rather than of seedling origin.

The strains isolated from bud variations may be better adapted for commercial culture than the parent varieties by reason of one or more superior characters, such as improved color, size, shape, or season of ripening of the fruits, or combinations of these characters. However, it is possible that the strains originating from bud variations may be inferior to the parent forms in one or more characters, as for example, lighter yields, or through bearing apples of poor commercial quality.

The growing interest in strains of the commercial apple varieties that have originated from bud variations is due largely to those that have been found to be superior in some respect to the parent varieties for orchard culture. The trees of the commercially valuable strains that are best known at this time produce fruits of deeper, richer, or more completely red color than the parent varieties.

From observations of a number of bud variations in apple trees in different apple-growing districts of the United States during the past 22 years, the writers believe that valuable commercial strains may be isolated and propagated in which the trees will produce fruits having better commercial size and shape, and earlier or later ripening than the parent varieties. In the case of those varieties in which the trees have a strong tendency to bear full crops only every other year it may be practicable to isolate strains in which the trees tend to bear annual crops.

(Concluded in the May Number)

World Birth Rates

In his presidential address before the Sanitary Inspectors Association (London), Sir Leonard Hill said that the birth rate was now the subject of intensive propaganda among the masses of the people, and the no-child or one child home was becoming the rule. Although the unskilled continued as yet to breed more than the skilled, birth control was practiced in every class of society. The result was that between 1921 and 1931 the population of Great Britain increased hardly more than in the previous decade, which included the war. The infant mortality had fallen from 167 per thousand births at the end of the last century to 60 in 1930, but this did not compensate for the enormous drop in the birth rate. The annual increase of births over deaths per thousand living was 12 at the end of the last century but in 1929 was under 3, so that soon the population would be stationary. Three children per family were required to keep up the population. The Japanese were increasing at the rate of a million a year. They were multiplying just as we did with the coming of the industrial revolution, and, with a birth rate continuing to rise at the present rate, in thirty years would number 100 millions, and be the largest great power. They had 27 million agricultural workers to keep up the virility of the race. Our dominions and colonies occupied a fourth of the best land in the world, and we ought to send thither 250,000 emigrants a year, and these should be young people who would breed. The alternative was to leave that vast area unoccupied or to be colonized by other nations. If the white men were going to lessen their breeding rapidity they must see to it that colored races were taught the methods of doing so. At present such races were kept down to the level of the food supply by high infant mortality, famines and epidemics. The population of the world was increasing by some 12 millions a year, but this could not continue. A far less cruel world could be brought about only by limitation of the birth rate, but this must be coupled with the duty of keeping up a virile race. Birth control was at a minimum among those with a tendency to mental defect, and there was some danger of increase in inferior types.—*Jour. Amer. Med. Assoc.*, Oct. 10, 1931.

A RYE-WHEAT HYBRID*

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UNTIL recently the cross between rye and wheat, when rye is used as the female parent, was not successful, although a number of experimenters have been successful in making the reciprocal cross. In 1922 Gaines and Stevenson[1] described F_1 and F_2 rye × wheat crosses.† Meister and Tjumjakoff[2] in 1928 described F_1 rye wheat crosses made at the Saratov Experiment Station in Russia.

The plants described by Gaines and Stevenson were all obtained from Rosen rye. Three different wheats were used as the male parents and six F_1 plants were grown. Both the F_1 and F_2 plants are described as being very rye-like and only slightly modified by the wheat parents. The modified characters were sterility; shorter plants, heads, and beards; purple straw; lemmas ciliated only towards the tips; and necks below the head thicker and stiffer than rye. The last three characters were found only in some of the plants. The rye-like character of these plants is attributed to maternal inheritance.

The writer has never selfed Rosen rye but has selfed Abruzzi rye and Georgia rye. All of the above modifications have been found in selfed rye grown at the Georgia Experiment Station. Sterility is the usual condition when rye is selfed or when a few plants are grown by themselves. In rye, as well as in other cross fertilized plants, the vigor and therefore the height of the plant and the length of the head is reduced by selfing. The length of beards in selfed rye is variable but is generally shorter than in open-pollinated rye. Rye often has purple stems, so that this character may have come from a mixture in the white-stemmed Rosen rye. In some strains of selfed rye the lemma is not ciliated, and in a number of strains the lemmas are ciliated only towards the tip. The neck of selfed rye will vary from about 88 mm. to 174 mm. diameter, while the neck of wheat grown on the same land will measure about 180 mm. It follows from this that the modifications found by Gaines and Stevenson in the F_1 and F_2 plants may have been caused by selfing the rye rather than crossing it with wheat.

Meister and Tjumjakoff describe F_1 plants obtained crossing Jelissejev rye with No. 648 wheat. The plants are described as resembling wheat more closely than rye, although several rye characters are described. All of the vegetative characters are described as wheat-like, the few rye characters found being in the head. The authors make a careful comparison between these rye × wheat crosses and some wheat × rye crosses of the same parentage and conclude that they are identical in morphological characters.

The rye × wheat described in this paper was obtained from a cross between Abruzzi rye and Chinese wheat made in 1929.‡ Heads of the parents and reciprocal hybrids between the Chinese wheat used was a selection from the same variety which Leighty and Sando[3] found would cross readily with rye when used as the female parent. It was probably due to the use of this wheat that the cross was successful, as repeated attempts to make

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†Here and elsewhere in this paper the female parent is given first.

‡The cross was made by S. J. Hadden, Assistant Agronomist.

the cross with other wheats have proved a failure.

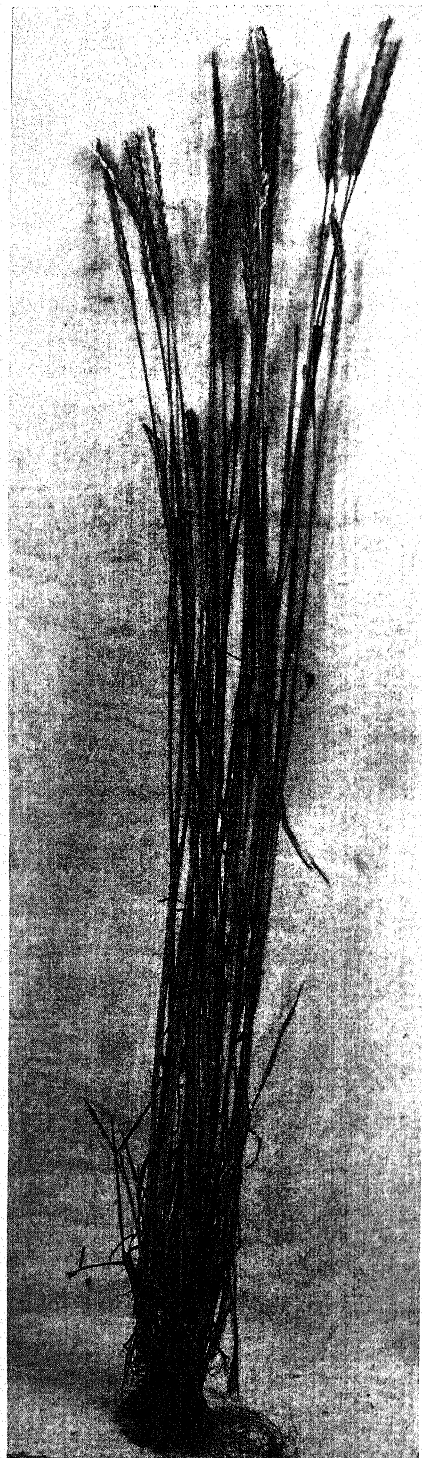
Two very small, shrunken seeds were produced on one head of the hybrid. Only one seed produced a plant. This plant was grown in the greenhouse along with a number of strains of selfed Abruzzi rye. A careful comparison was made of the young rye plants and the supposed hybrid. In every particular the young plant appeared to be a pure rye plant. The width and color of the leaves, ligule, auricles, etc., of this plant so closely resembled rye that its hybrid origin was considered very improbable. With two parents as variable and as nearly alike vegetatively as Abruzzi rye and Chinese wheat it is very hard to state definitely what vegetative characters come from either parent. However, two characters were found that were very characteristic of Abruzzi rye.

On the young plants of the Abruzzi rye auricles are either absent or are very poorly developed. Later on, some stools may develop with one fair size auricle, while the other one remains small and undeveloped. The rye × wheat hybrid belonged to this type. In Chinese wheat the auricles are of average size and both about the same size.

During one stage of the development of many young singly-planted specimens of Abruzzi rye the growth is very closely bunched together. It sometimes looks as if the stools are tied together into a bundle. This character is not found in all rye plants but it is one which the writer had never observed in any wheat plant. In Chinese wheat the growth is also upright but the tendency is for the stools to spread out in single plants rather than to bunch together.

One vegetative character was noted which may or may not have come from the wheat parent. Abruzzi rye generally has hairy sheaths, although when selfed the sheaths are sometimes smooth. In Chinese wheat the sheaths are glabrous. In this hybrid the sheaths were glabrous.

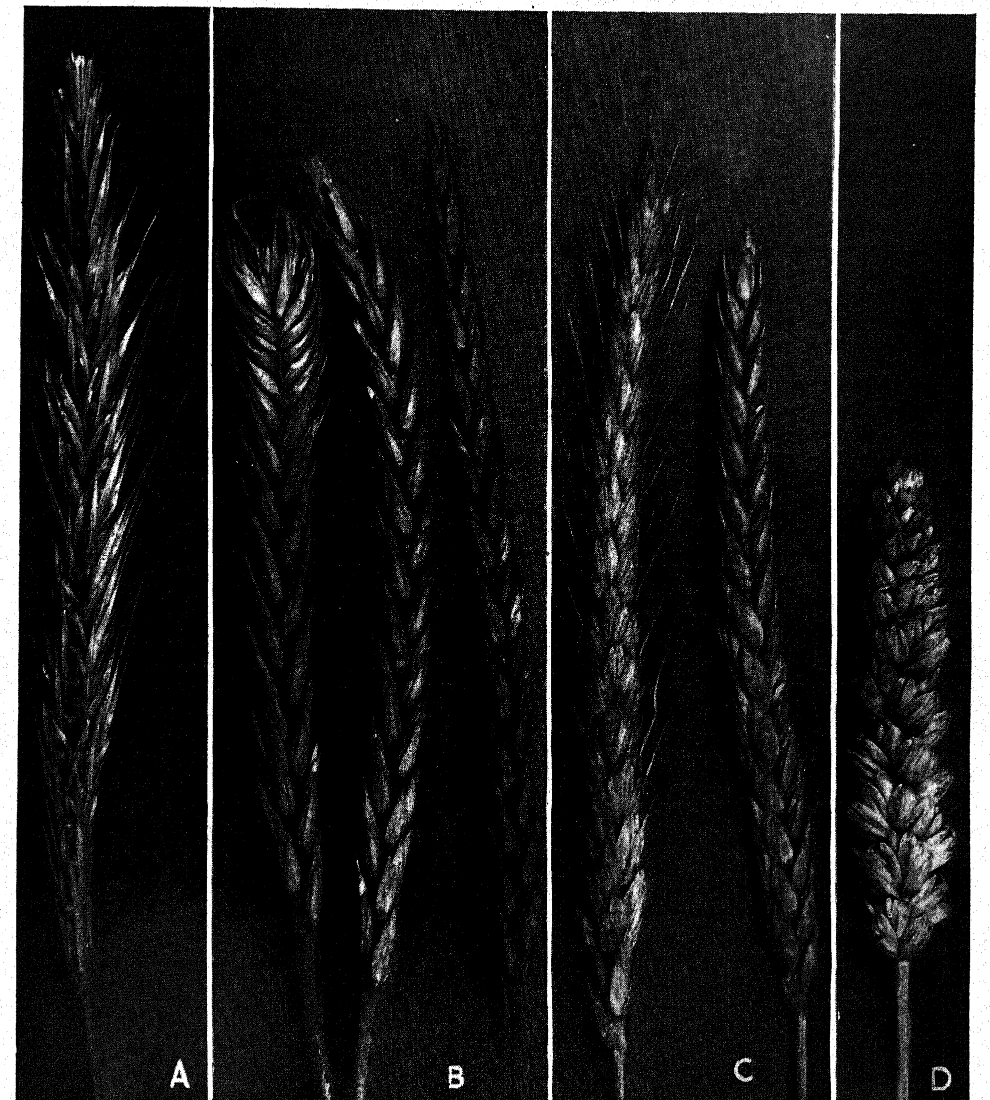
During the past five or six years the writer has grown a large number of wheat × rye hybrids, some of which



A RYE-WHEAT HYBRID

Figure 11

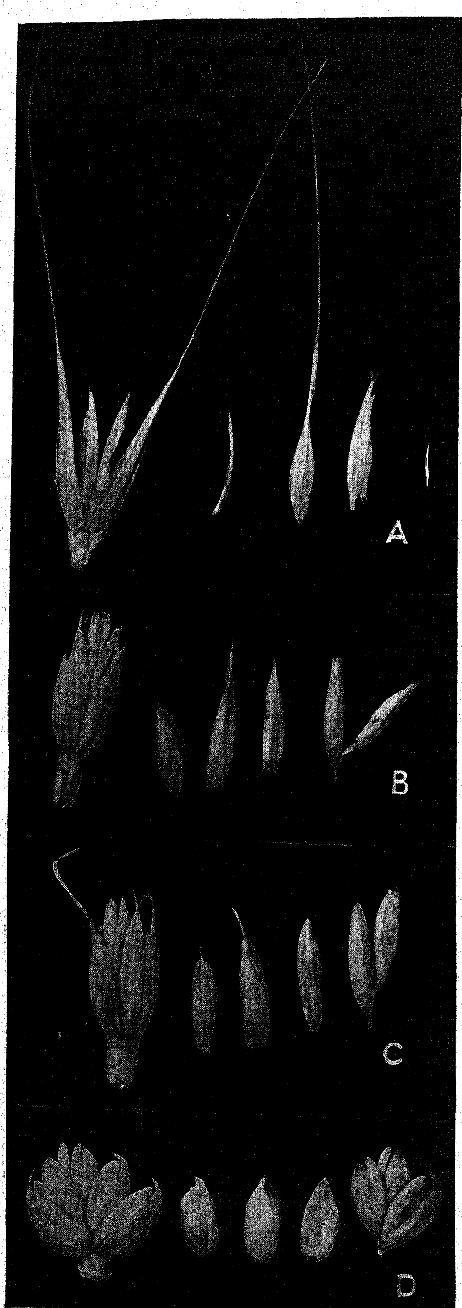
Wheat-like characters from the paternal parent prove the plant a hybrid.



HEAD-CHARACTERS OF PARENT SPECIES AND HYBRIDS

Figure 12

A—Abruzzi rye; *B*—Abruzzi rye × Chinese wheat *C*—Chinese wheat × Abruzzi rye; *D*—Chinese wheat. In comparing the shape of the heads of the parents with their hybrids, allowance must be made for the effect of the seed in the parent heads on the shape of the spikelet. Both hybrids were sterile or nearly so.



SPIKELET AND GLUME CHARACTERS
Figure 13

A—Abruzzi rye; *B*—Abruzzi rye × Chinese wheat; *C*—Chinese wheat × Abruzzi rye; *D*—Chinese wheat. Part of the difference in shape between *D* and *B* and *C* is due to the fact that *D* produced seed while *B* and *C* are sterile.

were Chinese wheat × Abruzzi rye. In every case the young plants were more like wheat than rye. The auricles have always been well developed and usually have been very large as compared with wheat.

In the early spring the hybrid was moved from the greenhouse to the field. As soon as the plant headed there was no doubt as to its hybrid origin. The plant is shown in Figure 11. Only a few of the head characters resembled rye, while a number of wheat characters were apparent. The rye head and culm characters identified were: hairy neck or pubescence on the upper part of the culm, elongated glumes intermediate in type between the lanceolate glumes of rye and the ovate glumes of Chinese wheat, and length of head intermediate between rye and wheat. The glumes were identical with those on Chinese wheat except for the elongation already mentioned, and a slightly greater ciliation of the outer glumes. The spikelets had three or more florets. The terminal spikelet was present. Spikelet and glume characters of the parents and hybrids are shown in Figure 13.

The hybrid produced seventeen seeds from open pollinated flowers and two very small shrunken seeds from flowers back-crossed to rye, making a total of nineteen seeds. One seed was perfectly developed, eleven were somewhat shrunken and seven were badly shrunken. Both in color and shape the seeds looked like wheat.

The head characters of the reciprocal cross, i. e., Chinese wheat × Abruzzi rye, corresponded in every particular with the hybrid.

Discussion and Summary

The F_1 rye × wheat plant described in this paper is very similar to those described by Meister and Tjumjakoff. In both cases the head characters were predominantly wheat, although markedly modified by the rye, and the heads were similar to those of the reciprocal cross. In one respect this cross dif-

fered from those obtained by Meister and Tjumjakoff, and also from the reciprocal cross. Instead of the vegetative characters being predominantly wheat-like, they were rye-like. This difference may have been due to the heterozygous character of both parents, rather than to any maternal inheritance.

Literature Cited

1. GAINES, E. F., and STEVENSON, F. J. 1922. Rye-wheat and Wheat-rye Hybrids. *Journal of Heredity* 13:81-90.

2. MEISTER, N., and TJUMJAKOFF, N. A. Rye-Wheat Hybrids from Reciprocal Crosses. *Journal of Genetics* Vol. 20, No. 2, November, 1928.

3. LEIGHTY, C. E., and SANDO, W. J. 1925. Natural and Artificial Hybrids of a Chinese Wheat and Rye. *Journal of Heredity* 19(1):23-27.

The Beginnings of Society

ANIMAL AGGREGATIONS, *by* W. C. ALLEE. Pp. x + 431. 20 Chapters. 35 Figures, 1 Chart, 37 Tables. Price, $5.00. Chicago, The University of Chicago Press. 1931.

THIS is a study of the relationships existing among the more loosely integrated collections of animals, which may rightly be designated as "animal aggregations," with regard to their ecological and behavioristic physiology, as well as with regard to their strictly social implications.

This book is built about a phenomenon or a series of phenomena, rather than about a philosophy. In the present form it may even be designated as notes on an unsolved problem; but since a presentation of a problem is necessary for its ultimate solution, and since an inquiry into the universality of a given problem is imperative before undertaking laborious experimentation directed toward finding a solution, no apology is offered by the author for summarizing our growing knowledge on the subject of animal aggregations at the present stage of inquiry into the problems involved.

The book discusses the classification, formation, factors conditioning, and the integration of animal aggregations. Among the stimuli utilized in the integration of aggregations are those perceived by the several senses. Some animals respond definitely to vibrations of lower frequency than sound vibrations. The mitogenic rays of the Gurwitsch school are suggestive of similar subtle means of group integration epitomized in Maeterlinck's "spirit of the hive," but, as yet, the matter rests only on suggestion.

The harmful effects of aggregations of individuals are seen under certain conditions in the inhibition of growth, the retardation of reproduction, and the increased death rate among certain animals. On the other hand, crowding at certain densities stimulates growth in snails, Drosophila, tadpoles, sea urchin plutei, and heterotypic tissue cultures. Robertson and also Yocom have demonstrated that true acceleration of the rate of reproduction may occur associated with the introduction of more than one organism into a limited amount of medium. Under laboratory conditions aggregations of isopods and of ophiurans serve to quiet these animals, thus affecting respiration and in the long run this proves to be an adaptive reaction. Aggregations are also protected from toxic reagents by the wider distribution of the toxic material, by its absorption on the slime produced by the aggregating group, or to the lowering of the protection resulting from the rate of metabolism in aggregated as compared with non-aggregated individuals. Survival values in crowded insect populations vary with number, age, and previous history of the individuals. Experiments indicate that there is, however, an optimal density

(35-55 flies per one-ounce bottle or 8 cc. of fluid substrate) below and above which specific death rates are higher at all ages than they are at this optimum.

An aggregation implies that the grouped individuals have tolerance for the presence of other organisms in the same limited area, and that they have a reaction system which causes them to aggregate or to remain aggregated if passively collected. In addition certain other qualities are needed, particularly the ability to establish close group integration. The facts indicate that not all animals whose groups show survival values are to become more closely social, but that animals, whatever their endowments, could not have developed the social habit had the incipient social stages lacked the type of survival values which we have repeatedly demonstrated for different sorts of animals, and now also for bacteria. These values are a function, other conditions being equal, of the mass of animals in relation to the volume of their effective environment.

Crowding and Sex-Determination

From our present knowledge it appears that sex determination, at least for the majority of animals, is normally associated with the chromosome mechanism, but that chromosome determinations may be overruled by other factors, among them the effects produced by crowding. In certain cases these effects appear to be produced by the transfer of material from one individual to another, as in *Bonellia;* in others, as Cladocera, by the effects of crowding upon animals that are not necessarily in physical contact with each other. The important thing is that they occur at all and can be controlled, in certain animals of widely distributed taxonomic position, by the degree or kind of aggregation obtaining.

In Cladocera the tendency to change the type of reproduction as the culture becomes crowded, from the production of parthenogenetic eggs rapidly produced bu tnot resistant, to the sexual eggs which can withstand adverse conditions, has survival value In nature such crowding usually precedes the drying-up or freezing of small bodies of water in which the animals have been living; and either usually follows a long reproduction period which has given time for the increase of the cladoceran population to effect a definite change in the environment. Under these conditions the production of resistant eggs has definite value.

Morphological modifications due to crowding are seen in production of the winged forms among crowded aphids. The evidence from Uvarov's phase theory that locusts pass to and from the solitary and swarm phases with marked morphological changes is regarded as unproved as yet, but promising for further investigation. Some data on the association of environmental factors with morphological changes occur in the work of geneticists. Crowding affects bristle number in Drosophila and exercises a selective effect.

Evolution of Aggregates

There is nothing in recent work which displaces the earlier conclusion that overcrowding is harmful; but this newer evidence shows that under proper conditions, and entirely apart from breeding or hibernation, beneficial results may follow aggregation, in many organisms of the same or different species, within a limited space. This means that in grouping caused by the tropistic reactions of individuals to environmental factors there may be a natural co-operation effective long before the physiological organization of the group has reached the level of development which occurs in the groupings usually designated as being truly social.

The knowledge that co-operation exists among loosely organized, or among apparently unorganized, groups of animals living even temporarily in the same region, gives us much clearer evidence than has been available to students of social life, that their conclusion that co-operation is one of the major biological principles in evolution is correct, and that its roots extend far

below the level of well integrated social activity.

The last advance in this series comes when individuals cease to react as separate units and respond only as members of a group—when, as in the case of ants or termites and, rarely, with men, they are largely group-centered rather than self-centered. Many of the so-called "altruistic" drives in man apparently are the development of these innate tendencies toward co-operation, which find their early physiological expression in many simpler animals.

With the development of the nervous system, closer co-operation becomes possible and larger numbers are affected. There is much reason for thinking that many of the advances in evolution have come about through the selection of co-operating groups rather than through the selection of individuals. This implies that the two great natural principles of struggle for existence and of co-operation are not wholly in opposition, but that each may have reacted upon the other in determining the trend of animal evolution.

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University of California.

The Vinderen Laboratory A Quarter-Century Old

THE Vinderen Laboratory of Oslo has recently celebrated its twenty-fifth birthday and has received congratulations from biologists and eugenists all over the world. Among the congratulators we find Ch. B. Davenport, Irving Fisher, Henry Fairfield Osborn, of the United States; Virchow, Germany; Gini, Italy; Lundborg, Sweden, and Darwin, England. Among politicians we find Reichspresident Hindenburg. The Antropological Society of Berlin has on the occasion elected Jon Alfred Mjöen, director of the laboratory, as corresponding secretary.

The Vinderen Laboratory was founded in 1906 by Dr. Mjöen for the experimental study of biological problems with particular reference to human inheritance. At first it was entirely a private venture, but soon received subsidies and recognition from the Norwegian Government.

Dr. Mjöen and his assistants first devoted their attention to the isolation and study of such mental characteristics as could be recognized and measured with accuracy. Delicate methods were developed for assessing and tabulating varying degrees of mental ability of genius and criminality. Such characteristics—"psycic basic qualities"—were then measured in three generations of more than one thousand families and 10,000 individuals. After being measured they were valued. It is the first time in biological or psychological research that mental qualities have been valued and could be expressed numerically, thus affording a solid basis for study of the heredity of mental traits in human stocks.

By these measurements it was possible to prove a long series of correlations between children and parents, children and grandparents, and children and side-lines (collaterals).

During recent years the Vinderen Laboratory has been studying the application of eugenic principles to politics, and has worked out a series of reform bills. A program for Race Hygiene, called "A first step," was laid before The Medical Association of Oslo as long ago as 1908. In this program we find the following guiding lines for a new population policy, based on biological principles:

A new system of law: "We shall not treat the crime, but the criminal."

A new system of health: Prevention (not only cure) of racial and national diseases as a function of the state. Total eradication of venereal diseases.

A new system of education: Mental culture (character) and body culture. "Life test" instead of examination test. No masculine education of women.

A new system of poor relief: "Help only to self-help." (Working colonies).

A new system of community and state: Biological and physiological in-

vestigation of the whole nation and *selective* inner colonisation, biological control of immigration and clearing house for the diffusion of biological knowledge.

This program from 1908, which divided the work into "negative," "positive," and "prophylactic" Race Hygiene was probably the first detailed program for race hygiene based on biological principles. No wonder that a program of such revolutionary tendencies aroused a storm of indignation and that the leader of the Laboratory was attacked in certain circles in a manner which is probably without parallel in scientific circles in the history of the country. A great change in public opinion has nevertheless followed, and in the course of 20 years many of the points in the above program have received legislative sanction and the last two points, biological control of immigration, and sterilization and regregation of inferior race elements will, in all probability, come up in the Norwegian Parliament (Storting) in the near future.

N. H.

Vinderen Laboratory.

First Glance

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

SPECIAL EDUCATION, The Handicapped and the Gifted. White House Conference on Child Health and Protection; Report of the Committee on Special Classes, CHARLES SCOTT BERRY, PH.D., Chairman. Pp. 604. Price $4.00. The Century Company, New York. 1931.

The Procrustean Mind, keyed to the Average (strange non-existent figment of a lockstepped imagination!), makes, of our Plusses and Minuses, incongruous bedfellows—

DIE MITOGENETISCHE STRAHLUNG. *By* A. GURWITSCH. Pp. 386. Verlag von Julius Springer, Berlin. 1932.

The latest news of the mysterious rays that Gurwitch believes emanate from living things (some of them at least), and increase the rapidity of cell division.

THE FAMILY, Source Materials for the Study of Family and Personality, *by* EDWARD BYRON REUTER, Professor of Sociology, University of Iowa, and JESSIE RIDGWAY RUNNER. Pp. 604. 18 Chapters. New York and London, McGraw-Hill Book Company. 1931.

Perhaps it is possible to have a human culture without families, but to date it has not been done (Soviet Russia notwithstanding). First glance suggests that we may not be civilized very long, if Reuter-Runner view the situation aright.

THE TECHNIQUE OF SOCIAL PROGRESS, *by* HORNELL HART, Associate Professor of Social Economy, Bryn Mawr College. Pp. 708. 24 Chapters. 17 Illustrations. New York. Henry Holt and Company. 1931.

Covers somewhat the same ground as the preceding, but with a less morbid outlook.

BROWN AMERICA. The Story of a New Race. *By* EDWIN R. EMBRE. Pp. 311. Price, $2.50. The Viking Press, New York. 1931.

The Director of the Rosenwald Foundation out to "sell" race mixture—

FERTILITY AND ANIMAL-BREEDING. *By* F. H. A. MARSHALL, Sc.D., F.R.S., and JOHN HAMMOND, M.A. Pp. 50. Price 45c. His Majesty's Stationery Office, London. 1932.

The rock on which many a genetic experiment has split.

HUMAN STERILIZATION, The History of the Sexual Sterilization Movement. *By* J. H. LANDMAN, PH.D., J.D., J.S.D., The College of the City of New York. Pp. 341. Price, $4.00. The Macmillan Company, New York. 1932.

Extensive documentation of legal phases. Lengthy discussion of heredity, with a peculiar twist, that if applied by a plant or animal breeder would put him in the poorhouse in short order.

THIRD INTERNATIONAL CONGRESS OF EUGENICS

At the American Museum of Natural History, 77th Street and Central Park, West, New York City, August 21-23, 1932

Exhibition at the Museum, August 22-September 22
Excursion to Cold Spring Harbor, August 21

THE First International Congress of Eugenics, which was sponsored by the Eugenic Education Society of Great Britain, was held in London in 1912, under the presidency of Major Leonard Darwin. The Second Congress of this series met in New York in 1921, under the presidency of Henry Fairfield Osborn. The Third International Congress of Eugenics will be held in New York City in August, 1932, under the presidency of Charles B. Davenport, Director of the Department of Genetics of the Carnegie Institution of Washington and organizer of the Eugenics Record Office.

Scope of the Congress

It is the aim of Congress, by means of papers, conferences and exhibits, to review briefly the history of eugenical work, and to present a survey of the present status of eugenics, both as a pure and as an applied science. If its work is well done it will serve to clarify the principles and aims of eugenics, and to point out the most profitable lines of eugenical endeavor for the next decade. The Congress will strive to mark a mile-post in eugenical research and also to present to the public the real meaning and content of the science of eugenics and an appreciation of its importance in human affairs.

Participation in the Congress

The Managing Committee of the Third Congress is anxious to establish early contacts with all persons in all countries who are interested in eugenical research and in race and family-stock betterment. It is hoped that this Congress will take full and critical stock of eugenical progress. In order to do this it must have wide and earnest support; it must be participated in by the outstanding students of human genetics, migration, mate selection, differential fertility and those forces which influence the turn-over of population quality from generation to generation. It invites friendly contact with, and participation in its work by, investigators in the contributing sciences—particularly anthropology, psychology, physiology, medicine and education. It welcomes collaboration also with those business houses and industries the prosperity of which depends most heavily upon scientific human capacities.

The Two Congresses

The Third International Congress of Eugenics, New York City, and the Sixth International Congress of Genetics, Ithaca, New York, are working in close collaboration. Papers on human genetics will be read at Ithaca, while all other phases of both pure and applied eugenics will be centered at the Eugenics Congress in New York.

Visit to Cold Spring Harbor

The members of the two Congresses will be taken on an excursion to Cold Spring Harbor on Sunday, August 21st, to visit the Eugenics Record Office and

the Station for Experimental Evolution, which, together, constitute the Department of Genetics of the Carnegie Institution of Washington.

Publications

It is planned to cover the history and proceedings of the Congress appropriately in a published report. This report will give in full the more important papers read before and submitted to the Congress, and will give an account of the proceedingsof the Congress and a description of the exhibits.

Eugenics Exhibition

An exhibition covering the history and present status of eugenical research will be held at the Museum in connection with this Congress. It is planned to open this exhibition on August 22d and to continue it, open to the public, until September 22d.

CLASSIFICATION OF EXHIBITS: This classification is meant to suggest, in logical relationship, the main subjects of investigation in the field of eugenics. Appropriate exhibits in any of these classes may be offered for display and demonstration.

Class 1. Human Traits or Qualities.
Class 2. Human Genetics.
Class 3. Heredity and Environment.
Class 4. Heredity and Development.
Class 5. Human Migration.
Class 6. Mate Selection.
Class 7. Differential Fecundity.
Class 8. Population Study.
Class 9. Eugenical Forces.
Class 10. Other Sciences in Relation to Eugenics.
Class 11. The Races of Man.
Class 12. Human Family Stocks.
Class 13. Parallel Between Improvement by Better Breeding of Plants and Animals and of Human Family-stocks.
Class 14. Applied Eugenics.
Class 15. Eugenical Organizations and Publications.

MATERIALS: The materials for this exhibit will be shown principally in the form of (1) pedigree charts and their genetical analysis; (2) statistical charts and tables; (3) photographs and maps; (4) models, casts and drawings; (5) books and scientific papers; (6) scientific apparatus, and (7) living specimens.

SPACE: The exhibit will be held in the Education Hall of the American Museum of Natural History where approximately 5,000 square feet of wall space, in 14 booths, are available for this display. Besides this wall space there are in this hall many hundreds of square feet of floor space available for and well adapted to exhibition cases and tables.

Membership in the Third Eugenics Congress

The following classes of membership in the Congress have been established: Active membership, \$5.00; Sustaining membership, \$25.00; Supporting membership, \$100.00; and patrons, those who contribute \$500.00 or more. Both individuals and institutions are eligible to membership. Make all checks payable to the Third International Congress of Eugenics.

Each member will be entitled to all privileges of the meetings, exhibits and entertainments of the Congress, and will receive, without further charge, one set of the publications of the Congress.

The Managing Committee of the Congress will pass upon and either definitely accept or reject each application for membership.

Applications for membership and inquiries concerning the Third International Congress of Eugenics may be addressed to Harry H. Laughlin, Secretary, Cold Spring Harbor, Long Island, N. Y.

The

Journal of Heredity

Vol. XXIII MAY, 1932 No. 5

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COMING

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Maize-relative reappears, or is refound, in Guatemala after many years.

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Heredity dwarfism suggesting thyroid deficient types in other forms.

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Another nerve defect that is inherited.

Identical Triplets

Variations in degree of identity as exemplified by finger-print patterns. The most strikingly similar set had multiple birth membranes.

White Sheath in Maize

A character controlled by two complementary factors.

Wheat-Emmer Hybrids

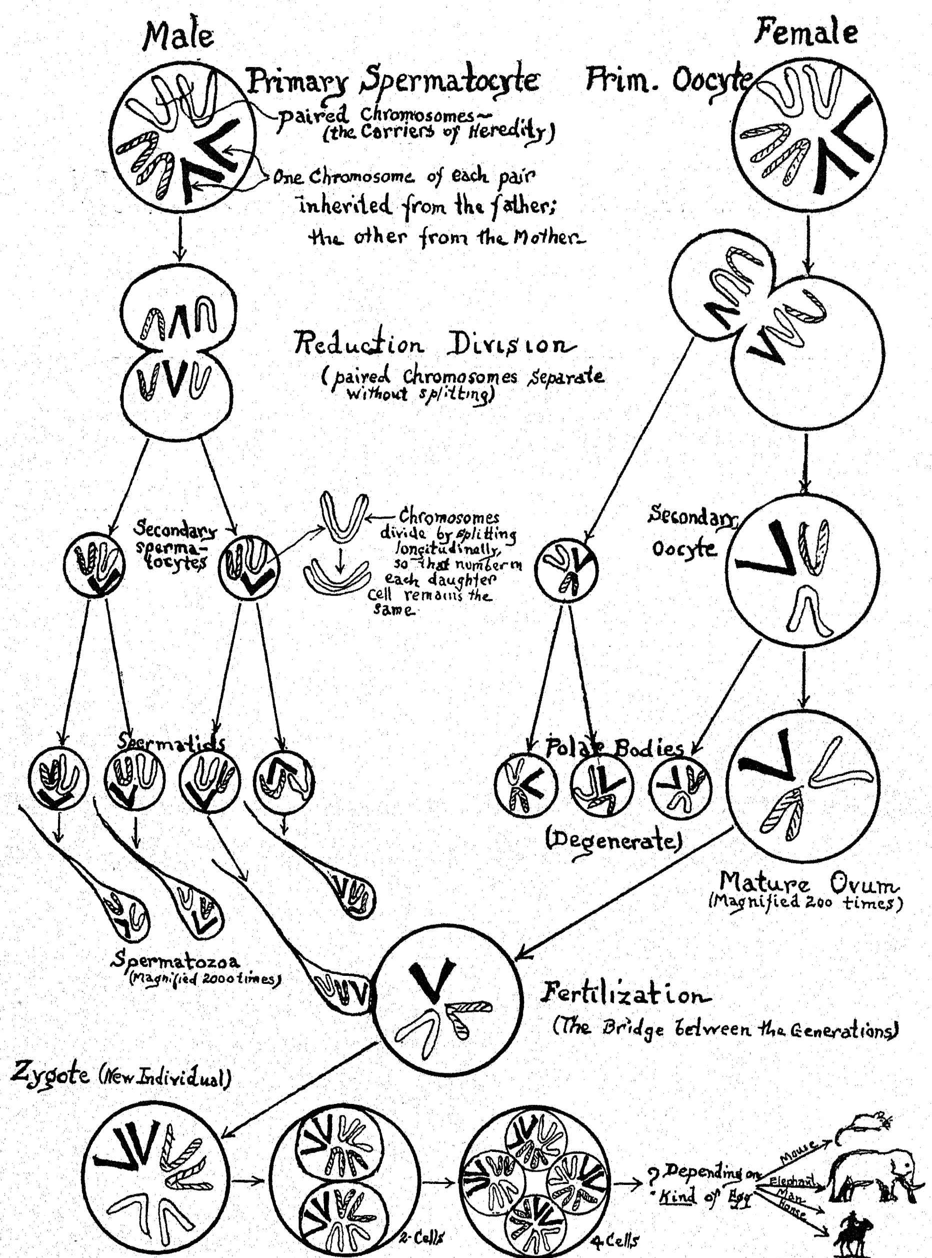
Species hybrids of certain varieties show an unusual degree of fertility.

Wooly-Haired Nordics

A dominant mutation, producing in a blond race a frizzle-haired condition characteristic of the negro races.

The JOURNAL OF HEREDITY is published monthly by the AMERICAN GENETIC ASSOCIATION, at 1918-32 Harford Avenue, Baltimore, Md., and Victor Building, Washington, D. C. Address all communications regarding editorial matters and the business management of the Association to Room 306, Victor Building, Washington, D. C. Entered as second-class matter February 24, 1923, at the postoffice at Baltimore, Maryland. Acceptance for mailing at the special rate of postage provided for in section 1103, Act of October 3, 1917, authorized December 20, 1918.

$3.50 a Year *Prices of back numbers on application* *35c. the Copy*



THE MECHANISM OF FERTILIZATION

Frontispiece

The one-celled stage through which all the higher forms of life pass is shown diagrammatically here. The spermatocytes and oocytes develop from the germ-tract, which is made up of undifferentiated tissue set aside early in the embryonic development of the higher animals. Individual cells in this germinal tissue develop to form the mature germ cells, and above are outlined the more important features of this process: the reduction division, in which the paired chromosomes separate; and fertilization, when the full complement of chromosomes is reassembled by union of ovum and sperm. Actual photographs of egg and sperm cells are shown in Figures 1 and 2. The sperm cells in this drawing are ten times as much magnified as the egg cell, to show the details of chromosome transmission. The adult organism may be made up of billions of cells, of such form and function as to constitute a mouse, an elephant or a Shakespeare, but in this single tiny fertilized egg cell is contained *all the potentialities of the final form*—and nothing can be *inherited* in the biological sense except what is transmitted by these microscopic objects. (In the higher animals many more pairs of chromosomes are present, but the principles are the same.)

A ONE-CELLED COW

Photographs of Bovine Eggs Illustrate the Microscopic "Living Bridge" Which Joins the Generations

IT is hard to realize that with all the complexities life has attained, and with all the specialization of structure and function that has been developed, the living link that joins one generation to the next is still today, as it was millions (even billions?) of years ago, a single microscopic cell. That this is a biological truism makes it no less marvelous when we consider that all the hereditary potentialities that are transmitted from generation to generation cross a Bridge of Life so tiny that in mammals one component of it is altogether too small to be seen by the human eye, while the other is just on the verge of visibility. Mice and elephants, giraffes, and tapirs and humans, all go through this invisible single-celled haploid "generation," in which the tails, trunks, necks, noses, and brains, and all the multitude of other distinguishing differences, are preserved with an incredible precision of microscopic detail.

Under the microscope it is safe to say that it would be extremely difficult to tell an elephant in the one-celled stage from a monkey or a tapir or a giraffe. Hidden in this tiny object are much finer details than those that separate genera. Given a dozen fertilized cow eggs, could any one tell by the most refined methods which will be a Holstein, which a Jersey, which a bull, which a heifer, which will yield 5,000 pounds of milk and which 20,000? Given an array of human eggs, the detection of giants and dwarfs, either mental or physical, would be altogether impossible. Extremely fine details of structure and function are just as accurately and irrevocably determined—pits in the ear, webs between a certain pair of toes and such disasters as Huntington's chorea that may not become manifest for two-score years or more. There is room for all this in a speck of protoplasm whose volume is measured in cubic microns!*

One wonders what would be the effect on the philosophy of the next generation if this story of the Perpetuation of Life could be properly, dramatically and adequately expounded. If our billion dollar school system could really, livingly and vitally convey this one fact, the effect might be almost incalculable.

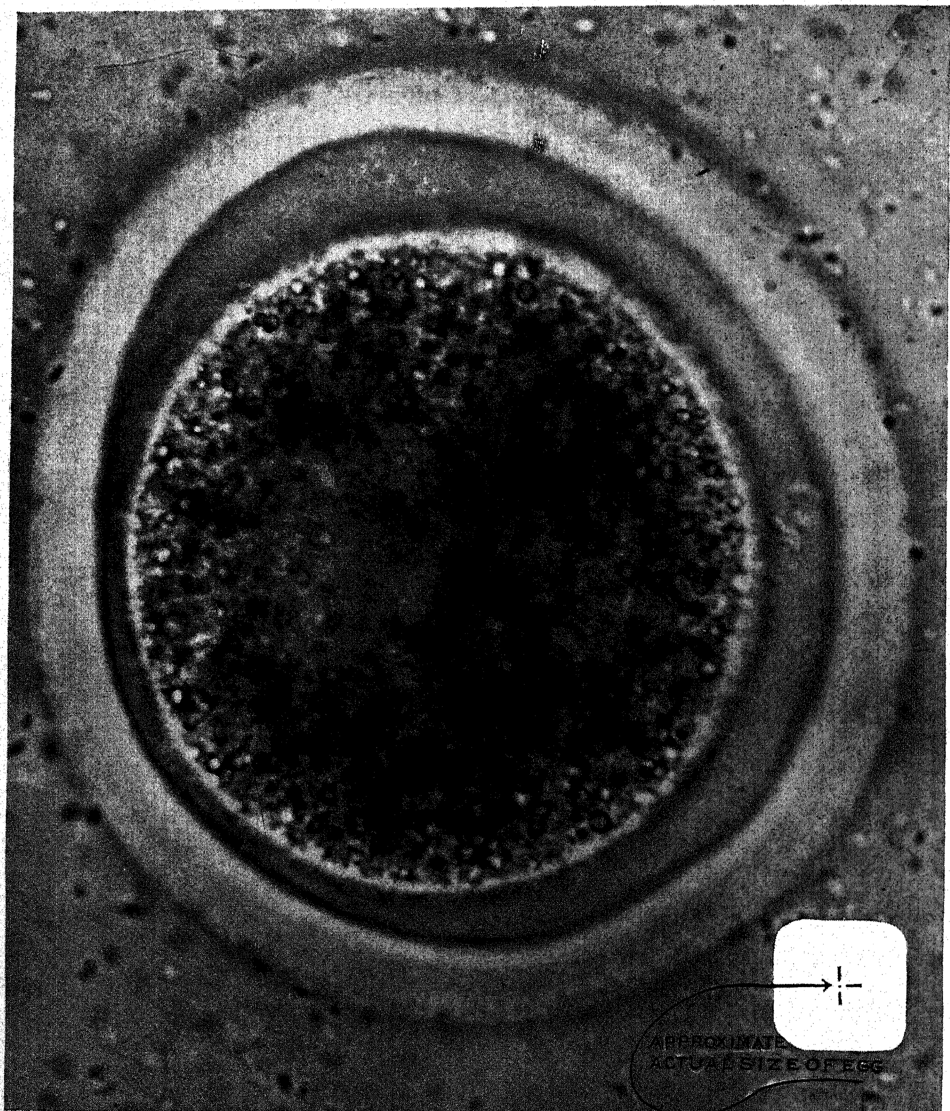
The occasion for this "sermon" is a recently published report† describing the "capture" of two cows, one in the single-celled haploid stage and the other in a more advanced stage of development boasting two complete cells and a complete "diploid" set of chromosomes. The details of this discovery are of considerable interest in the light they throw on the reproductive cycle in cattle. Important as this information may be, it is overshadowed in popular interest by the "portraits" of these one- and two-celled cows, which accompanied the article. Through the courtesy of the Bureau of Dairy Industry these are reproduced herewith.

Size of Egg and Sperm

The estimated diameter of the single-celled egg (Figure 1) is about 143 microns. When it comes to forming a mental concept of what they are, microns are about as unsatisfactory as light-years. In terms of familiar objects, just what does 143 microns mean? The paper on which this is printed averages about 275 sheets to the inch, or roughly 11 sheets to the millimeter. Thus 11 sheets are about 1,000 microns thick, and each sheet is about 90 microns thick. The diameter of the egg, in-

*A micron is one-twenty-five thousandth of an inch.

† MILLER, FRED W., W. W. SWETT, CARL G. HARTMAN and WARREN H. LEWIS. A Study of Ova from the Fallopian Tubes of Dairy Cows, with a Genital History of the Cows, *Journal of Agricultural Research* 43: (7) 627-636. 1931.



Photograph by C. C. Hartman and Warren H. Lewis

A ONE-CELLED COW—THE BOVINE EGG

Figure 1

The one-celled stage of the cow is almost invisible to the naked eye. The small dot, in the center of the white square, is approximately the size of the actual egg. This photograph, which is of the first unfertilized egg to be discovered, is greatly enlarged (750 diameters). Were it possible to imagine the cow that bore it enlarged to the same scale, she would be higher than the Empire State Building and her shadow at noon would cover a 240 acre farm. In the center of this remarkable photograph is the granulated protoplasm which conceals from view the carriers of heredity—the chromosomes. Two envelopes surround this precious bit of living matter and protect it. They appear as a lighter and a darker zone in the photograph. It is an amazing fact that this almost invisible particle is a cosmos of activities and that it carries in it—transmits as they say—half of those entities called chromosomes which, paired with similar entities from the sperm cell of the male, determine the characteristics of the calf.

Photograph from Bureau of Dairy Industry

THE TINY SPERM CELLS CONTRIBUTE HALF THE HEREDITY

Figure 2

The sperms, swiftly swimming cells (in proportion to their size), are vastly smaller than the egg. A hundred of them are shown in this photograph. They are produced with prodigal abundance, and in the process of fertilization swarms of them may swim about the egg cell, but only a single one succeeds in penetrating the envelopes and burying itself in the protoplasm of the egg. (Magnified about 1,500 times.)

The entities called chromosomes within the egg cell, upon the arrival on the scene of the chromosomes of the male (which are packed into the head of the sperm cell) begin immediately to range themselves in a definite pattern, suggesting the struts and beams of a complicated structure—which in fact this incredibly tiny object is—a real living bridge.

cluding its envelopes is approximately represented by the thickness of the edges of two sheets.*

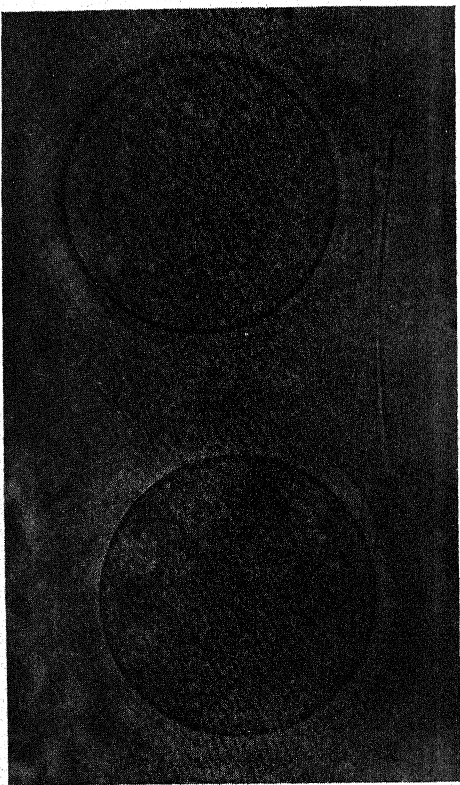
An attempt is also made in the illustration (Figure 1) to convey an idea of this size. In the white space in the corner is a single dot at the intersection of the guide lines. This is a square, of which the edges have a length of between 150 and 200 microns. The dot would be expected to increase somewhat in size during the printing process. It is realized that this is only a rough approximation which will vary somewhat from one impression to the next, but it serves to give an idea of the minute dimensions of the most important and valuable form of matter—living or non-living—the reproductive cell.

Small as it is, a mammalian egg is fairly well filled with "waste material" from the point of view of hereditary transmission. The volume of the cow's egg shown in Figure 1 is just over a million and a half cubic microns. Contributing one-half the inherited characteristics and thus equally potent from the point of view of heredity, are the tiny sperm cells (Figure 2), which at fertilization furnish a complimentary set of chromosomes and initiate the development of a new individual. This picture is enlarged twice as much as the illustration of the egg in Figure 1.

ERROR IN LEGEND TO FIGURE 3

The cut for Figure 3 is reversed, so that the text of the legend reads incorrectly. The sentence in lines 8 and 9 should read: "A single spermatozoon is shown to the *right* of the *upper* egg."

half may be 50,000 times as large as the other! About 8,000 of the sperm cells could be laid side by side in one inch. Each of them contains the potentialities for the production of a new individual. Is it inaccurate or irreverent to feel that we have in this process actual miracles that are still entirely beyond our understanding?



Photograph by Joseph Long

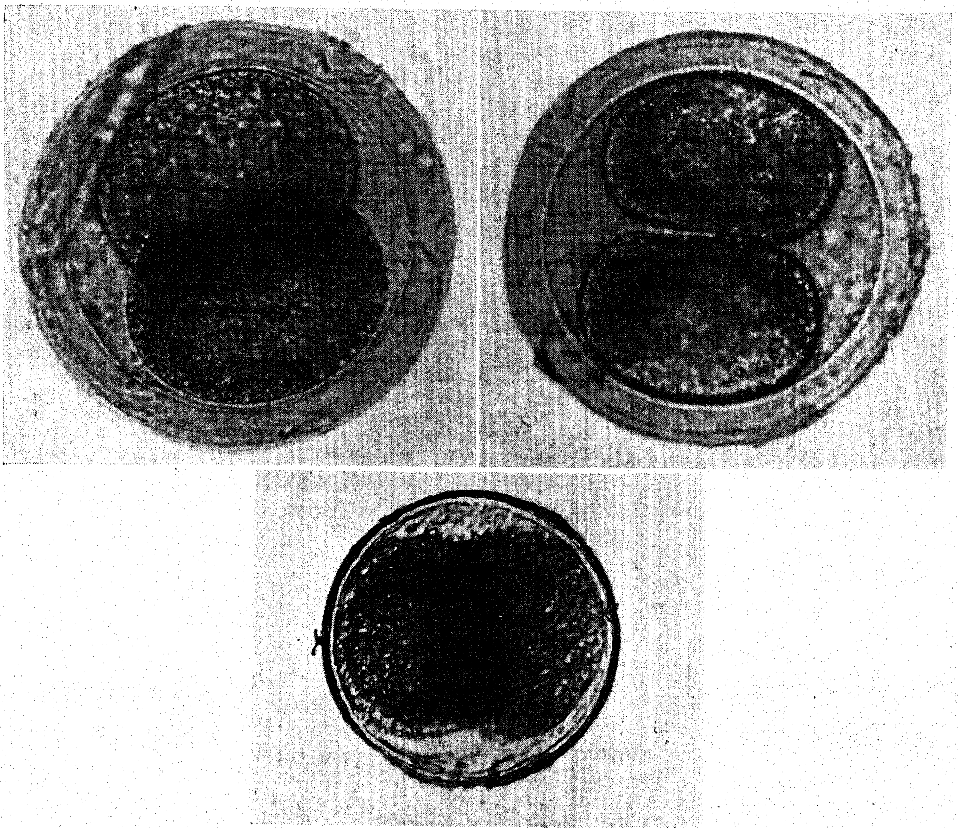
RATS EGGS

Figure 3

Two living rat's eggs, for comparison with the cow's eggs shown in the other figures. These eggs are somewhat smaller than a bovine egg, being enlarged about 600 times. Their appearance is such that they could hardly be identified as rats, although it might be possible to decide the eggs were not cow's eggs. A single spermatozoon is shown to the left of the lower egg. There are greater specie differences in sperm cells than in egg cells, and this cell can be seen to have very different proportions than the bovine sperm cells shown in Figure 2.

Surrounding the central portion of the egg is the zona, a covering, probably a secretion from the egg itself. The vital part of the egg is the dark central portion, which contains the chro-

*This method of visualizing the size of cells in terms of a readily available measure was suggested recently by Dr. Edgar Anderson (*Science* 75: 359, April 1, 1932).



Photographs by C. G. Hartman & Warren H. Lewis

THE BRIDGE CROSSED—A COW IN THE TWO-CELLED STAGE

Figure 4

Fertilization of the egg has taken place and the new complement of chromosomes has initiated the complicated series of events which would eventually result in a new individual. Two views of the egg are shown as it appeared when it was recovered from its normal environment. The shrunken object below is the same egg after it had been placed in a preservative—emphasizing that the student of killed material does not see the same thing as the student of living material. The photographs are enlarged about 325 diameters.

The details of structure transmitted in these germ cells determine the smallest characteristics of the offspring, such as nicks in the edge of the ear, shape of the udder, amount of milk produced, etc. The genes carried in egg and sperm determine whether the resulting calf would have been a Holstein or a Jersey, whether it would have been horned or polled, black, red or roan. Whether the resulting calf would have been a bull or a heifer would be determined by the sperm cell which penetrated the egg, for half of them produce females and half males, the eggs being all alike in their sex-determining potentialities.

It seems a great pity that the profound importance of this universal bridge of life should not have taken a larger place in the consciousness of the race, for a clear understanding of its tremendous significance is a necessary first step toward the production of the selected, super races of mankind which will, if the race survives man-made obstacles to the perpetuation of his own species, some day inhabit the world.

matin, the material that actually transmits the inherited characters from one generation to the next.

While the sperm cell is relatively "pure heredity" it doubtless contains some waste material. The British biologist, Haldane, has pointed out that the chromosomes of the entire human race in the single-cell stage could be put in a thimble with ample space to spare. In this thimble would be all racial differences, all potential genius and imbecility, all the differences in eye, hair, skin, and teeth which are passed on from one generation to the next—all the hereditary talents and afflictions which belong to our race. All of these, in being transmitted to the next generation, pass through this one-celled stage, which must be so greatly magnified merely to enable us to *see the outside* with the human eye!

Size of a Gene

Even the chromosomes, small as they are, are by no means "pure heredity." Muller has likened the structure of the cell and the chromosomes to that of a city. From the point of view of mass and volume the buildings, streets, and bridges make up by far the greater part. The all-essential *people* represent only a small fraction of the total volume of the more imposing structures surrounding them, but of which these same insignificant individuals are the architects. Similarly the all-important gene represents only a small fraction, not only of the cell, but probably of the chromosome as well. We have progressed beyond the point discussed by the Athenians—the priority of owl or egg. As Weinstein points out, we realize today that eggs not only make owls and hens and humans, as a convenient way of making more eggs, but genes use the amazingly complicated living structures which we are, and which we see about us, as the most satisfactory way they have found to make more genes. It is these ultimate tiny particles of living matter that control the entire course of life. The eggs of a cow, an elephant, a giraffe, and a human perhaps could not be told apart by microscopic examination. Given the proper conditions the genes within them will differentiate the eggs and give us the organisms familiar to us in Natural Histories. Alter the genes and the animal changes, but known alterations of environment will not have any significant effect in making an elephant's egg turn out a mouse or a marmosat as an end product. In spite of all that has been claimed for the power of the environment, the only way to prevent an elephant's egg from fabricating an elephant is to destroy the egg. Nothing less will do.

Just how large are these vitally important potentialities, the genes, stored away in so tiny a space? By computing the volume of the chromosome and by dividing this figure by the minimum number of genes, which number is probably much too small, we arrive at a maximum size for the gene. Muller has computed that on this basis a gene would have a diameter not greater than one sixteenth of a micron, and probably much less. Muller has also called attention to the fact that ordinary concepts of cell division fail to apply when we consider the reproduction of a gene. A cell grows to a certain size —swells, we ordinarily say—and then divides, with an attendant longitudinal splitting of the chromosomes somewhat analogous to what happens when a "zipper" is "unzipped." The resulting daughter cells and daughter chromosomes increase to normal size and the process is repeated. In the case of the genes the concept of swelling and dividing falls down, and we must visualize a more intricate and exact mechanism.

The copper half-tone plates with which the illustrations accompanying this article are printed will make up to 50,000 replicas of the illustrations etched in hills and valleys on their surfaces. By that time the details would begin to fade out, until, if the number of impressions be tripled or quadrupled only smudges would remain. Other examples of replica-formation, more

persistent than this, might be cited in industrial processes. None are known which equal in accuracy the replica-formation which genes regularly display, *where exact replicas are made by the million*. Here is a process that cannot be fitted into the ordinary concept of "swelling" and the formation of a cell wall.

It is hard to think of the process of gene duplication as being carried on by a group of molecules. It is of a nature that can be most readily rationalized if we consider the process to be carried on by a single unique molecule. Some of the forces operating in a cell are not of a kind known to physics. Chromosome behavior at cell division and in the formation of sex-cells shows "likes" attracting at one time and repelling at another. Thus, it is not altogether fantastic to consider that this gene-molecule of a definite atomic pattern is able to draw from the surrounding medium the constituent atoms necessary to make another molecule of exactly identical arrangement. If this is a true picture of what happens the gene is unique in its ability in replica-formation for, on the average, a million or more "impressions" are produced without any visible alteration in the resulting "product."

When, the one time in a million, the pattern is not exactly reproduced, a mutation is the result. This new atomic arrangement may go on reproducing itself exactly and indefinitely. At the same time the old pattern is continuing to be produced by the gene-die that made the mutant error. In all the world about us there is hardly a more amazing example of a precision of mechanism than this, and in a field where it is ordinarily thought that a vague hit-or-miss process can be considered an adequate explanation of the processes of cell reproduction.

A decade or so ago geneticists were content to describe the end-product of gene action, the "finished character" in the adult organism. Modern tendencies in research reflect the unsatisfactory nature of this method. More and more effort is being concentrated on following through the activities of the genes, from the time they initiate their activities in the fertilized egg cell, to the final appearance of the adult characteristics. As yet, however, only a beginning has been made in a real understanding of these processes. We know, for instance, that the lethal "creeper" gene in fowls does not appear to affect the early development of the embryo, but kills during the third day of development. We know of other genes whose lethal effects are not manifested until birth or even later. Even in many of these relatively simple cases we yet know practically nothing about *why* these genes are incompatible with continued existence. In this fascinating and important field of what might be called gene-ontogeny a beginning has been made—nothing more.

Miracles of old, even the miracle of resurrection, are hardly to be considered more amazing than this continuing miracle of every day—the resurrection of life from the separated and supplementary halves necessary to initiate this wonderous process of growth and differentiation. Is not this passage through the early stages of growth amazingly like the passage of the Valley of the Shadow of Death? From the complex forms of life today, back to the first primal cell, the invisible bridge extends—over the Valley of Oblivion—unbroken.

Robert Cook.

From Egg to Egg

DAS DETERMINATIONSPROBLEM IN ANALYTISCHER DARSTELLUNG, by Adolf Cohen-Kysper. Abhandlunmen zur Theorie der organischen Entwicklung, No. 5. Pp. 48, Rm 4.80. Julius Springer, Berlin, 1930.

This compact sketch outlines the current answers to the question, why does a fertilized egg produce what it does produce, and how? It goes, therefore, to the very heart of genetics.

Paul Popenoe.

Anthropology for Sociologists

THE MAKING OF MAN, AN OUTLINE OF ANTHROPOLOGY. Edited by V. F. CALVERTON. Pp. vii + 379. The Modern Library, New York, 1931.

THE book is a small octavo with thin leaves, narrow margins, and rather flexible board covers, which will fit one's side pocket, if desired. It is a compilation of standard Anthropological literature.

The editor wrote the introductory chapter which is entitled "Modern Anthropology and the Theory of Cultural Compulsives." He here stresses the informing idea of the book that man is a product of the culture of his social group in all matters except those of biological heredity. He might have added with equal truth that in hereditary biological factors many groups of men are also largely the product of their cultures, because the dominant social forces of artificial selection of mates has produced in some tribal groups, as the field Anthropologist knows them, quite as distinct "breeds" of people as domestication of animals has produced "breeds" of domestic animals. The editor also stresses the fact that Westermarck's theory of marriage has now been battered down by the data published by Briffault—to whom the volume is dedicated.

The volume contains a chapter of five papers on "Fossil and Prehistoric Man." The second chapter is composed of two papers on "Race and Language." The third has fifteen articles on "Social Organization." The fourth is entitled "Sexual Customs and Social Practice" and carries six papers. There are six articles in the fifth chapter entitled "Religion." And there are five short articles in the sixth and last chapter named "Evolution of Attitudes."

The selection of material reprinted in the compilation is quite as good as could be expected in so extensive a field as covered. If the articles on fossil and prehistoric man had been left out, and if there had been a section devoted to economic life, the goal of the volume would have been reached better and with more singleness of aim.

It would have been possible to have summarized the findings of the various articles reprinted. I believe such a summary, say as a last chapter of the book, would have been proper and of great value to students.

The editor definitely says the book will have served its purpose if it "helps social scientists . . . get a better and more informed and various idea of the nature of primitive man and the theories concerning him." Though I hold no brief for the Department of Sociology in the University where I reside, yet I am certain Minnesota Sociologists are much better informed in the Anthropological field than the editor believes American Sociologists to be.

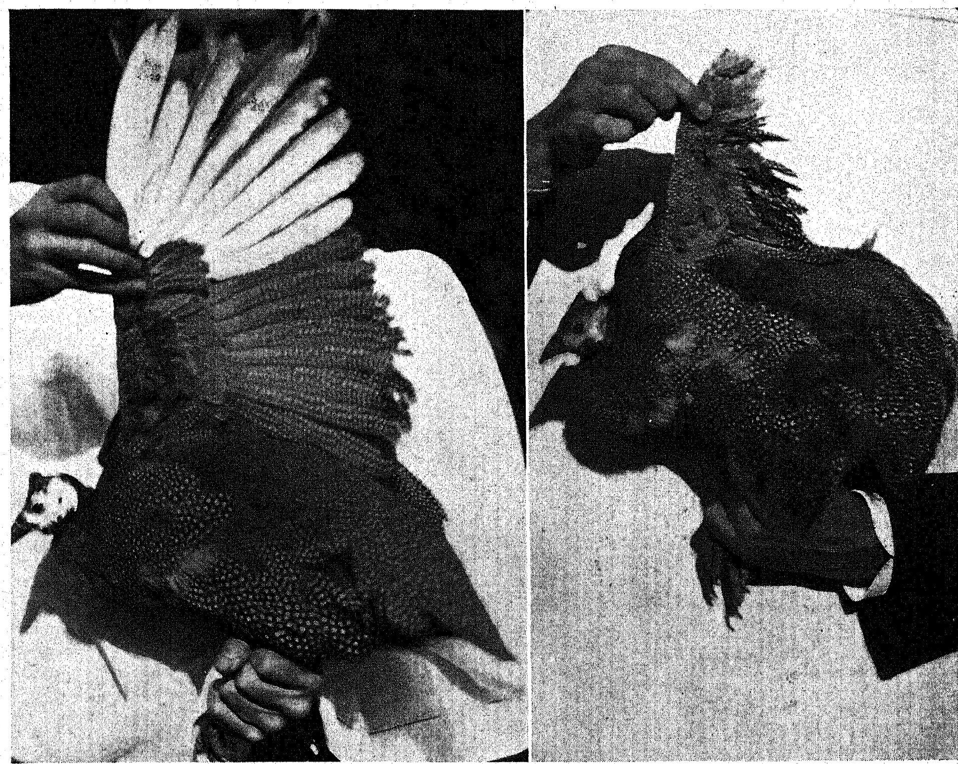
The book has no index—which, of course, all scientific books to be used as tools should have. Why an editor of the experience with which *Who's Who* credits Mr. Calverton could have decided that his book would have full student use without an index, detailed table of contents or an analytical summary, is a mystery to me. There is a short standard bibliography, and good foot-note references follow most of the chapters. The book should be of value to Sociologists, both instructors and students, because it has assembled so large an amount of Anthropological data necessary as a foundation for Sociology. I wish also that it might interest more Sociologists to leave the easy chair and take pot luck with field Anthropologists.

ALBERT ERNEST JENKS.
University of Minnesota

THE "PENGUIN" GUINEA FOWL

Absence of Flight Feathers Due to Hereditary Local Alopecia*

EDWARD McCRADY, JR.
Wistar Institute of Anatomy and Biology, Philadelphia, Pa.



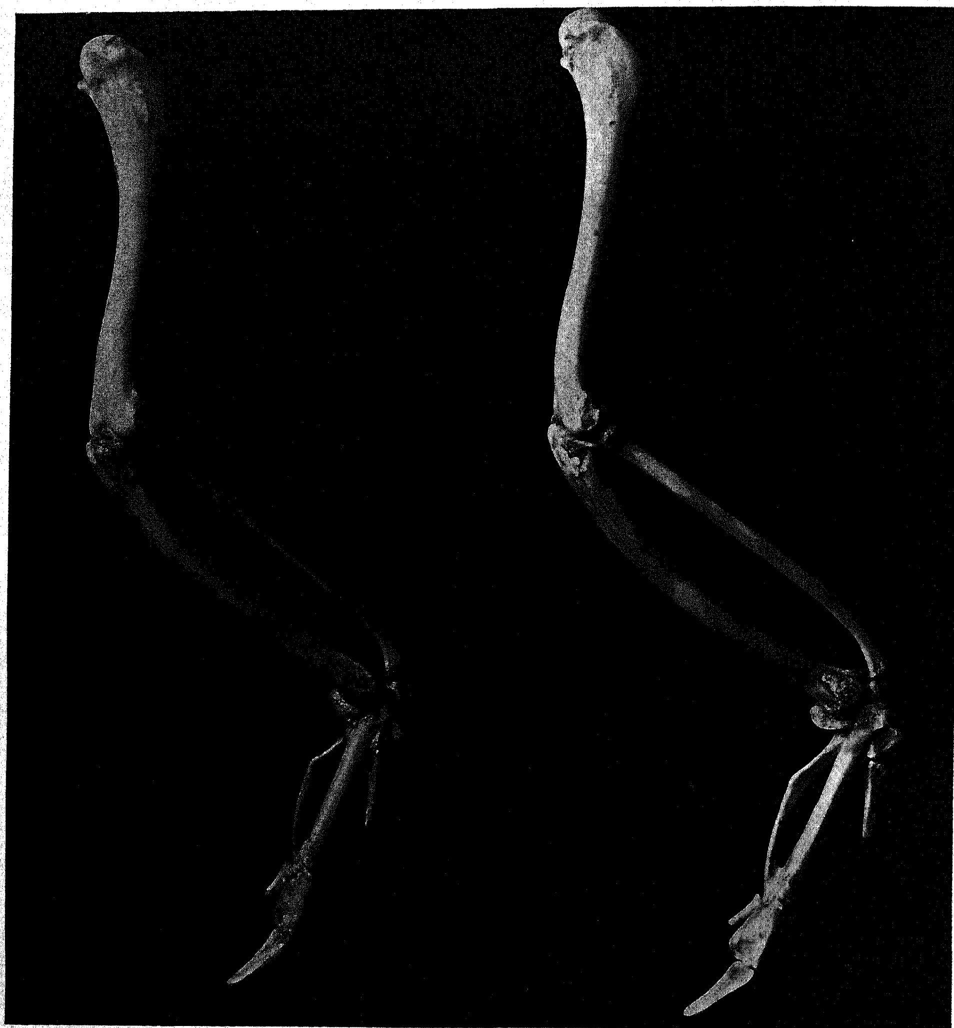
A "WINGLESS" GUINEA

Figure 5

At the right is a normal guinea cock nine months old; at the left a "Penguin" cock of the same age, and photographed at the same scale as the other. Note that the flight feathers of wing and tail are missing; otherwise the plumage is normal. A slight skeletal defect may also be produced by this gene (Figure 7).

IN the Spring of 1930 three cocks and three hens of the Guinea Fowl (*Numida meleagris*) were brought from Fairview Farm near Bristol, Pa., to the adjoining Bolton Farm. These animals were phenotypically normal in every respect. Being monogamous they were segregated into three pairs. After mating in April two of the pairs produced nothing but normal offspring. The third pair produced a brood of twelve, four of which showed a striking peculiarity which the farm manager noticed and described as "winglessness." In the Autumn of the same year two of the

*I have referred to the condition to be described in this paper as Alopecia after the precedent of C. H. Danforth[1], who in 1928 used the term to apply to a feather defect for the first time. However, the term is too broad to apply specially to the character here concerned, and so another name is proposed for this purpose in the course of the paper.

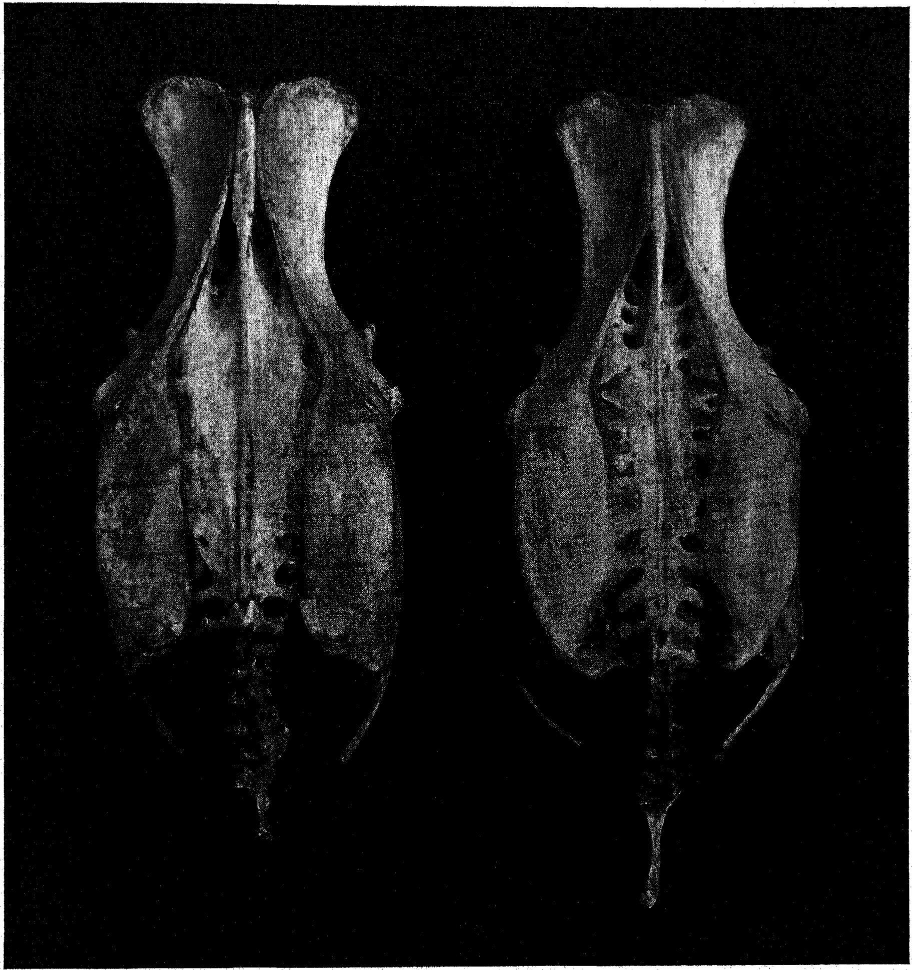


WINGS OF NORMAL AND "PENGUIN" GUINEAS

Figure 6

Skeletons of right wings of guinea cocks, that of the "Penguin" mutant at left and of a normal cock at right. The "penguin" gene affects only three rows of feathers on the edge of the wing, and homologous feathers in the tail. The wing bones are not affected. The mutation is not in any sense "winglessness" though the birds are so called on account of their peculiar appearance.

four were killed by accident so that no information can be had concerning their sex, but of the two surviving, one was male, the other female. It was at this time that I heard of the peculiarity. I examined the male of the pair closely, and found that instead of being a true amelus or phocomelus, it exhibited a peculiarity of a new type in which the remiges, the long feathers of the wings, and the rectrices, the corresponding feathers of the tail, were absent. The wing proper was present and apparently normal, though when not extended it was considerably covered up by the contour feathers of the body. One would not suppose on first thought



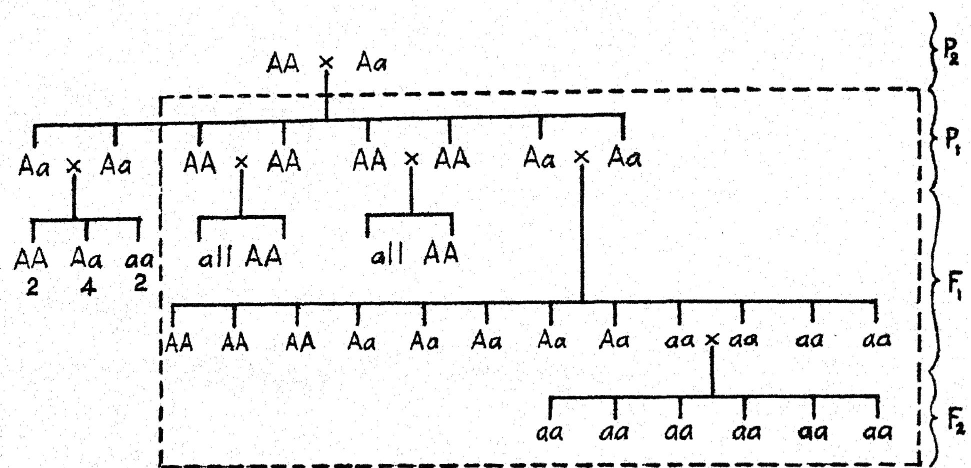
PELVIC BONES POSSIBLY AFFECTED

Figure 7

The caudal vertebræ of the "penguin" guinea cock (left) show definite abnormality, as compared with normal (right). The small number of individuals available has made it impossible to carry on further dissections, and until this is done it cannot be finally concluded that this is another manifestation of the same "penguin" gene which affects the flight feathers.

that the absence of the flight feathers could make so extraordinary a difference in the appearance of the bird; but actually when the wing is not extended, it is easy to understand how such an individual could be thought to be entirely wingless, and even when the wing is extended it seems disproportionately short as Figure 5 will bear witness. I shall reserve the more complete description of this specimen for another section of this paper.

Since there were only two of these anomalous guineas alive in the autumn of 1930, it seemed advisable to mate them to one another, with a view to preserving the stock, if the anomaly should prove inheritable, and also, in this event, gathering the first data for the analysis of the genotype. They were allowed to run together



INHERITANCE OF WING AND TAIL DEFECT

Figure 8

The individuals designated as *Aa* in the F_1 are phenotypically normal, and their heterozygosity has not been tested by breeding. All other data in the chart have been definitely ascertained. The dotted rectangle separates the Bolton Farm group (inside) from the Fairview Farm group (outside).

all winter. The male showed some aversion to the queer looking hen, and seemed to prefer to mate with a normal one; but not being given opportunity for this, in April, 1931, he mated with her. Many eggs were laid, but only six hatched. Four of these died before maturity; two are still alive at the time of writing.

Meanwhile two more mutants of the same sort have appeared on Fairview Farm, the original home of this stock. An examination of their ancestry shows that they belong to the generation (designated F_1 in the Chart) corresponding to that in which the first mutants appeared on Bolton Farm, and that they had the same grandparents.

All the specimens have now been removed to the Morris Biological Farm of the Wistar Institute, where I am continuing breeding tests with them. However, from the data given above it is already possible to make a fairly conclusive analysis of their genotype.

Interpretation of Breeding Data

The fact that the peculiarity appeared for the first time in six members of one generation indicates either that the mutation causing it took place six times separately and simultaneously, if heterozygous, and twelve times, if homozygous; or that it took place in a previous generation. Since the F_2 breeding data show that it is homozygous, it would be necessary in this instance to assume that it had occurred twelve times according to the first of the alternatives. However, as mutations under normal conditions are at best rare, and identical mutations even rarer, the simultaneous and independent occurrence of twelve identical mutations in one generation is certainly exceedingly unlikely. But if the second of the alternatives be the correct one, i. e., if the mutation took place in a previous generation, the new gene is necessarily a recessive; inasmuch as it did not express itself phenotypically in any members of the parental generation. Furthermore, it can not be located in the sex chromosome, for, if it were, even though recessive, it would have shown itself phenotypically in the digametic parent (in fowls the female). Since, then, it did not appear at all in the parental generation, and appeared in six individuals simultaneously in the first filial, and bred true in the second

filial, we must assume that it is an autosomal recessive, and that the individuals that show it are homozygous.

If, with these data in mind, we designate the mutant gene *a* and its normal allelomorph *A*, we can chart what we believe to be the case as is shown in Figure 8.

It will be seen from this chart that the expected ratios are very well fulfilled. In the P_1 the progeny of $Aa \times AA$ are all normal in appearance, and show an exact 1:1 ratio of homozygous and heterozygous individuals as proved by breeding. The fact that the heterozygotes in this generation bred with each other and not with homozygous normals was due to accidental good fortune. In the F_1 from $Aa \times Aa$ in the Bolton Farm group the ratio of normal phenotypes to mutants is eight to four, or two to one, instead of the expected three to one. But, of course, if a single one of the *aa* had been *Aa*, a perfect three to one ratio would have resulted; so the discrepancy is probably due to sampling in small numbers. In the Fairview Farm F_1 of the same cross an exact 3:1 ratio is realized. In the descendants of all $AA \times AA$ matings the expected 100% normals was realized. And also in the F_2 progeny from $aa \times aa$ only the expected 100% mutants appeared.

Discussion

It now remains to describe the anatomical peculiarities of this mutant in more detail, and to compare it with related mutations in poultry.

Though the lack of the long feathers makes the wing look so small that it suggests phocomelia, or at any rate ectromelia, dissection shows that no such condition is present. Phocomelia and amelia, the complete absence of the limb skeleton, are both known in chickens. Kirkham and Haggard[7] in 1915 described a wingless rooster, and showed by breeding tests that the condition was not genetic. They assume that it was caused by abnormal temperature variation in the incubator, and cite one case of a wingless hen possibly affected by the same factor. Terry[14] in 1919 described another wingless hen. In each of these cases there was no wing skeleton present at all, and the condition was a non-hereditary developmental anomaly. I believe there has never been described a case of hereditary amelia.

Of more interest in connection with these guineas is the work of Landauer[4, 10, 11] and Dunn[4, 10] on the Creeper Fowl. In this instance we are dealing with a definitely hereditary phocomelia. The condition is inherited as a simple autosomal dominant which is semi-lethal and cumulative in its effects—that is to say, the peculiarities are more pronounced in the homozygous than in the heterozygous individuals. As an instance of this, the heterozygotes are less viable than normals (more heterozygotes fail to hatch than normals), but the homozygotes are 100% lethal. Most of these lethal embryos die at a critical period around 72 hours after fertilization before the limb buds have developed far enough to show any peculiarities. A few survive until the time of hatching, but do not hatch. These latter have decidedly phocomelous limbs.

It was with a view to comparing the wing defect here described with that of creeper fowls that I made dissections of the limb skeletons of a mutant and a normal guinea. I find that in the present mutation there is no apparent abnormality of either the leg or wing skeleton. Figure 6 shows a photograph of wing skeletons taken from a mutant guinea (left) and from a normal guinea (right). It will be seen that all the bones present in one are also present in the other and are approximately equally developed.

The situation in the caudal skeleton is not quite so certain. Figure 7 shows very well the abnormal appearance of the mutant guineas with respect to the rump. This suggests an affinity to hereditary rumplessness and hereditary intermediate rumpless-

ness as described by Landauer[8, 9] and Dunn[2, 3, 8]. Pictures taken from their dissected specimens show that in rumpless fowls some or all of the caudal vertebrae are lacking in each case. Those in which all the caudal vertebrae are absent belong to the true rumpless group, and those in which the reduction is not complete belong to the intermediate rumpless group. In this latter group the synsacro-caudal vertebrae are always lacking; the five free caudal vertebrae are usually present but much reduced and fused with one another in various degrees; and the pygostyl, though present, is composed of fewer embryological elements than is normal. I find on dissection of these mutant guineas that the normal number of synsacro-caudal and free caudal vertebrae is present. However, it may be seen from Figure 7 that the pygostyl is very decidedly reduced. Whether it is composed of fewer embryological elements; whether these elements are of smaller size; or whether, indeed, it is always as abnormal as in the case photographed, remains to be determined. We have so few of the mutant guineas alive that we can not afford to kill them at random for dissection. And they breed so slowly that it will be several years before we can have them in sufficient numbers to make all the desired crosses and still have some for dissection. The most that we can say at present is that in addition to its ectodermal expression the new gene apparently extends its influence to the skeleton in the caudal region reducing the pygostyl.

There are many known genes affecting feathers. Most of them influence color pattern[5], rate of feathering[15], structure of feathers[11, 12], etc., but there are two that show some similarity to this new mutation. I refer to the gene for the "frizzled" character[12] and that for absence of the hackle[6]. The gene "frizzled" not only modifies the fine structure of the feather very radically, but also in the homozygous condition affects the distribution of the feathers, leaving large normally feathered areas unfeathered. Since the gene I describe leaves two normally feathered areas unfeathered it bears in this respect some similarity to it. "Frizzled" is an autosomal dominant, and in the homozygous state impairs fertility and viability. The gene "hackleless" is even more similar, inasmuch as it affects the feathering of a smaller and more constantly defined region, the neck, preventing the development of the feather follicles in that region. The neo-Lamarckian view that it may be caused by the inheritance of acquired dermatitis erythematosa has been expressed, but this seems to have been definitely disproven by Greenwood[6] who showed that it is due to a dominant autosomal mutation. This mutation differs from the one here described in being dominant rather than recessive; in affecting the neck region rather than the ulnar border of the wing and the edge of the uropygium; in having no skeletal effect; and in causing a highly vascular condition in the denuded area which gives it a red appearance to which there is nothing corresponding in these guineas.

In the new guinea mutation I have found no abnormalities of the feathers in any region other than the two mentioned (ulnar border of the wing and the edge of the uropygium). In these bare areas the feather follicles may be seen, but the development of the feather above the skin does not usually complete itself. In one instance in the F_2 three of the remiges, two on one side and one on the other, are present above the skin, though imperfectly developed. In all other instances they are entirely lacking externally. The adjoining rows of feathers on either side, the upper and under coverts are present and normal both on the wings and the uropygium. Only two rows of feathers (three if you count the right and left remiges separately), the ones constituting the flight feathers, are absent. It is this strict and narrow confinement of the effect of the new gene which makes

the term alopecia too broad and unspecific. For this reason, I propose the name aprotopteria for this condition. Whether the mutation causing aprotopteria* has occurred before historically, whether, for instance, it played a part in the evolution of the Ratitae, the flightless modern birds, is a matter of speculative interest. In all of these the "rectrices are absent or irregularly arranged, and the pygostyl is small or undeveloped"[13].

One other detail in the breeding data deserves attention. The frequency of lethal or semi-lethal effects of mutations in poultry makes one look with suspicion at the small number of aprotopterous guineas which have survived to maturity, and the large number of eggs laid by the F_1 female which did not hatch. Unfortunately these eggs were not opened to see whether they contained lethal embryos. This observation will be made on the eggs laid in the crosses now in progress. As for the homozygous mutants which hatched but did not survive to maturity, it may be said with certainty that the two in the F_1 died by accident. They were eaten by a dog. But for the four in the F_2 there is no explanation, and one must hold them in suspicion.†

Summary

1. A new mutation, aprotopterous, preventing the development of the flight feathers of the wings and tail, and apparently reducing by nearly 50% the size of the pygostyl, has appeared in the guinea fowl (*Numida meleagris*).

2. The new gene is an autosomal recessive.

3. There is some basis for a suspicion that it impairs viability.

4. It is suggested that a similar mutation may have played a part in the evolution of the flightless birds.

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* From the Greek: a—lacking; protos—primary; ptera—feathers. It will be remembered in this connection that the original meaning of pteron is feather, not wing. The forms, aprotopterus (noun—an individual exhibiting the condition) and aprotopterous (adjective—pertaining to the condition), which with their Latinized endings are really linguistic hybrids, I feel constrained to adopt after the almost universal Medical precedent in such words as amelus, phocomelus, ectromelus, and their adjectival forms.

† I am indebted to Mr. Effingham B. Morris, President of the Wistar Institute, for the gift of the guinea fowls described in this paper; to Mr. C. G. Dietrich, Manager of Bolton Farms, for the information of their first appearance; and to Mr. P. R. David of the Storrs Agricultural Experiment Station for many and valuable references to the literature.

GENIUS AND HEREDITY

A Review

THE literature on genius is very meagre. We know very little that is fundamental about that rarest type of man who is of utmost importance to humanity. The reason for this is perhaps, as Dr. Hirsh states, "that no final study of genius is possible until a genius writes upon the Nature, Position and Problems of Genius."

This book* is divided into three main parts. Part I, "History, Creative Adaptation, and Genius" (64 pages) discusses "the nature of Man by succinctly analyzing his socio-historical development, his methods and means of adjustment, and some of his future possibilities in this direction." Part II (191 pages), seeks to understand and interpret Man by analyzing his psychological constitution. Part III (55 pages), is devoted to the nature, function, and singularity of Genius.

After revewing man's types of adaptation to his environment such as passive physical and social adaptations, active physical and social adaptations, Dr. Hirsch concludes that there is an urgent necessity for a new type of adaptation termed creative adaptation which he divides into (1) positive eugenics, (2) negative eugenics, and (3) birth control. He believes that a hopeful asset with which to face and overcome world-wide racial retrogression is our knowledge of the principles of biology and heredity:

Never before have so many of the facts and principles of variation, heredity and selection been known; a Freshman course in biology gives the student more information than the greatest naturalist possessed a century ago. But can our knowledge be turned into power—our knowledge of the kinds and conditions of variations, the modes of transmission, etc?

The biological situation of mankind today is unique in two important ways. First, never before have there been a number of currents all converging in the direction of national and racial amalgamations. Previously, the decadence of a civilization and its consequent submergence were only a social and national disaster; the vigorous barbarian *supplanted* the representatives of the dying civilization but *assimilated* the civilization. But today, if the civilized nations succumb, because of the exhaustion of their most vital element, superior blood, there are no new hordes to supplant them. A race of mongrels, morons, and maniacs, is the displeasing possibility, if the present tendencies of differential, racial, national, and class birth rates are not *reversed.*

Beauty, with intelligence, character, and initiative is declining. The supremacy of the under-man appears possible; and if so the death knell of civilization is almost within hearing.

The necessity for Creative Adaptation reveals itself in the realm of beauty, as in governments, economics, education, religion, and science. An insurmountable disjunctive situation faces the world today: either Creative Adaptation or a return to barbarism.

Dr. Hirsch makes these conclusions only after a careful and dispassionate survey of facts which he ably summarizes. Civilizations decay largely because their human harvests are bad, according to Dr. Hirsch, and war is one of the fertile agents for the propagation and multiplication of the less fit.

Rarely if ever before in history has there been such a wealth of great opportunities, such a crying need for great men, and such a dearth of them as has characterized the world in the last fifteen years. The most titanic of situations—the most volcanic events—the daily eruptions that formerly took years or decades to witness, occurred in the midst of a human, living drought. History was and is being manufactured—the hand-and-brain-making of history has temporarily ceased. Second-rate military leaders, and third-rate naval men, statesmen and diplomats are pawns in the movements that be.

The author believes that the decline of the birth-rate is in itself no evil. Recent surveys seem to indicate a decided trend of opinion in this direction.

The deplorable state today is not the decline, but the differential decline of the birth-rate. The less fit in each nation are

*GENIUS AND CREATIVE INTELLIGENCE, by Nathaniel D. Mttron Hirsch. Cambridge, Mass. Sci-Art Publishers, 1931. Pp. 339. $3.00.

propagating at no less a rate than a hundred years ago, and the lessened death-rate multiplies their numbers continually; on the other hand, the most fit do not reproduce their numbers and the lessened death-rate cannot effect nearly as productive results as with the first-mentioned type. Furthermore, the most civilized and most progressive nations are suffering from this worst of all human maladies at a higher rate than the countries of Eastern and Southern Europe.*

Intelligence is divided by the author into three kinds: "The first dimension of intelligence" which corresponds to instinctive behavior; "the second dimension of intelligence" found in man alone having for its motor mechanism some form of language or some external object such as tools or machinery in contrast with the first dimension which utilizes the animal organism; "the third dimension of intelligence" or creative intelligence.

The Third Dimension of Intelligence, like the first two kinds of intelligence, has three aspects or phases. The cognitive aspect of the highest dimension of intelligence we shall term "intuition." The conative aspect of this dimension of intelligence is the impulse to Create, which in artists, scientists, inventors, and philosophers of the first magnitude is more persistently present and more imperiously impelling than the instinctive drives of the ordinary man. The affective phase of the Third Dimension of Intelligence is the emotional experience of Ecstasy.

The Third Dimension of Intelligence has likewise its motor mechanisms. *Some of* these motor mechanisms are (1) Language, as utilized in poetry, drama, and other forms of literature, and as the vehicle of expression in original systems of philosophy, in great religious teachings and in profound and epoch-making works of science; (2) The material that sculpture, painting, and architecture utilize in creation; (3) The symbols of logic, mathematics, and music; (4) The tools and machinery used in the inventions and discoveries of the sciences; (5) The human body as the instrument of the fleeting creativity of acting and aesthetic dancing; (6) The human bodies and groups of persons employed by the military genius and political genius.

The different aspects of the three dimensions of intelligence are carefully analyzed. Many biological, anthropological, and psychological viewpoints are brought to bear on the problems involved. The author gives us an excellent survey of the existing theories relating to creative intelligence and genius, which is a distinct contribution to the subject.

In regard to genius he states:

The conclusions reached by writers and investigators in the last three-quarters of a century may be classified roughly into three broad groups: a negative view of genius, in which genius is biologically linked with insanity, degeneracy and imbecility; a positive view of genius which discovers its affinity with normal humanity, genius merely being an enlargement and quantitative increase of the faculties common to the average man (on this view, genius is superlative talent); a view which regards genius as qualitatively differing from talent, a phenomenon generally distinct from the extension of normal capacity. Genius, on this view, has a nature *sui generis.*

Dr. Hirsch defines a genius as "a person motivated by and frequently experiencing the functioning of creative intelligence." He has no patience with the shallow view identifying genius with insanity. Yet very recently this view is again championed in "The Problem of Genius" by Wilhelm Lange-Eichbaum. The best refutation of this view is that "the genius which runs to madness is no longer genius." It is the supreme sanity and not insanity

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* The editor of *New York Times* (May 1, 1932) observes:

Not so long ago the program consisted chiefly in scolding the "unfit" for having too many children. This line of attack has been rendered obsolete by the fact that the masses everywhere, except in Japan, Italy and Russia, have been converted with a vengeance.

How to "sell" the four-children family to teachers, doctors, lawyers, business men and the like is the crux of the problem. The basic reason for a falling birth-rate nowadays is not economic, as among primitive peoples. The birth-rate sinks as living standards go up. Increased well-being regularly encourages escape from the responsibilities of parenthood and toward a "fuller" individual life for the parents. This is why families grow smaller as income increases and why in virtually every country the birth-rate goes down as the national income goes up.

of genius which has given mankind their gifts.

The author concludes:

The genius differs in *kind* from the species, man. Genius can be defined only in terms of its own unique mental and temperamental processes, traits, qualities, and products. Genius is another psycho-biological species, differing as much from man, in his mental and temperamental processes, as man differs from the ape. As to outward garb, his vesture resembles that of all men, varying as they do from climate to climate, from age to age, and varying also as men vary within an age and territory. But these are garbs and vestures *externally* relating him to the talented and to the imbecile, and to the neurotic and to the stable. Beneath, when we approach the inner parts and envisage genius unclothed, naked to the rays of the sun, lo, we behold another species, novel, fascinating, godly, a physical mutation which lacunizes nature as the birth of the butterfly from the caterpillar.

Geniuses are born, not made. Environment's and nurture's chief rôle in the lives of genius has been the negative one of truncating their lives and curtailing their creative work. Environment cannot produce genius but it can destroy him. As eating does not produce thinking but is a necessary condition for mental activity, so the social milieu of a genius is related to his creative gleam.

There is a wealth of data assembled in this volume. Dr. Hirsch, as he has already demonstrated in his previous volume "A Study of Natio-Racial Mental Differences," has an unusual ability to organize, classify and evaluate numerous facts and theories bearing on every phase of the problems under consideration. He is a tolerant psychologist who is willing to accept what is helpful from any school of thought, be it behaviorism, Gestalt psychology, psycho-analysis, or hormic psychology.

JOSEPH ROSSMAN.

U. S. Patent Office.

The Feeble of Mind

THE MENTAL DEFECTIVE. BERRY, R. J. A., and GORDON, R. G. New York, McGraw-Hill Book Co. 1931. Pp. XIII + 225.

THIS book aims to present an epitome of "the real achievements made in the recent study of mental deficiency and the underlying underdevelopment of the growing brain." As is pointed out in the preface, a large number of people are now called upon to deal in some capacity with mental defectives, and it is desirable that authoritative knowledge on the subject be readily available. Hence this volume.

The first chapter on "The Problem of Mental Deficiency," sets forth the extent of mental deficiency in Great Britain, the operation of the Mental Deficiency Acts, and the social, educational, and economic problems which mental deficiency creates. The following two chapters on "The Evolution of the Brain" and "The Making of the Mind" present many facts of anatomy and physiology which, for the most part, are not very closely correlated with the general subject matter in hand. Had these chapters been omitted and the space, about a third of the book, been devoted to giving more information about mental deficiency, the volume would have been much more useful.

Chapter V deals with the various classes of defectives, and chapter VI with the relation of mental defects to other forms of illness. The last chapter is concerned with how mental deficiency should be dealt with. The authors are not sanguine in regard to the eventual elimination of the burden; "it is much to be feared that such a task is beyond our power." We must, therefore, put up with mental deficiency indefinitely. Sterilization appears to be classed as one of the "highly debatable procedures" which are mentioned but not further discussed. The ultra conservatism of the authors on this subject represents a quite typical English attitude.

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University of California

SOME OBSERVATIONS ON THE TEACHING OF HEREDITY AND EUGENICS

CLIFF R. OTTO
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IT may be said that scientific research is without value until it is translated into acts to alleviate suffering and to advance civilization. This is as certainly true of the science of Heredity and Eugenics as it is of any other science.

Rapid strides are being made in the advancement of these two closely related sciences from the standpoint of research. However, it is our opinion that far too little is being done to carry existing knowledge along these lines down to the population as a whole. Information pertaining to what is probably the greatest problem facing western civilization is locked in the minds of a small group of enthusiastic workers in the field. Much is being done to advance these sciences technically but comparatively little is being done to correct the grave faults their study has brought to light.

With our characteristic custom of locking the barn after the horse is stolen, we may get the desired information to the public after most of the damage has been done.

With a race that is clearly breeding from the bottom, with that problem becoming rapidly more acute during the past decade, correct information on Heredity and Eugenics should be carried to the public now.

Our experience in teaching Heredity and Eugenics to junior and senior students in a teacher-training institution has led us to the conclusion that the thinking persons in our society are ready and anxious to receive this information.

Interest in the Subject

During the school year, 1929-30, we offered courses in Heredity to 297 students. Eugenics was offered to 119 students. This was a representative enrollment.

It should be borne in mind that the great majority of these students were not biology majors. They were taking the courses for their cultural value and for help in solving their teaching problems.

The courses are based upon the usual background of the chromosome theory of inheritance, Mendelism, mitosis, combination of inheritance factors, organic evolution, natural selection as applied to man, continuity of germ-plasm, biological immortality, etc. A careful study is made of the effect of assortative mating and the differential birth rate.

No effort is made to popularize these courses by making them easy. No laboratory work is done. We feel that in a course in Heredity carrying only two semester hours credit, we have so many big ideas to offer the students that we can not afford to have them spending their time attempting experimental work that any specialist in the field can do better than they. Extensive reading is required from the works of recognized writers in the field.

Objectives

We feel that we are generally successful in reaching the following objectives:

1. The student must have an active interest in Heredity and Eugenics.
2. The student must have a working knowledge of the fundamental theories of heredity.
3. The student must recognize the significance and importance of the continuity of the germplasm.
4. The student must recognize the

significance and importance of biological immortality.
5. The student should be "sold" on the idea of lowering the undesirable hump as shown on the differential birthrate curve.
6. The student should be "sold" on the idea of raising the desirable hump as shown on the differential birthrate curve.

Probably no other courses offered to our students create as much interest and discussion about the campus as do the courses in Heredity and Eugenics. We find that most students take a delight in the work. They readily grasp the significance of the present population trends.

Let's Let the Public In

After handling hundreds of students in these classes during a period of ten years, we reach the following conclusions:

1. A large part of our population has no conception of the part heredity plays in human affairs.
2. Many valuable family lines are approaching extermination without ever having their attention called to the consequences.
3. The idea of continuity of germplasm and biological immortality is new to most persons.
4. A large part of our thinking population is ready and anxious to receive information on heredity and eugenics.
5. The correct information properly presented will increase the birthrate of the better classes.
6. Active steps should be taken at once to present a correct and sane program of popular eugenic education to the public.
7. Naturally, we realize that the last word has not been said in these fields. However, if we wait until the last word can be said, it will be too late to say it.

WHY NOT LET THE PUBLIC IN ON OUR SECRETS?

The Institute of Forest Genetics

The name of the Eddy Tree Breeding Station, at Placerville, California, has been changed to "Institute of Forest Genetics," according to an announcement from Lloyd Austin, director. The new name is believed to be more truly expressive of the character of work being carried on. Development of improved, rapid-growing strains of timber trees is a primary objective of the Institute. The early work of the Station was described in this JOURNAL in 1928.

In connection with the change of name, steps are being taken to place the Institute on a permanent basis. A non-profit corporation since its beginning, the institution is now being reorganized to provide for government by a self-perpetuating board of fifteen trustees, who are being chosen from among prominent foresters, geneticists and business men. Gifts for current expenses and for a permanent endowment fund will be sought in the near future.

The institute was established in 1925 by Mr. James G. Eddy, a Washington lumberman who had the imagination to see in Luther Burbank's achievements with domestic plants a key to the solution of the reforestation problem.

BUD VARIATION IN APPLES

A Study of the Rôle of Bud Mutation in Deciduous Fruit Improvement

A. D. Shamel and C. S. Pomeroy

(Concluded from the April Number)

Elimination of Undesirable Strains

IN a recent study of young bearing Peach trees of the leading commercial canning varieties in an orchard of more than 3,000 acres located near Merced, California, many trees of undesirable strains of the varieties have been found. These off-type trees were first thought to have been of seedling origin but subsequent investigations have shown that some of them at least were probably grown from buds that were unintentionally cut from bud variations in the parent trees from which the buds for nursery use were obtained. In similar studies in Washington Navel orange orchards of California more than 25 per cent of the trees were found to be of undesirable strains that were the result of the unintentional propagation of inferior bud variations. In recent observations of several young bearing apple orchards located in the Wenatchee Valley of Washington some of the growers have reported that undesirable or off-type trees have been found in these orchards that are apparently the result of the use of buds from limb or entire-tree variations in the parent trees from which the buds were secured for nursery propagation.

In the citrus fruit varieties grown in the Southwest the unintentional propagation of undesirable strain trees from limb or entire-tree variations has been greatly reduced through the systematic selection of parent trees as sources of budwood for commercial propagation in which the fruits on all of the branches are consistently like those of the typical ones for the variety. This method of bud selection for the elimination of undesirable strains in the propagation of commercial varieties may be as possible for apples as is the case with the other species of fruits, where it has already been found to be practicable and efficient.

Healthy off-type trees of undesirable strains or of inferior varieties can often be successfully top-worked with buds from carefully selected good type parent trees. In some instances apple growers in the Northwest have reported that they prefer replanting the undesirable trees with dependable young nursery trees but the most generally used practice at this time seems to be top-working rather than replanting. In the case of undesirable limb variations in otherwise normal trees they probably can be eliminated through the cutting out of these limbs at the time of pruning if they have been marked while the fruits were still on the trees so that they can be subsequently identified.

Description of Several Striking Apple Bud Variations

In this progress report it is impracticable to attempt to describe any considerable number of the striking apple bud variations that are known at the present time. However, descriptions of a few typical variations will be presented in order to show something of their nature. Only the characteristics of the variations that most clearly distinguish them from the parent forms will be mentioned. These descriptions will be largely confined to fruit characters and in order to illustrate them photographs of a few typical fruits will be shown.

Starking. A limb of an otherwise normal Delicious apple tree in an orchard belonging to Lewis Mood located

near Mullica Hill, N. J., was found to bear apples that consistently developed a deep and almost solid red color two or three weeks ahead of the fruits borne by the remaining limbs. This tree with its limb variation, was called to the attention of a nurseryman who in 1922 purchased the exclusive right to propagate the limb variation. Progeny tests by the nurseryman proved that the characteristics of the limb variation were perpetuated through budding after which it was propagated commercially and sold under the varietal name of Starking. (This tree was shown in Figure 9 in the April number.)

The fruits of the Starking apples as grown in the Wenatchee, Wash., districts are mainly distinguished from ordinary Delicious apples by the earlier development of the characteristic red color with basic stripes and by the fact that in the case of the Starking the red color at the time of maturity is almost solid over the entire apple.

In shape and size characteristics the Starking apples appear to be identical with the parent Delicious apples. The texture, spicy flavor, and seeds of the Starking are very similar to those characteristics of Delicious grown under comparable conditions. Owing to the early coloring of the Starking apples they will usually be well colored when they have reached their optimum storage and dessert condition. It is important to allow them to mature before picking, otherwise an unsatisfactory product is likely to be obtained. When picked at the proper stage of maturity for storage and dessert qualities, the earlier coloring strains will usually grade a higher percentage of Extra Fancy or No. 1 fruit.

The foliage characters of the Starking trees appear to be similar to those of the ordinary Delicious trees but it is usually possible to detect on the Starking nursery trees a slightly darker color at the base of the leaf petioles and a somewhat darker color of the bark of the trees.

Richared. Another bud variation of the Delicious variety that is attracting wide interest among apple growers is one that has been propagated under the name Richared. This strain originated as an entire-tree variation in the orchard of Lewis Richardson located at Monitor, Wash. It is said to have been found in 1919 and progeny propagations of the parent tree were made in 1922 by a commercial nurseryman in order to find out whether or not its characteristics would be perpetuated through bud propagation. A top-grafted tree came into bearing in 1925. It was described in *Fruits and Gardens* for December, 1928. The success of the progeny propagation was followed by the commercial introduction of this strain under the variety name of Richared.

All available evidence indicates that the parent Richared tree is the result of the accidental propagation of a bud sport among the Delicious buds used in the propagation of the nursery trees with which the Richardson orchard was planted. In several other Delicious orchards near Wenatchee from one to five trees with more or less solid red color of fruits have been found which came from the same nursery the same season. These instances constitute additional evidence as to the origin of these entire-tree variations through the accidental propagation of bud sports of this nature.

The fruits of the Richared variation have a very deep and almost solid red color that begins to develop as a blush two or three weeks earlier than the red stripes on comparable normal Delicious apples. The color of the Richared apple is a very dark red, and differs sufficiently from that of the Starking to enable growers and others to identify the fruits of each variety with certainty in orchard and packing-house tests.

The shape and size of the Richared apples are about the same as those characters of the fruits of comparable Delicious apples. Owing to the early development of the red color on the Richared apples they are sometimes picked

before the Delicious fruits that have been grown under similar cultural conditions. In this instance, as is the case with the Starking, care must be taken not to pick the apples before they have matured to the point where they have developed their full flavor and best commercial qualities.

The foliage characteristics of Richared trees are apparently similar in most respects to those of the parent variety and the quantity of production so far as it has been determined is about the same as that of the Delicious variety. The commercial propagation and planting of the Richared variety, as is the case with the Starking, is being carried on in a number of apple growing districts of the United States, particularly in the Wenatchee section of Washington.

Staymared. A strain of the Stayman Winesap originated as a bud sport in a block of Stayman Winesap apple trees that was planted by the late B. C. Moomaw in his orchard near Covington, Va. It was first noticed about 1924 as a tree that possessed typical Stayman Winesap characteristics except that its fruit began to color earlier than the normal ones and at harvest time had a more attractive, darker red color than the normal Stayman Winesap apples in the same orchard or in other orchards in that part of Virginia. Not only was the color of the fruit an improvement, but it was also reported that the apples were not so subject to scald as the normal Stayman Winesap fruits. A careful study of the foliage and fruit characteristics led to the conclusion that the tree bearing the apples with improved color was probably the result of the accidental propagation of a bud variation in the budwood used for the propagation of the Stayman Winesap apple block in the Moomaw orchard. The original entire-tree variation was purchased by a nurseryman in 1926 and has been commercially propagated and sold under the varietal name of Staymared. A somewhat similar solid red Stayman Winesap bud variation from the Wenatchee, Wash., district is shown in Figure 10, and a tree with a similar limb variation is also shown there.

Blaxtayman. Another variety originating as a bud variation in a Stayman Winesap orchard near Lake Chelan, Washington, has been given the name Blaxtayman and is being propagated and grown commercially in the Pacific Northwest. The trees of this variety resemble those of the parent variety, but the fruits develop a darker red color as they reach maturity.

Bud sport of Esopus. A striking limb variation in an Esopus Spitzenburg tree in an orchard belonging to W. H. Goodenough, Jr., near Hood River, Oregon, was studied by the writers in September, 1924, and September, 1930. This limb variation, shown in Figure 9 *E,* was discovered in 1922 by Mr. Goodenough, who had purchased the orchard the year before. It occurs on the north side of the tree and is one of the larger limbs starting from about two feet above the crotch of the parent tree. It bears apples that develop red color about two weeks earlier than the normal fruits on the same tree and when mature the apples have a very deep red color with much wider stripes than comparable normal Esopus Spitzenburg apples. The apples of this bud variation have the characteristic shape of those of the parent variety, but they do not seem to have quite so tart a flavor as the normal fruits. The foliage of the limb variation has a somewhat darker green color but is otherwise similar to that of the normal Esopus Spitzenburg limbs. Propagations of this striking limb variation have been made by Mr. Goodenough and the progeny trees have apparently identical characteristics to those of the parent limb.

Blackjon. One of the outstanding bud variations of another commercial apple variety that is now being propagated in the Wenatchee district of Washington has been named Blackjon. It was found as a limb sport, as shown in Figure 9*B,* in an otherwise normal Jonathan tree located in the J. N. Cox

orchard, now owned by T. R. Slack, near Wenatchee, Wash., in 1926. The deep, dark red color of the fruits borne by the parent limb, shown in Figure 10, as distinguished from the greener and reddish-striped apples produced by the other branches on the parent tree led to its discovery. Progeny tests of the limb variation proved that its characteristics were perpetuated through budding.

The fruits of the Blackjon bud variation are similar in shape and size to those of the Jonathan variety. They develop a deep and almost solid red color, while comparable Jonathan apples are still of a greenish color. Definite records of the relative productiveness of the Blackjon and comparable Jonathan trees are not yet available, but they appear to be similar in this respect.

The foliage characteristics of the Blackjon trees are apparently similar to those of comparable Jonathan trees. The Blackjon is being planted in certain apple districts such as Wenatchee and Yakima in Washington and particularly as pollinators in orchards of the red strains of the Delicious variety. The Blackjon strain of the Jonathan variety, as is the case with the others described in the foregoing paragraphs, is another illustration of an apparently important apple strain that originated as a bud variation during recent years.

Uses of Apple Tree-Performance Records

The term "apple tree-performance records" is here used to mean the records of the quantity of fruit borne by individual trees over a period of years together with descriptive notes on any outstanding or abnormal characteristics of the fruits and the foliage. The performance records that are now being kept in commercial apple orchards of the Pacific Northwest are being obtained by giving each tree a number and tying a strong paper tag about 3 by 6 inches in size at the same relative position on each tree on which the individual tree number, dates of picking, and the quantity of fruit of each pick can be recorded. If any abnormal or particularly interesting characteristic of the fruits or foliage of a particular tree are observed they are noted on the tags which are collected as soon as the apple harvest has been finished. The notes on the tags are copied onto performance-record blank forms where the data can be brought together and the results of each crop and those for a period of years for each tree can be analyzed and studied in detail by the growers.

There are three principal uses for apple-tree performance records: (1) comparing the results of orchard experimental tests, (2) the selection of undesirable trees for top-working or replanting, and (3) the selection of good type trees or limb sports for propagation. No attempt will be made in this article to discuss these subjects but attention is called to their increasing importance in the business of apple growing in order to obtain the maximum of efficiency in the orchards for the production of more uniformly good crops. The correspondence and orchard contacts of the writers indicate that there is an increase in the use of apple tree records in the orchards of the Pacific Northwest and that their use is being studied by growers in other apple growing districts.

Collecting Information As to Bud Variations

In order systematically to search for striking bud variations in apple trees, the plan for making such observations should be carefully considered. Early coloring variations in fruits, as illustrated in Figure 10, are most easily distinguished by looking over the trees in the orchard two or more weeks before the normal apples begin to color.

Late coloring variations, or those with green fruits as illustrated in Figure *9F*, can be observed best after the normal apples have become fully colored. Limbs bearing abnormally large apples,

MUTANT LIMBS IN APPLE TREES

Figure 9

A—Stayman Winesap tree showing (center) a small limb variation bearing deep red fruits. J. H. Dickey, Wenatchee, Wash., Sept. 24, 1930. *B*—Jonathan tree showing a large limb variation (upper left) which bears deep red fruits from which the Blackjon variety has been developed by bud propagation. Orchard of T. R. Slack, Wentachee, Wash., Sept. 22, 1930. *C*—Winesap tree which produces very large flattened fruits on one large limb. In orchard of A. L. Tertsagian, Cashmere, Wash., Sept. 24, 1930. *D*—An old Porter apple tree in which one limb (lower left) is a sport bearing only seedless fruits. Orchard of Julian Dorrance, Scotland, Conn., Sept., 1917. *E*—Esopus Spitzenburg tree with limb variation (right) which produces very red fruits. Orchard of W. H. Goodenough, Jr., Hood River, Ore., Sept. 22, 1930. *F*—Winesap tree with limb variation (left center) bearing green fruits. The mutant limb is marked with a black cross. Orchard of J. H. Dickey, Wenatchee, Wash., Sept. 24, 1930.

as illustrated in Figure *9C*, or those having different shapes from the normal, can be observed just before or at the time of picking the crop.

It is advisable to examine the seed content, particularly of fruit showing unusual shape, season of maturity, or other similar characteristics. A limb variation bearing seedless apples is shown in Figure *9D*.

Some of the most important bud variations found thus far in deciduous fruit trees have been discovered by the growers or orchard workers incidental to cultural work and as a result of their ability to distinguish the abnormal from the normal fruit and foliage characteristics.

After limb or entire-tree variations have been found a record should be

made of their location so that they may be easily found for future observations. The tree number and a note as to the outstanding characteristics of the variation is usually sufficient. It is often desirable to tie a piece of rope or tag loosely to the limb or to the entire-tree variations to help identify them during subsequent observations.

Progeny tests of striking limb or entire-tree variations will determine whether or not their characteristics are transmitted through bud propagation. There are two methods of making such tests, (1) top-working an older tree with buds obtained from the variation, or (2) by budding a few nursery seedlings with the selected buds.

The top-working method will ordinarily give an earlier progeny performance reading than the nursery method, but the nursery-tree propagation test is generally considered the most satisfactory. Commercial nurserymen are usually glad to cooperate with the growers in such tests through supplying the seedlings and carrying out the budding.

Significance of Apple Bud Variations

The phenomenon of bud variation in apples has both scientific and commercial significance. From the scientific standpoint the study of the characteristics of apple bud variations, the causes of their origin, the frequency of their occurrence, the relation of bud variations to the development of off-type strains in the commercial varieties, and the use of desirable variations, in the improvement of varieties are of particular interest. Commercially, apple bud variations may be of significance from the standpoint of the isolation of better strains of the existing varieties through the selection and propagation of the valuable ones, the conservation of the established varieties through the use of buds for propagation from inherently stable and good-type parent trees and the elimination of inferior strain trees through top-working or by replanting.

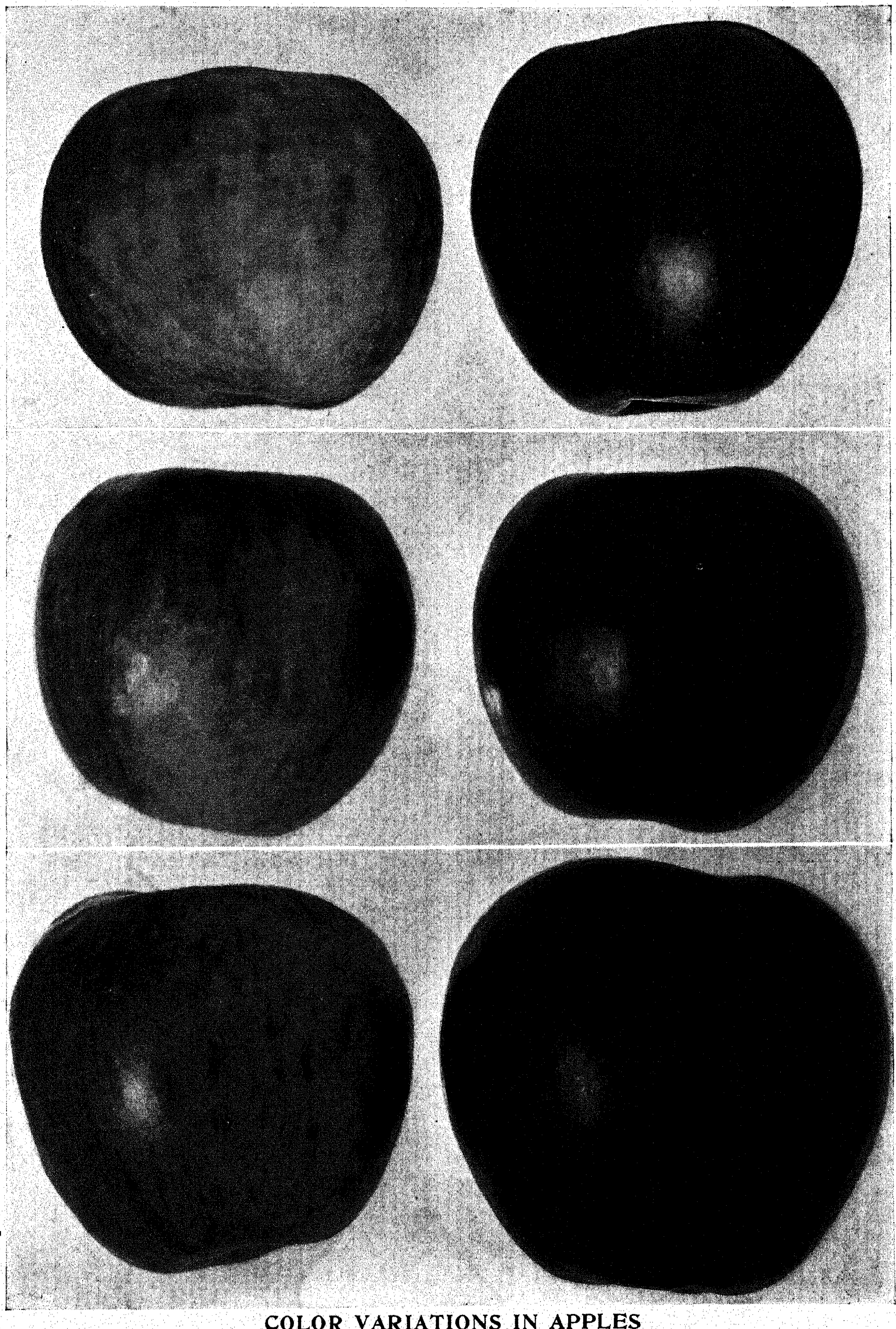
The scientific investigation of bud variation in apples is a recent development and work along this line has just been started. The commercial utilization of some of the strains originating from bud variations and their increasing importance in apple growing in certain districts will inevitably lead to an appreciation of the need for the scientific study of this phenomenon.

While the success of some of the red fruit strains of several commercial apple varieties indicates that they have financial value, it seems likely that bud variations in characters other than color will also become of importance through the development of strains that produce apples having better characteristic shape, size, texture, or flavor, or more regular bearing, as compared with those characters of the parent varieties. Color variations or those correlated with color changes are most easily found because they strike the eye of the observer most forcibly. Variations in other characteristics are less apparent and require closer observation in order to find them than do those that are distinguished by reason of marked color differences. However, it has been found that experience in searching for color variations is a helpful training in the systematic study of trees for the purpose of finding variations of characteristics other than color.

Summary

The increasing commercial use of strains of apples originating as bud variations in some of the apple growing districts has stimulated an interest in the study of the phenomenon of bud variation and the practice of bud selection from both the scientific and the commercial points of view.

The possibility of improving the commercial quality of some apple varieties has been demonstrated through the isolation and propagation of strains that produce fruits having superior commercial value in one or more characteristics as compared with those of the parent varieties.



COLOR VARIATIONS IN APPLES

Figure 10

Solid red fruits from limbs of otherwise normal trees of Stayman Winesap (top), Jonathan (center) and Winesap (bottom). Normal apples from the same trees are shown at the left in each instance. Two of these trees are at Wenatchee, Washington, and one at Monitor, Washington (bottom). The Jonathan variation has been propagated and is being rather extensively grown as the Blackjon variety. Photographed December, 1930.

The scientific study of apple bud variations includes the systematic gathering of information concerning their characteristics, the frequency of their occurrence, their perpetuation through bud propagation, and comparative studies of their commercial quality and quantity of production with the yields of comparable trees of the parent varieties.

Apple bud variations are of significance to the growers from the standpoint of the development of valuable strains from desirable variations, the elimination of undesirable strains in commercial nursery practice through avoiding the propagation of the inferior ones.

At the present time 173 bud variations are known, some of which are almost surely destined to become of commercial value to the apple industry of the United States; the remainder are of scientific interest from the standpoint of the study of bud variation in apples. With increasing interest in the commercial importance of this subject and a greater familiarity with the characteristic appearance of bud variations by growers, many additions to this list are almost certain to be made in the near future.

It is, as yet, too early in the study of this subject to evaluate fully its commercial importance or scientific significance, but it can be stated that it seems likely that the subjects of bud variation and bud selection in apples will become of much greater interest during the next few years than has been the case during the past.

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Eugenics and Democarcy—A Paradox

In the *Eugenics Review* for October, 1931, R. Austin Freeman offers a plan for the "Segregation of the Fit," a plea for positive eugenics:

Probably few of us have any doubts that if these principles of eugenic reform [reversing the differential birth-rate] could be put into practice, a steady, if slow, improvement of our population would result. But it is just at this point that a most formidable difficulty arises. Measures of eugenic reform applicable to the whole population could be enacted only by the State. Legislation would be necessary; and that legislation would have to be of a kind that would encourage the multiplication of the definitely superior classes while discouraging or restraining that of the definitely inferior. But is any such State action imaginable in the prevailing political conditions or in any political conditions that may prevail in the calculable future? The answer is given in Mr. Bertrand Russell's curt dismissal of eugenics as a practicable policy: "Democracy stands in the way."

The obstacle seems to be insuperable. Democracy—or, at least, our present democracy—is based on the assumption that all men are born equal. Eugenics is based on the observed fact that men are born very unequal. Eugenics seeks to perpetuate the superior stocks and to eliminate the inferior. Democracy denies the existence of either and seeks, through environmental agencies, to give effect to a postulated inborn equality. The two principles are at opposite poles, and the respective schemes of social policy which logicaly issue from them are mutually irreconcilable.

Nor is it a mere question of opposition in theory. The principles of democracy are being put very energetically into practice; and since those principles are the opposite to e u g e n i c principles, so the practice is the opposite to eugenic practice; that is to say, it is dysgenic. In its enthusiasm for equality, democracy even tends to fly past its theoretical objective and create a privileged class; which class includes the very group that eugenic reform would seek to eliminate. The dysgenic tendency is thus inherent in the present political system; for, whereas the multiplication of the less capable—and even the definitely unfit—is encouraged by subsidies, the more capable minority are hindered from rearing even moderate families by the drastic taxation which is necessary to provide those subsidies.

Moreover, this is no passing phase. Universal suffrage combined with immense pecuniary gifts to a large part of the electorate create a situation from which retreat is almost impossible. We must assume that the present political system, with its subsidies to the inferior at the expense of the superior, will continue until its economic and biological unsoundness manifests itself in some national catastrophe. Meanwhile, eugenics, as defined above, must apparently remain a more or less academic study of a social policy whose application appertains to some remote and unvisualized future. * * *

Of this deterioration there are two complementary factors, which may be considered separately; the increase of the inferior stocks and the decrease of the superior. These factors are of very unequal importance. For, whereas the relative increase of the least intelligent part

of the population is a grave evil, tending to produce impossible social and political conditions, it does not of necessity involve the utter destruction of the present civilization; the disappearance of the superior types would bring civilization definitely to an end. A population composed mainly of persons of the unskilled labourer type would be utterly unable to maintain anything resembling national life. * * *

The political and economic confusion of the modern world, the financial distress combined with wild extravagance, the general march towards insolvency, the combination of paid unemployment with scarcity of labour, and the spectacle of nations but half recovered from one great war, busily preparing for another; are surely the indices of declining intelligence. The political phrase, "world conditions" is scientifically meaningless. There are no cosmic or other conditions beyond human control that are responsible for this muddle. The social environment of today is undeniably the product of human conduct. Whatever the difficulties of modern man may be they are of his own creating. The recurring "crises" and the endless conferences associated with them seem to suggest that modern man is less capable of reasonably managing his affairs than were his recent ancestors; which yields the further suggestion that his general capacity—his intelligence and character—has already appreciably declined.

—Segregation of the Fit. *Eugenics Review* (London) October, 1931. Pp. 207-213.

Mr. Freeman feels that the only alternative to this situation is the formation of communities limited to individuals of superial endowment and superior stock. He realizes that the difficulties in the way of such an organization are formidable; he believes however that the success of M. Alfred Dachert in founding Les Jardins Ungemach near Strasbourg, in Alsace, Lorraine, offers a ray of hope:

To this question M. Dachert's experiment seems to have given an answer as surprising as it is encouraging. In his eugenic settlement, Les Jardins Ungemach, the birth rate has been more than double that of the adjacent city of Strasbourg, while the excess of births over deaths has varied from about six times to thirteen times that of Strasbourg. It is true, as M. Dachert points out, the figures are not strictly comparable. But they are highly significant, and, as he says, "all the more striking since our citizens belong to just that class which is deemed the most infertile, the small clerk and small official class." This rather unexpected result does, in fact, encourage us to believe that in a eugenic community the conscious attention to the inborn qualities of the children would result in a greater willingness to rear families of a size at least sufficient to maintain the population by natural increase. (*Ibid,* P. 210)

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Hereditary Blindness

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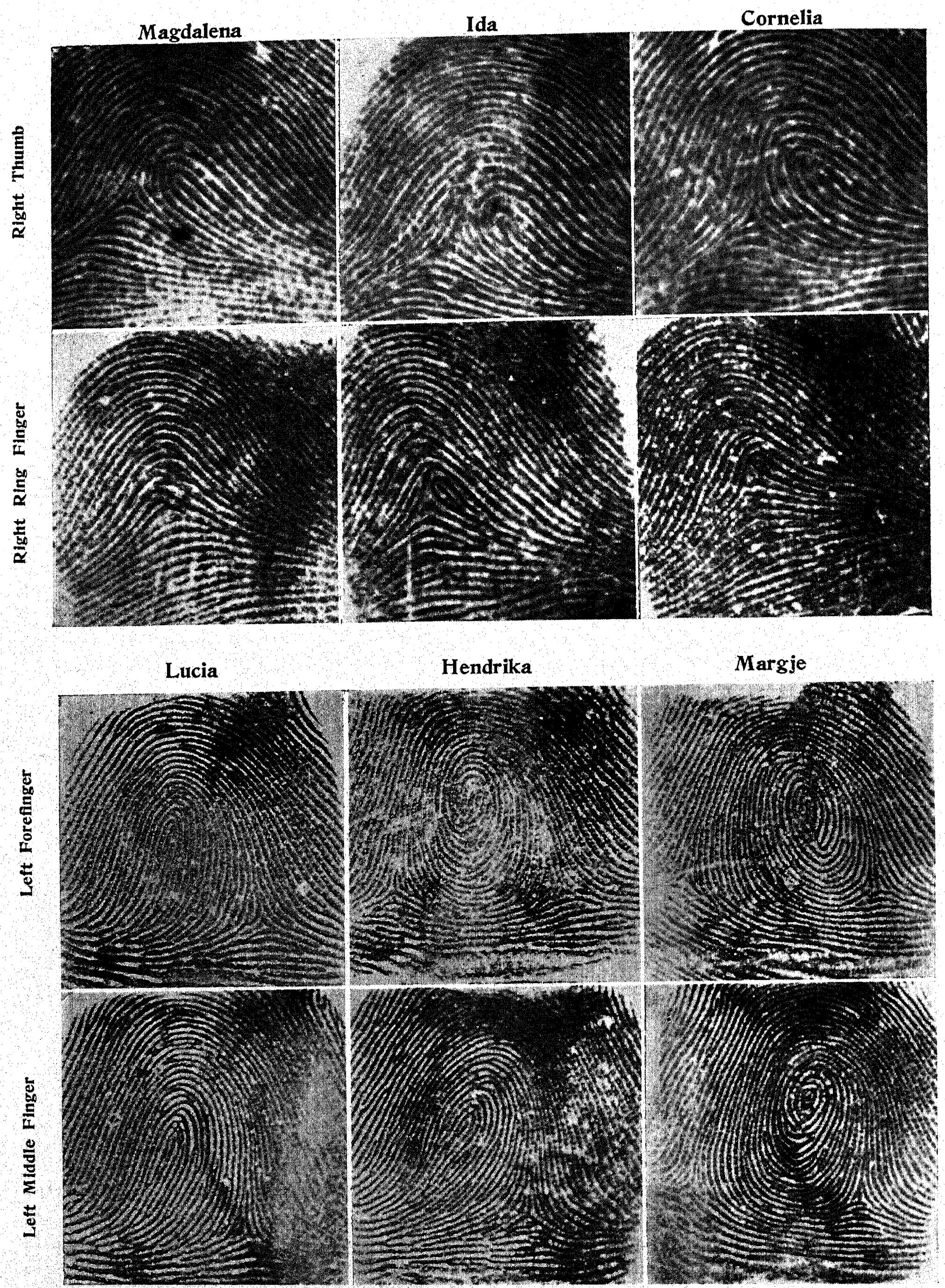


ENLARGED FINGERPRINTS OF TWO SETS OF TRIPLETS

Frontispiece

Fingerprints of two fingers of two sets of triplets, to show differences in degree of resemblance. The fingerprints of Magdalena, Ida, and Cornelia resemble each other much more closely than do those of Lucia, Hendrika, and Margje. Nevertheless the latter show sufficently striking cross-resemblance to contribute rather convincing evidence that the triplets are derived from a single fertilized egg cell. The prints of the left middle fingers of Lucia and Hendrika are extremely similar but they are little more so than the prints of the left forefingers of Hendrika and Margje. Similarities such as these would rarely if ever be found in the fingerprints of fraternal twins or of ordinary brothers and sisters. The triplets are as alike as the fingerprints. (See Figures 1 and 2.)

SIMILARITIES IN TRIPLETS

A Contribution to the Knowledge of Heredity In Triplets

J. Sanders, M. D.
Rotterdam, Holland

Since the English anthropologist Francis Galton published in 1876 his famous essay on twins, many writers have given their attention to this subject and have emphasized the importance of these investigations for research concerning the laws of heredity. Nearly fifty years afterwards Wilder, Newman, Siemens and others gave a new impetus to the investigation of twins by introducing a systematic diagnoses of similarity. Siemens found that the diagnosis of one-egg twins (identical) or two-egg twins (fraternal) is not always to be derived from placenta and chorion. Subsequent investigations by Siemens himself as well as by many others (Von Verschuer, Curtius, Waardenburg, Bar) confirm this opinion.

Various difficulties adhere to the diagnosis by means of placenta and fetal membranes and according to Siemens the principal objections are:

1. Generally one has to fall back on the memory and for the greatest part on that of the mother. Also the statements of midwives and physicians are not wholly to be trusted. Most reliable are written annotations made by them and in that case it is necessary to specify the reasons on which the diagnosis is founded. 2. To base a diagnosis on one placenta and one chorion is often extraordinarily difficult. A large part of the twins of the same sex born in clinics remain without diagnosis as regards the fetal membranes. Prinzing found that of 1,088 twins, born in seven university clinics, 17 per cent were put down as uncertain (identical *or* fraternal). For the twins of similar sex the percentage is 30. Among 848 cases in the literature Prinzing found 206 uncertain.

Therefore Siemens looked for another method and he finally adopted the polysymptomatic system. He did not base his diagnosis on placenta and fetal membranes, but on a systematic investigation of the whole body. He worked out the following scheme, consisting of twelve points, which are on the whole sufficient to ascertain if we have to do with identical twins or with fraternal:

1. Hair color and form.
2. Eye color.
3. Skin color.
4. Downy hair of the body.
5. Freckles. (Location of.)
6. Appearance of blood in the skin.
7. Follicular processes. (Lichen pilaris, acne.)
8. Tongue (furrowed or not) and teeth.
9. Form of face and head. (Physiognomy.)
10. Form of ear.
11. Form of hands (and of nails).
12. Body build.

In cases that still remain doubtful or to make the investigation as complete as possible, the following three additions are to be considered:

13. Mental make up. (School standing, character, talent.)
14. Illness and abnormalities.
15. Traits which are the bases of special methods of investigation. (Fingerprints, microscopic comparison of the capillaries, refraction of the eyes, blood groups and so on.)

According to Siemens the traits 1, 2, 3, and 4 agree always in identical twins, almost never in fraternal twins. The traits 5, 6, 7, and 8 differ among identical twins only within narrow bounds and only as an exception; among fraternals they usually differ a little more. Traits 9, 10, 11 and 12 are as a rule more similar in identical twins than in fraternal; with the latter agreement is rarely encountered.

The dactyloscopic method was first

applied by Wilder and has been developed by Poll, Bonnevie and Leven; the palm prints by Rominger. It was evident that these prints in themselves alone were not sufficient to reach a diagnosis. Siemens sent seven sets of fingerprints from twins to Poll and Rominger. From their answer it appeared that for half of them the diagnosis was uncertain.

The matter of psychical qualities also seem to agree more in identicals than in fraternals. Muller has given an account in this journal of a case of twins reared apart. At two weeks of age the two children were separated from one another. As adults they met for the first time. Then so great was the similarity between them, bodily as well as mental, that one-egg twins could be positively determined. Newman described three similar cases two years later, and Siemens has placed on record a similar case of two German boys.

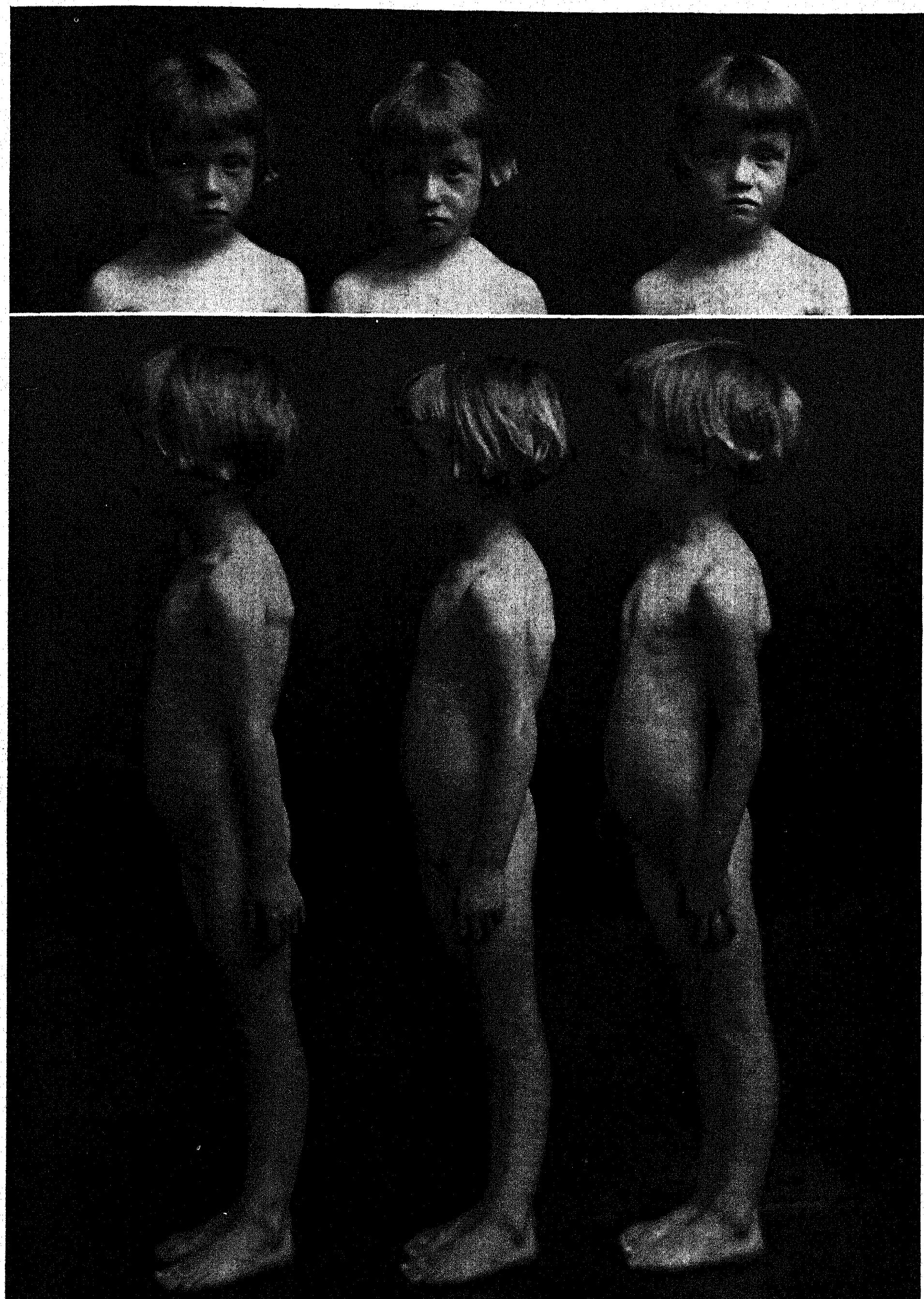
Further Siemens found, that of 29 fraternal twins of the same sex, examined by him, it was 17 times reported that their school standing differed much, 6 times it differed little and 6 times not at all. Of the 45 identical twins only 8 showed little differences, the other 37 no differences at all.

Identical Triplets

By a lucky chance I had the opportunity to examine two sets of triplets. Prof. J. P. Kleiweg de Zwaan at Amsterdam was so kind as to measure the children and to calculate the indices; Inspector J. V. Tas, head of the dactyloscopic department of the Police at Rotterdam took the finger- and palm-prints and made the photographs, while my colleague Penon at Rotterdam coöperated in the mental measurements. I ask those three gentlemen to accept my thanks for their coöperation.

TABLE I.—Physical Measurements of the W— Triplets at 4½ Years of Age.

	Magdalena	*Ida*	*Cornelia*		*Magdalena*	*Ida*	*Cornelia*
Length of head	15.7	15.7	15.6	Knee joint—bottom	25.8	24.7	23.5
Breadth of head	13.4	13.3	13.1	Malleolus internus—bottom	5.1	4.0	3.9
Smallest frontal breadth	8.9	9.4	9.0	Edge of breastbone—bottom	76.3	75.0	74.2
Breadth of zygomatic arch	10.3	10.4	10.5	Edge of shamebone—bottom	46.6	45.6	45.3
Breadth of lower jaw	8.0	7.7	7.7	Acromial breadth	23.3	22.4	22.2
Morphological height of face	10.0	9.8	9.7	Spinal breadth	14.9	15.5	15.6
Physiognomic height of face	13.9	13.8	13.9	Eye color	9.	9.	9.
Root of nose—mouth	6.1	6.1	6.2	Hair color	D	D	D
Length of nose	4.3	4.3	4.5	Form of the skull	L-R	L-R	L-R
Breadth of nose	2.7	2.4	2.4	Cephalic index	85.3	84.7	83.9
Depth of nose	1.2	1.3	1.2	Fronto-zygomatic index	86.4	90.4	85.7
Breadth of mouth	3.0	3.2	3.3	Jugo-mandibularis index	77.7	74.0	73.3
Distance inner corner of the eye	3.0	3.0	2.9	Morphologic face index	97.1	94.2	92.4
Distance outer corner of the eye	7.4	7.3	7.1	Physiognomic face index	74.1	75.3	75.5
Length of ear	4.7	4.7	4.3	Nasal index	62.8	55.8	53.3
Breadth of ear	2.9	2.9	2.8	Physiognomic ear index	61.7	61.7	65.1
Horizontal circumference of the head	48.	46.8	46.3	Total arm length	41.4	38.9	42.0
Bodily length	98.	97.2	96.7	Relative arm length	42.3	40.0	43.4
Acromion—bottom	74.	72.9	74.	Upper arm length	17.8	15.7	17.8
Capitulum radii—bottom	56.2	57.2	56.2	Lower arm length	14.1	12.9	13.1
Tip middle finger—bottom	32.6	34.0	32.0	Hand length	9.5	10.3	9.1
Processus styloideus radii—bottom	42.1	44.3	43.1	Brachial index	79.2	82.1	73.9
Spina anterior superior—bottom	53.8	51.6	50.5	Relative length of leg	54.9	53.1	52.2
				Upper leg length	28.0	26.9	27.0
				Lower leg length	20.7	20.7	19.6
				Tibio femoral index	73.9	76.5	72.6
				Trunk length	29.7	29.4	28.9



MAGDALENA, CORNELIA, IDA—IDENTICAL TRIPLETS

Figure 1

Showing similarities in stature and in physiognomy. These are the girls with the more nearly identical fingerprints. It is recorded that the birth membranes of these triplets were of the type that would indicate that they were not identical. This case furnishes further evidence of the unreliability of this method of classifying twins and triplets as identical or fraternal.

Now follows a description of the first triplets.

Magdalena, Ida and Cornelia W—— were born March 17, 1926 (Figure 1). The midwife, who assisted at the delivery, took down at the time and is yet quite sure about it that one large placenta and two chorions were present. Magdalena weighed at her birth 1,600 grams, Ida nearly 1,500 grams and Cornelia 1,100 grams. In the same order of succession the children were born. At first they were reared in an incubator and for seven months they received natural nutrition.

The measures and the indices of the children, recorded in August, 1930, are given in Table I. The eye color is determined with the eye color index of Rud. Martin; the hair color with hair color index of Fischer-Saller.

In the photographs of the children it is distinctly to be seen that all three have a dent in the right part of the chest under the mammilla. Furthermore all three girls have the second toe on the right foot smaller than the first and third, and, on the left foot, smaller than the first but larger than the third. Added to this Ida has, about 6 cm. left of the nombril, a birth-mark large as a pin's head and also one on the left ringfinger.

As to the mental condition of the children it is evident that all three children have their attention easily distracted. When asked for instance: "What is a horse?" immediately a tale follows. "This morning, walking with my mother, I saw a horse that would not go," or something like that. When, however, the attention has been fixed by asking several times the same thing, then the answer comes in most cases fluently. According to the Binet Scale the intelligence coefficient (IQ) is for Magdalena 95, for Ida 93, and for Cornelia 90. The mother asserts that the characters of the three children are nearly the same. However Magdalena and Cornelia play more with each other and Ida more by her self.

With regard to the fingerprints Mr. Tas acquainted me with the following facts:

As I have to assume ignorance of the principles of dactyloscopy by the reader, I will discuss briefly the nature of the fingerprint.

It is known that the inner part of the fingertips is traversed in all directions by lines of varying lengths, the so called papillary lines. Except scars and enlargement by growth these figures remain the same. The pattern of these forms from the moment of their coming into existence during the embryonic state until they are destroyed by death, never varies. Furthermore, every person has a pattern that does not exist for a second time on the earth.

These two facts give the fingerprints their criminological value. All dactyloscopists have met in their practice with the fact that the finest details remain always the same.

Are there at one's disposal two fingerprints, taken some time after another, of the same criminal and is the last description almost quite illegible as a result of a thorough damage of the fingertips (this often happens with persons whose hands are always in contact with rough implements of their trade, for instance, bricklayers and firemen), then it appears after a "dolce far niente" in prison that the dactyloscopic pattern returns after a short time and is quite the same as at first. Details on a scale of fractions of a millimetre appear in their original form.

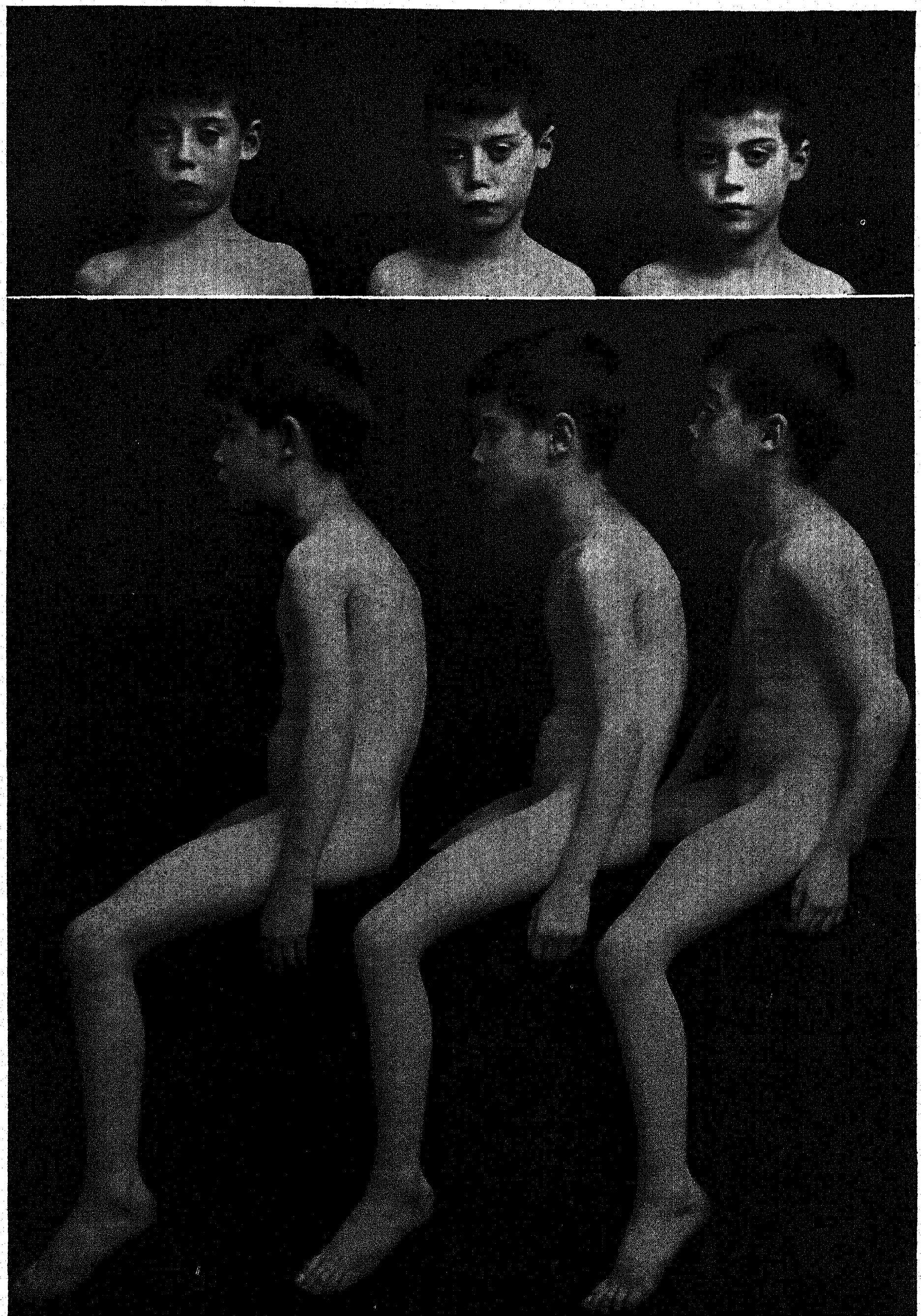
Also from a criminological point of view it is of importance to find out how far twins, triplets and, more especially, those of the one-egg variety, fail to adhere to these rules by having the same or similar fingerprints.

We distinguish in the pattern of the fingertips six principal forms:

1. *The Arch.* In this type the ridges run from one side to the other, making no backward turn.
2. *The Tented Arch.* Here the little curve of the common arch is replaced by a pronounced upward thrust near the middle. The ridges thus converging give to the pattern the appearance of a tent in outline.
3. *The Loop.* In loops some of the ridges coming from one side make a backward twist of 180° and return round a core.
4. *The Twinned Loop.* This has the ridges bending double after forming the loop, the result is something like a butcher's hook.
5. *The Whorl.* With this type the ridges turn in circles or ellipses round a core.
6. *The Composites.* Under composites are included patterns in which combinations of the other five forms are found.

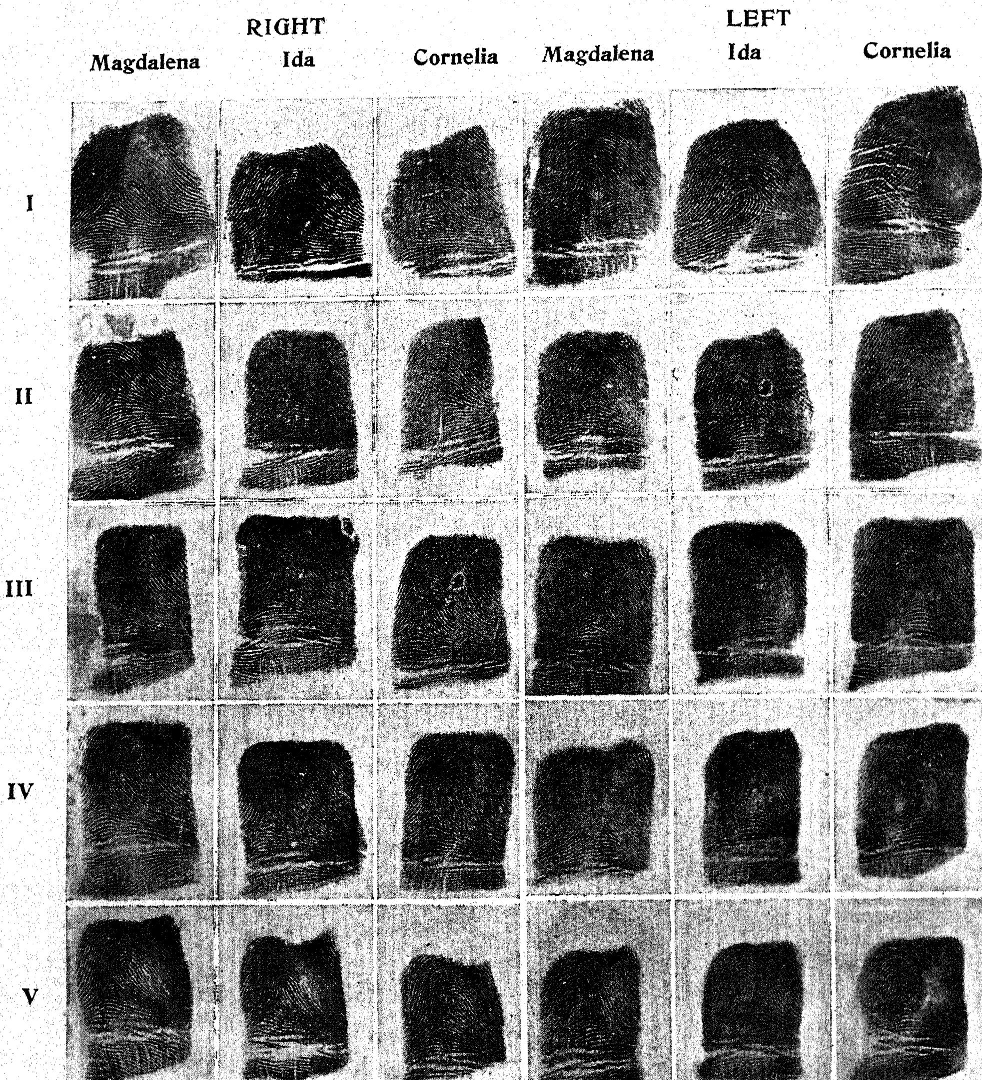
Loops are supported on one side by one delta, formed by an abrupt divergence of two ridges. Whorls and twinned loops are on the left and right under-side supported by a delta and so have two deltas.

The number of capillary ridges between core and delta is also important, while atten-



HENDRIKA, LUCIA, AND MARGJE
Figure 2

The difference in these triplets is so slight that appearance alone would almost establish them as identical triplets. Quite remarkable cross-similarities in small details and peculiarities were discovered. Note especially the shape of the back of the head.



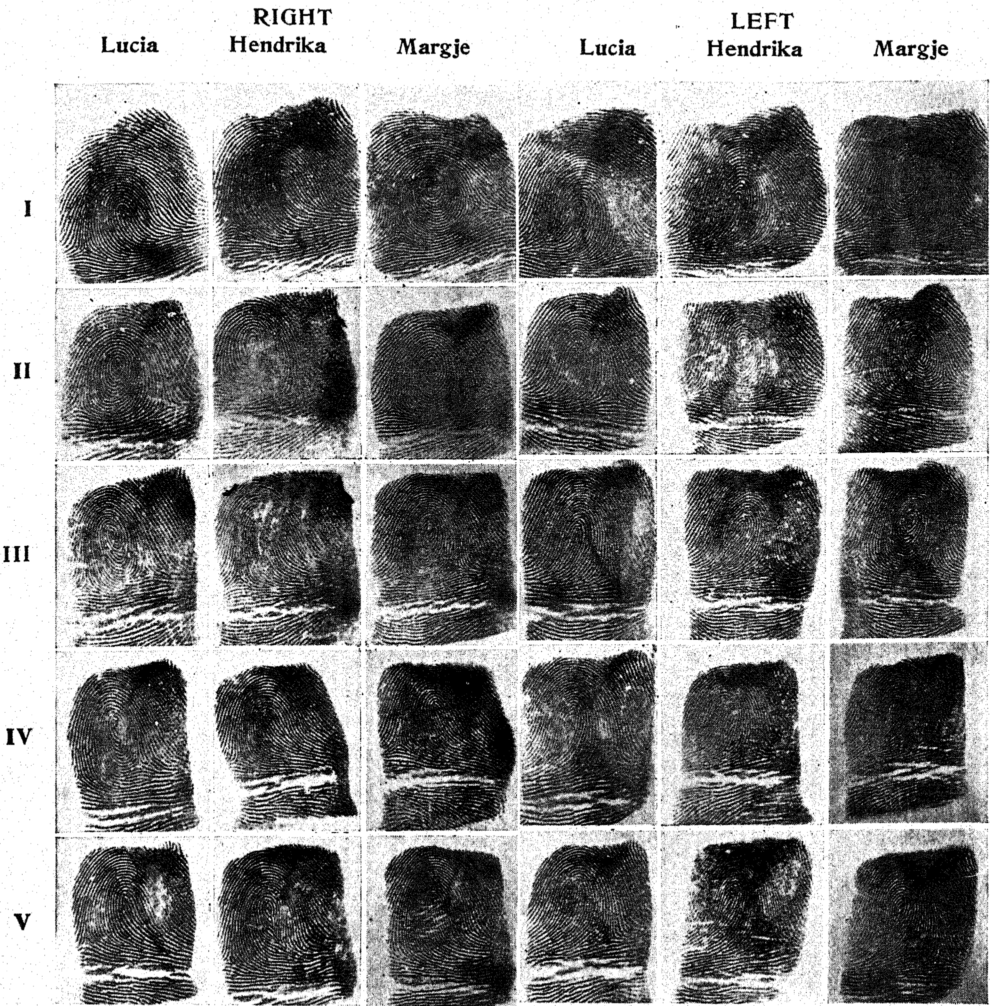
REMARKABLE SIMILARITIES OF FINGERPRINTS
Figure 3

While many of the minor details of the fingerprints are different, such similarity in the main outlines of the patterns is never found in non-identical twins or in ordinary brothers and sisters. The fingerprints alone establish unequivocally that these triplets are derived from a single egg cell. The absence of mirror imaging in the fingerprints shows that the early embryonic cell mass must have separated into three growth areas before differentiation had begun.

tion is drawn to all kinds of transition forms existing between the principal forms, by which some loops somewhat approach an arched pattern or show a whorl.

When we look at the fingerprints of the *W* triplets, we see at once a striking similarity (Figure 3). As a stranger can suddenly give utterance to his astonishment at a striking likeness by exclaiming: "How that child resembles his father!" so for a dactyloscopist there is no escape from the feeling that a great congeniality exists, in spite of many differences in smaller details.

Firstly all children show in the identical fingers loops of about the same quality, that is to say, loops of more or less the same



GREATER VARIATION IN FINGERPRINTS

Figure 4

The fingerprints of this set of triplets show less cross-resemblance than those of the other set. Nevertheless the similarities are rather more remarkable than the differences. The prints of Lucia and Hendrika resemble each other more than they do those of Margje; but in some instances the three prints are very much alike, as in the case of the left ring fingers. It is probably safe to say that no examples of similarities approaching these have been recorded among non-identical twins or ordinary brothers and sisters.

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form with about the same number of ridges between core and delta (Frontispiece).

Secondly, all other principal forms are found wanting in the three children, except the arch in both fore-fingers of Magdalena.

Thirdly, the two fore-fingers of Magdalena show really a certain relationship with the two other children. What is here actually the case? Ida shows in both her fore-fingers the loop, leaning, however, already towards the arch. Cornelia also has the loop in the two fore-fingers. Here, however, the leaning towards the arch is yet stronger. Finally, we see in Magdalena's patterns, both her fore-fingers the pronounced arch. Nature shows here a gradual transition from loop to arch, going from Ida via Cornelia to Magdalena.

It would carry me too far to go too deep into the composition of the dactyloscopic formula. Making out the formulae, for instance, according to the system of Henry, all three children are found to belong to the same head

class, namely that of the class $\frac{1}{1}$. Henry divides all dactyloscopic descriptions into 1024 classes, going from class $\frac{1}{1}$ to class $\frac{32}{32}$.

According to Henry the complete formulae for these children are:

Magdalena $\frac{1}{1}\ \frac{A}{A}\ \frac{(i)i}{(i)i}$

Ida $\frac{1}{1}\ \frac{U}{U}\ \frac{i\ i}{i\ i}$

Cornelia $\frac{1}{1}\ \frac{U}{U}\ \frac{i\ i}{i\ i}$

The layman, too, will see the striking similarity of the formulae.

The almost absolute similarity of the bodily characteristics as well as that of the mental qualities shows that we have here a case of one-egg triplets, in spite of the definite statement of the midwife. In the introduction it has already been noted that Siemens, Von Verschuer, Waardenburg, and Curtius have published cases of one-egg twins, at whose birth two choria were found according to the physician.

The second triplet E. was born on December 9, 1922 in the Training College for midwives at Rotterdam (Figure 2). Borrowed from the history of the parturition are the following facts:

Lucia is born at 4.30, Hendrika at 5.10 and Margje at 5.50. The measures of the children at birth and in August, 1930, are given in Table II.

TABLE II. Physical Measurements of the E— Triplets

	Lucia	*Hendrika*	*Margje*
Length	45.	42.5	47.5
Weight at birth	2000.	1830.	2390.
Sizes of the skull:			
Medium circumference	30.	30.5	33.5
Greatest circumference	33.	33.	35.5
Smallest circumference	28.	25.	.29
Great transverse	8.4	7.8	8.4
Little transverse	7.1	6.7	7.6
Straight transverse	10.8	10.5	10.6
Great oblique	11.7	11.6	12.4
Little oblique	8.1	8.3	8.6
Weight when they left on March 24th, 1923	4070.	3530.	4880.
August, 1930:			
Length of head	16.1	16.5	16.5
Breadth of head	13.2	14.0	14.1
Smallest frontal breadth	9.2	9.2	9.2
Breadth of zygomatic arch	11.4	11.9	11.9
Breadth of lower jaw	8.7	9.0	8.8
Morphological height of face	10.1	10.8	10.5
Physiognomic height of face	14.5	14.8	14.5
Root of nose—mouth	7.1	7.3	6.6
Length of nose	4.9	5.0	4.7
Breadth of nose	2.8	2.7	2.8
Depth of nose	1.4	1.4	1.5
Breadth of mouth	4.1	4.2	4.0
Distance inner corner of the eye	2.5	2.6	2.7
Distance outer corner of the eye	7.4	7.3	7.4
Length of ear	5.2	5.1	4.8
Breadth of ear	3.2	3.0	3.1
Horizontal circumference of the head	47.0	48.5	48.6
Bodily length	119.7	120.7	117.4
Acromion—bottom	94.1	95.7	93.1

	Lucia	*Hendrika*	*Margje*
Capitulum radii—bottom	74.2	73.6	72.9
Tip middle finger—bottom	44.3	44.8	42.9
Processus styloidei radii—bottom	57.7	57.6	58.9
Spina anterior superior—bottom	65.7	65.7	64.1
Knee joint—bottom	30.8	31.7	30.3
Malleolus internus—bottom	5.3	4.6	5.0
Edge of breastbone—bottom	94.6	95.1	92.6
Edge of shame bone—bottom	58.3	57.9	58.6
Acromial breadth	25.9	26.4	25.5
Spinal breadth	17.5	17.3	17.0
Eye color	12.	12.	12.
Hair color	P	P	P
Form of the skull	*	**	***
Cephalic index	81.9	84.8	85.4
Fronto-zygomatic index	80.7	77.3	77.3
Jugo-mandibularis index	76.3	75.6	73.9
Morphologic face index	88.6	90.8	88.2
Physiognomic face index	78.6	73.8	82.1
Nasal index	57.2	54.0	59.1
Physiognomic ear index	61.5	58.8	64.6
Total arm length	49.8	50.9	50.2
Relative arm length	41.6	42.2	42.7
Upper arm length	19.9	22.1	20.2
Lower arm length	16.5	16.0	17.0
Hand length	13.4	12.8	13.0
Brachial index	82.9	72.4	84.6
Relative length of leg	54.9	54.5	54.6
Upper leg length	34.9	34.0	33.8
Lower leg length	25.5	27.1	25.3
Tibio femoral index	73.1	79.9	74.9
Trunk length	36.3	37.2	34.0

* Form of a point 3 centimeters left of the median line.

** The same to the right.

*** The same to the right.

The placenta was large, 25 by 30 cm., and weighed 1,400 grams. It resembled a surface consisting of three placentae, which formed together one whole. The navel-strings of the first and second child were membranously implanted. The third navel-string was implanted exactly near a deep cleft between placenta I and II. There were three amnia and one continuous chorion. Consequently we have here a case of one-egg triplet.

Lucia has a little pigment spot on the chest, a little above the right arm-pit and one near the left arm-pit. They are as large as a pin's head. Hendrika has the same spot at the right, 2 cm. under the collar-bone, 6½ cm. above the papilla mammae. All the three children have on both sides the second toe smaller than the first and larger than the third. At the same place every one of them has on the center of the skull a protuberance and the skull is flattened at the back. In Figure 2 this is distinctly to be seen.

As regards the psychic condition, all three children must be rated as stupid. The Benet intelligence coefficient is for Lucia 88, for Hendrika 88, for Margje 80. The scholastic knowledge of Lucia and Margje is that of a child at the end of the first form. Lucia is a little more vivid and less slow than Hendrika. According to the mother she cannot very well run messages as she often forgets what she was sent for.

Hendrika is a bit slow, but with patience she improves. She has a tendency to remain in the rectal form. During the examination she uses the minimum number of words. At home she talks much according to her mother. She is able to run messages to the grocer. She does not lead at games.

Margje is a little more shallow; she reflects less. She cannot carry messages.

All three children are addicted to biting their nails.

The teacher tells us that Lucia and Margje are having to repeat the second form; Hendrika was just able to pass. He does not consider them mentally deficient, but thinks them very stupid. All three are rather impertinent.

As to the fingerprints declares Mr. Tas:

Though Prof. De Snoo could establish the fact that we have here a case of one-egg triplet, the fingerprints show rather big differences, even in the principal forms (Figure 4). The most striking differences have been reproduced in the enlarged photographs (Frontispiece).

The right ring-finger (Figure 4) shows in Margje an ulnar loop and also in Hendrika, where the loop has, however, a tendency to an ulnar whorl. Lucia has in this finger an ulnar whorl.

The left middle-fingers (Frontispiece) show in the same order of succession (Margje, Hendrika, Lucia) an ulnar whorl, an ulnar loop with slight tendency to whorl and an ulnar loop.

The left fore-fingers (Frontispiece) show, respectively, a radial whorl, an ulnar whorl and an ulnar loop. More or less we could see here a gradual transition in the principal forms from Margje over Hendrika to Lucia, which does not alter the fact that the dactyloscopic formulae of these triplets are quite different. According to Henry's system they should read:

Margje $\dfrac{23 \text{ ii}}{27 \text{ oo}}$

Hendrika $\dfrac{17 \text{ i}}{3 \text{ m}}$

Lucia $\dfrac{25 \text{ io}}{9 \text{ oo}}$

For this second case of triplets we have to conclude that a great bodily and mental similarity exists. On the strength of these data it is beyond doubt that we have here one-egg triplets, as was stated by the physician. Yet the similarity of these triplets is not as striking as that of the first case, where the midwife stated expressly that two choria were found.

Mr. Tas resumes, after having concluded his dactyloscopic investigations and also in connection with other experiences obtained with other cases of children born at the same time:

1. With monozygotic twins, triplets, etc.,

the fingerprints of the same fingers *can* be strikingly similar, so that the non-initiated qualify them absolutely as similar.

2. The more subtile design of details of the ridges is with one-egg triplets, also, always quite different.

3. With one-egg twins, triplets, etc., the fingerprints of the same fingers *can* even deviate strongly in the principal forms, though often a gradual transition of them can be ascertained with triplets.

The lines of the palm follow the same rules.

Practice teaches us that no fixed rule can be laid down. Nature has its whims in this respect and sometimes shows a pronounced similarity, while in other cases it is absent, or, at most, a connection can be found where a gradual transition of the principal forms is existing.

For the sake of completeness, I have to mention the fact that between very near relations (for instance, father and son, brother and sister) sometimes the family tie is plainly visible, though generally not so striking as with monozygotic twins, triplets and so on.

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Introduction of Africander Cattle

A shipment of 29 cattle of the Africander breed from three Provinces in South Africa arrived in New York City late in December. This is the first importation of such cattle to the United States, according to W. H. Black of the United States Department of Agriculture, who selected the animals and is supervising the shipment.

After remaining in quarantine for 60 days the herd will be shipped to a ranch in Texas where it will be used in breeding experiments conducted by the department and the owners of the ranch. The purpose is to develop if possible a strain or cross of cattle that will be especially adaptable to the Gulf coast regions of the United States, where a hardy animal is required.

The new cattle are a rich red color, are excellent rangers, and are gentle in disposition. The horns are rather long and have a distinctive curve, some upward and others drooping. When slaughtered, Africander cattle are said to have an unusually high percentage of dressed carcass to live weight. Until recently Africander cattle have been raised in their native country principally for draft purposes. The 29 imported cattle consist of 16 bulls and 13 females.

INHERITANCE OF WHITE SHEATH IN MAIZE

F. H. Clark*

MANY chlorophyll defects in corn have been observed and their breeding behavior established. Others have been reported and described; however, very little has been published concerning their exact mode of inheritance. The white sheath character in corn is a good example of this latter class. It was described in 1921 by Kempton[1] who reported that it was a recessive character. The purpose of the present investigation was to determine the factorial composition and, if possible, the linkage relations of a white sheath which arose from a different source than that reported by Kempton.

Source of Material

The white sheath used in this investigation was first noticed in a pollen used in an inter-row corn crossing block at the Michigan Experiment Station at East Lansing in 1923. This pollen strain consisted of three ears selected from the Duncan variety of corn. In 1924, this strain was used as the check in an ear-to-row corn test, and the white sheath plants which appeared were turned over to the writer for the purpose of making these studies.

Description of Material

White sheath in corn shows up in the early seedling stage and lasts during the entire life of the plant. A typical white sheath plant has the following characteristics: The leaf sheaths are light green to vivid white, the whiteness in the seedling stage often extending into and including the basal half of the leaves. Later in development, the leaves become normal green except in some strains where the whiteness extends into the basal portion of the leaf in the form of irregular white stripes. This striping of the leaves may appear as vividly on a plant with light green white sheath as on one having white sheath of the greatest intensity, but is usually found associated with the latter type. This whiteness of the sheath extends from the very basal portion of the stalk to the tassel node and includes the outer husk covering the ear. On a white sheath plant the anthocyanin pigment shows up as a bright red color on the base of the stalk and contrasts sharply with the whiteness above.

Crosses Between Different Grades of White Sheath

Many crosses were made between different intensities of white sheath and in all cases white sheath was obtained in the F_1, the greener sheaths always being dominant to the whiter ones. Segregation took place in the F_2, but there was so much variation that definite ratios could not be obtained. Apparently environment, as well as numerous modifying factors, plays a part in determining the expression of the white sheath character.

Factorial Composition of White Sheath

In crosses made between normal green sheath and white sheath plants, the F_1 plants were green sheath showing that white sheath is, like other chlorophyll anomalies in corn,

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a recessive character. Out of a total of 19 progenies involving 3926 plants, 239 plants had white sheaths. This is a very good fit to a 15:1 ratio, the deviation being only 6.4 ± 18.30. White sheath apparently depends for its expression upon the presence of two complimentary recessive factors which for convenience may be designated as ws_1 ws_1, ws_2 ws_2.

The backcross data obtained strengthened the above supposition, for in backcrosses involving white sheath we would expect to obtain a ratio of 3 green to 1 white sheath. The backcross and resulting genotypes expected may be represented as follows:

Backcross—

$Ws_1\ ws_1\ Ws_2\ ws_2 \times ws_1\ ws_1\ ws_2\ ws_2$

$Ws_1\ ws_1\ Ws_2\ ws_2$	
$Ws_1\ ws_1\ ws_2\ ws_2$	3 green sheath
$ws_1\ ws_1\ Ws_2\ ws_2$	to
$ws_1\ ws_1\ ws_2\ ws_2$	1 white sheath

This expectation was realized, for in four backcross progenies made up of 750 plants, 559 were green sheath and 191 were white sheath, the deviation from a 3:1 ratio being 3.5 ± 8.0.

Several green sheath plants in one of the backcross progenies were self-pollinated and, as would be expected from the results shown above, approximately one-third of them gave a 15:1 ratio ($Ws_1\ ws_1\ Ws_2\ ws_2$) and the remainder segregated in a 3:1 ratio ($Ws_1\ ws_1\ ws_2\ ws_2$ or $ws_1\ ws_1\ Ws_2\ ws_2$).

Crosses With Kempton's Strain

In 1926, seed of Kempton's white sheath was obtained and planted for comparison with the writer's white sheath and to cross with it. It was noted that Kempton's white sheath was very white in appearance while the writer's had the suggestion of a yellowish cast. Crosses were made and the F_1 resembled Kempton's. The F_2 showed segregation and in several progenies a 3:1 ratio was approached between the white and yellowish tints. Classification was so uncertain, however, that the attempt was given up. It may be that these slight differences in color between the two white sheaths are due to other color factors carried by the plants which affect the expression of white sheath.

The fact that nothing but white sheath plants were obtained in the F_1 in crosses between the two proves that both types carry the same factors, $ws_1\ ws_1\ ws_2\ ws_2$, although it is quite possible that they differ in their modifying factors.

Linkage Tests

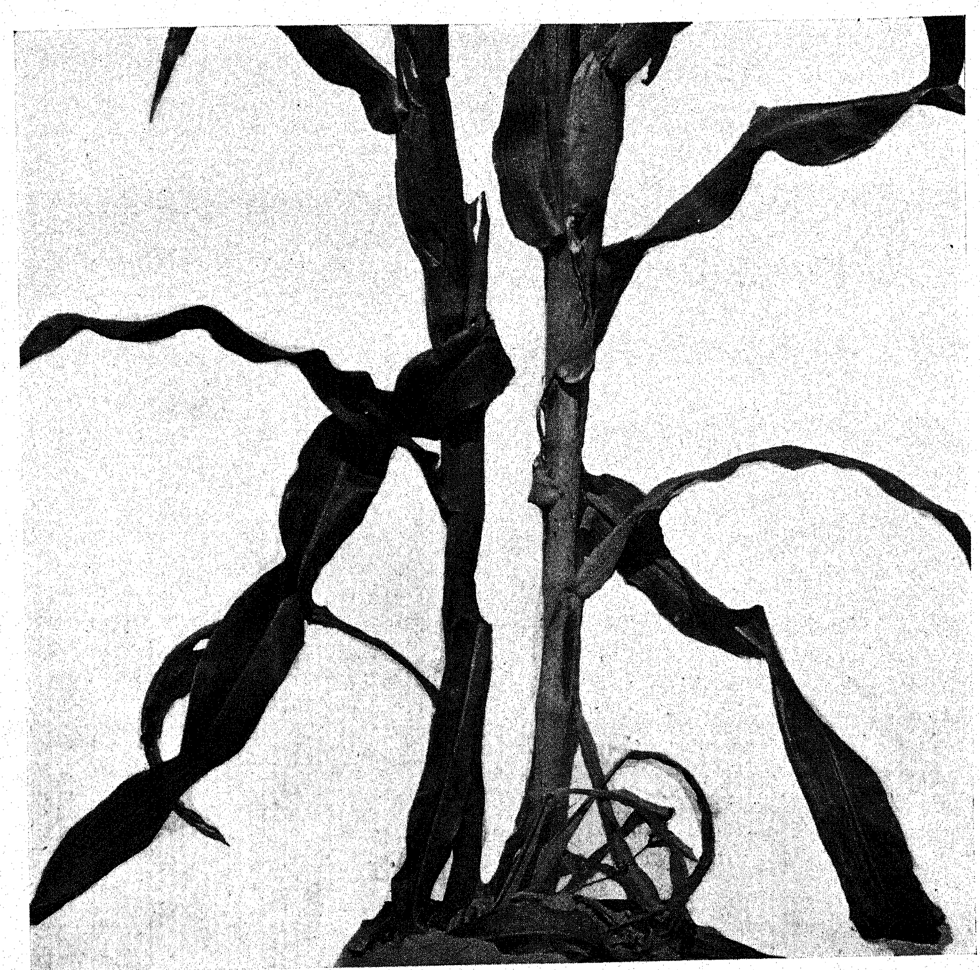
Plants having the white sheath character were crossed with various other types of corn in the hope of discovering evidence of linkage. An analysis of the data obtained in F_2 and backcross populations indicates that white sheath is not closely linked with sun-red, white cap, shrunken, R aleurone or liguleless leaf. White sheath may be loosely linked with shrunken endosperm, the cross-over percentage from a population of 626 being 46.86 ± 4.62. Kulkarni[2] has also concluded that white sheath is not closely linked with white cap.

Summary

1. White sheath, a chlorophyll abnormality of maize, is shown to depend for its expression on the presence of two complimentary unlinked recessive factors ($ws_1\ ws_1\ ws_2\ ws_2$). The great variation in the intensity of this character makes it seem likely that additional modifying factors are involved in its expression.

2. Crosses between the white sheath used in this study and Kempton's white sheath prove that the latter type also carries the two factors ws_1 and ws_2.

3. Linkage studies show that white



NORMAL AND WHITE SHEATH MAIZE

Figure 5

A normal plant (left) and a typical white sheath plant (right). The leaf striping shows up very plainly in this picture and is characteristic of certain plants having an intense white sheath. The whiteness of the sheath extends from the very basal portion of the stalk to the tassel node and includes the outer husk covering the ear.

sheath is not closely linked with sun-red, white-cap, shrunken, aleurone-R, or liguleless leaf. The cross-over percentage with shrunken endosperm of 46.86 ± 4.62 suggests a loose linkage.

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BRANCHED CATKINS IN THE CRACK WILLOW

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FOR three successive years, branched staminate catkins have been observed on a number of willows (*Salix fragilis* L.) near Thiensville, Wisconsin. Most of the catkins observed have shown a dichotomous branching (Figure 6). A number of catkins have been found with two or three short lateral branches. The phenomenon has not been observed in the female inflorescence of the same species.

Approximately one-third of the catkins on each tree have exhibited the anomaly, the branched clusters being evenly distributed over the entire crown. The number of abnormal clusters on a large tree, with a trunk diameter of nearly four feet, during the spring of 1931 was more than forty per cent of the total. Smaller trees in the vicinity produced catkins of which ten to thirty per cent were abnormal. The phenomenon has been observed only on trees growing within a radius of one-fourth of a mile.

Under microscopical examination the flowers of branched catkins appeared to be normal. Serial sections through the catkin axis have shown no unusual tissue development, and no evidence of insect stings as Wilms* has suggested for *S. babylonica*. Dehiscence of the anthers took place before abscission during the spring of 1930 and 1931. Observations of this nature were not previously made.

Examination of all the sheets of *S. fragilis* in the herbaria of the University of Wisconsin and of the Milwaukee Public Museum has failed to reveal a single specimen with branched catkins. Cuttings from these willows are being propagated in another state and should yield some interesting results in the future.

BRANCHED CATKINS
Figure 6
A large willow tree and several smaller ones in the vicinity had about a third of the catkins branched, as above. It seems probable that this is an inherited characteristic, as such branching appears to be rare in this species of willow.

* Penzig, O. Pflanzen-Teratologie. Band III, p. 243. 1921-1922

THE NEED OF NATURALISTS

A MEMORIAL of the life and work of David Starr Jordan appeared in *Science* for October 2, 1931, written by Dr. Barton Warren Evermann, Director of the California Academy of Sciences, who collaborated with Jordan for many years in the study of fishes.

Evermann's testimony to the value of his association with Jordan is a significant educational record, and voices a sentiment of personal appreciation and regard that finds few parallels this side of Plato:

I first met Dr. Jordan in the spring of 1877 at Butler University where he then was professor of biology. The next year I was one of a dozen students led by Professor Jordan on a walking trip of 550 miles through the South—from Somerset, Kentucky, across Kentucky, Tennesse and North Carolina, into northern Georgia. This was, in fact, a traveling school. Dr. Jordan had been through that country before and, being quite familiar with it, he gave us at least one lecture each day on the geology, natural history or political history of the region through which we were passing.

That tramp through the South with Dr. Jordan was the most important influence that ever came into my life. I came to know him intimately and from many angles, and my admiration for his scholarly attainments and for him as a man grew day by day. I then and there determined to continue as a student of his so that I might get as much from his inspiring teaching as possible. That I did, and the association and the personal friendship begun then continued without interruption and without a single jar to the end, I always as his humble and enthusiastic helper, proud of his friendship and grateful for his patience and encouragement. (*Science,* 74:327-328, October 2, 1931.)

The relation of Jordan's work as a naturalist to his activities in other fields of science is summarized by Evermann as follows:

Dr. Jordan was, first of all, a naturalist. His intimate knowledge of many species of animals and plants, gained through observation and study in the field of many individual animals and plants themselves, was marvelous. In the making of a naturalist and the study of systematic zoology and botany, he regarded books as of value but as of only secondary importance. He often said (quoting Agassiz) "When one studies Nature from books alone, he will not know her when he meets her in the woods or fields." He was amused by the discussion of evolution and the origin of species, in the papers at the time of the Scopes trial. He felt that the only way in which species can be studied understandingly is through a careful examination and comparison of numerous specimens of different and related species. Reading books about animals and plants can not alone give any clear conception of what a species is. And one can not very clearly understand what is meant by the organic evolution of species unless he compares many specimens of closely related species. Books about animals can not lead; a study of the animals themselves must come first. Dr. Jordan was a real naturalist. He knew the animals and plants of the old farm on which he was brought up, and those of every region in which he later lived; not only the fishes (his first and abiding interest), but the birds, mammals, reptiles, amphibians, mollusks, crustaceans, lichens and the algae not only of the fresh waters and damp places but of the sea as well.

As an educator Dr. Jordan was no less great. His influence upon educational theory and practice has been profound. Even while yet at Butler College he had begun to advocate the giving of greater freedom to the student in the selection of the subjects that should be required for graduation. When he went to Indiana University in 1879 he continued to speak for greater elasticity in the curriculum. When he became president of Indiana University in 1885, he at once began to put the elective system into effect. He believed that the natural and physical sciences and modern languages are no less valuable than mathematics and the dead languages for mind development and training, and that their cultural value should be recognized. Under this system the student's major professor becomes his counselor and guide. With the opening of Stanford University in 1891 with Dr. Jordan as its first president, the really great opportunity came and Dr. Jordan embraced it with all his wonderful power and enthusiasm. With a faculty of his own selection of able young men with unbounded enthusiasm and an institution unhampered by tradition, the time was auspicious for a great adventure in higher education. It was made; and what was largely educational heresy then is now accepted throughout America. Under Dr. Jordan's influence the natural sciences be-

came respectable parts of the college curriculum.

In his study of fishes Dr. Jordan was interested in finding out what different kinds or species are found in different waters, and the relationship between those of one stream to those of other streams. To interpret the problems of geographic distribution it is necessary to know the species involved; hence careful comparison, study of differences and resemblances, is necessary; in short, to understand the origin of species it is necessary to know what the species are. Dr. Jordan did not limit his study of species of fishes to an examination of research collections; he was one of the keenest, most observing of field naturalists; he "knew fishes when he met them in their native waters" and was interested in their ecological relations.

A complete bibliography of Dr. Jordan's papers and books on fishes is not at hand, but it contains more than 450 titles. These contributions varied in size from a few pages to one of 3,528 pages and 392 plates.

Dr. Jordan once said: "I have lived three more or less independent lives: first, and for the love of it, that of naturalist and explorer; second, also for the love of it, that of teacher, and third, from a sense of duty, that of minor prophet of Democracy." And he adds that "if he had his days to live over, he would again choose all of the three." And each of these lives, filled with life's enthusiasm and faith in the final triumph of truth and righteousness in the world, he lived joyously.

For David Starr Jordan was a man who enjoyed life and who gloried in the opportunities that came to him to use his tremendous mental and physical strength for the good of his fellow man. His power of memory was astounding, his knowledge profound. He was an inspiring teacher, kind, sympathetic, utterly unselfish, and always ready to give encouragement to any student who seemed to need it. (*Ibid* Pp. 328-329.)

The service that Agassiz and Jordan were able to perform, of placing the natural sciences in the curriculum of higher education in the United States, is hardly to be over-estimated, but the advance is not being sustained. Although the larger institutions have continued to expand their biological faculties, general education in biology is not keeping pace. Fewer courses and smaller classes in botany and zoology are reported in secondary schools than a generation ago. Even agricultural colleges may offer little botany or zoology, these outdoor sciences being replaced by classroom or laboratory work in agronomy, physiology, pathology, entomology, genetics, economics, or "business administration," assuming that crop-knowledge can be taught without the study of plants. The knowledge of nature that is needed as a background of scientific farming and of constructive interest in rural life is not conveyed through the present system of education.

Huxley is quoted as defining education as "the instruction of the intellect in the laws of nature," but instruction is the wrong place for the emphasis in biological education: Darwin was a naturalist,—Huxley a professor. Intellect is only one element of personality, and "laws of nature" are only abstractions to those not familiar with animals and plants. Pedagogy is powerless to furnish the foundation of biological intelligence. Systems of formulated training may only interfere with knowledge being obtained. Interest, diligence and clear thinking are contagious habits of mind, but are not deliberate accomplishments, hardly to be taught or learned in the class-room.

It has to be recognized that the subject-matter and discipline of the biological sciences are poorly adapted to purposes of scholastic instruction. Latin and Greek were better subjects for the schools to teach, and were more useful to the potential investigator than much of the routine instruction in biological specialties. Natural history as a diversion from classical education often went farther than the present "scientific courses." A dozen students might have such an opportunity as Evermann appreciated, but not a thousand students, or a hundred, or hardly a score. The methods of mass-production have turned us away from the outdoor world. The naturalist is difficult to institutionalize, and his outlook on life is in danger of being closed.

Interest in animals and plants is a natural instinct, not acquired in school, but commonly weakened under school conditions. As children we all are

naturalists, but soon our minds are dimmed to "the light of common day." The factory has dragged the school in its wake, and the urbanized school system is rapidly reducing the race to the mental habits of mill-workers, indifferent to nature and immune to books. Our closely graded schools, that limit the contacts of the pupils to others of the same mental age, were a special invention of the devil of ignorance, disguised as a saving of labor.

The years of youth are passively sacrificed to the scholastic routine, with no experience to develop the abilities, tastes, or ideals of the out-door investigator or explorer. Formal instruction can contribute very little to the development of the scientific mind, while personal contacts with working investigators may contribute enormously. Plato tells us of Greek youths who made active search for the best teachers, but such intelligence is rare among us. Great loyalty to institutions is the conventional virtue of our system of higher learning.

Evolution in the Classroom

Many specialists in biological sciences, as pathologists, physiologists, agronomists and geneticists are very far from being naturalists, but rather are physicists, chemists or statisticians, interested primarily in the technique of their specialties rather than in the general biological problems. The laboratory mind often gives no reflection of the background of nature, and hence is unable to form a practical conception of the evolutionary process. The diversity and multiplicity of species are dismissed as a needless complexity, or arouse a peevish disparagement of the efforts that naturalists have made to explore the organic world. Systems of classification are tabooed as "taxonomy," as though geographers were to discard maps, or historians disregard dates.

Laboratory investigations of organic chemistry, physiology and pathology are carried to marvelous refinements, and reach brilliant applications in the fields of surgery, therapeutics and dietetics, but the wider applications of heredity to public health, eugenics and sociology will require a vast extension of popular interest and knowledge on the biological side, instead of a restriction or recession.

Retrograde tendencies in biological interest are shown in the use of evolution as anti-religious propaganda, and in the recrudescence of the anti-evolution controversy. Only superficial and misleading ideas are conveyed in many of the current arguments, whether for or against evolution. When the actual state of knowledge is considered, the notion of teaching evolution as a class-room exercise in high schools and colleges is seen to be essentially absurd, as Jordan recognized. Instead of the students being led to believe that they understand evolution, they should learn the need of special study in that field.

Popular knowledge of evolution needs to go much farther than taking sides in the religious debate. What nature can tell us about our racial requirements we should wish to know, even as simple prudence. Only by using our naturalist instincts do we have a prospect of gaining the knowledge and understanding that is needed for constructive solutions of our racial problems. Philosophical elaborations of abstract ideas of evolution have filled many books, but have not made plain, even to the literary public, either the facts themselves or the need of clear understanding.

The day of true democracy may come, and of better social systems, when popular knowledge extends to the natural sciences. Naturalists can see many factors of human welfare that are hidden from popular report. A notion is current that eugenics would set up a new order of aristocracy, but making capable people into parasites, instead of preserving the better families, insures their more rapid extinction. The rapidly declining birth-rates of our "cultured class" are sufficient proof that our social ideals are perverted. All of the systems of social parasitism have

the same biological effect of adverse selection. The normal instincts of development are to be obeyed and all of our abilities exercised by practical use instead of being aborted by depending on others. Parasitism is one of the primitive delusions which more knowledge of biology would help us to escape.

Democracy of Species

Biology is essentially democratic because life is organized in species, which are large groups of individual organisms related to each other through a continuous network of descent. Separate lines of descent can be analyzed in the study of particular characters, but the species as a working system is a large community of inter-breeding individuals, a form of organization suited to the evolutionary function of collecting, combining and integrating the heritable variations that appear in any of the lines of descent. Thus the species is at the same time the product and the process of evolution, as conducted through the network of descent. "Nothing living lives alone; always life is many."

If it is necessary to be a "real naturalist" in order to know species and gain an understanding of evolution and heredity, an educational system that excludes such knowledge must be reckoned as a dangerous handicap in the advancement of the race. Evolution lets us see how the processes of creation are still going on, and makes us aware that we are participants. Heredity and evolution are profoundly transforming ideas, once they have entrance to the mind. The sense of responsibilty is increased, and the obligation to understand the course of nature. How species maintain their existence and how their characters are changed, become practical questions. The biological concepts of evolution, heredity, adaptation, selection and eugenics need to be used as the basis of constructive thinking on many present day problems, to meet the call for a "wide-spread social and economic intelligence." Economics and sociology must come into practical relations with the biological sciences before they can deal constructively with problems of racial welfare, and reclaim our human birth-right in the outdoor world. The naturalists' interest in nature is an essential factor in the solution of these problems.

Many efforts have been made to graft the natural sciences on the school system, through Agassiz societies, nature study, school gardens, and house plants in school rooms, but without lasting effects. Feelings of profound disappointment with the results of scientific education are finding expression, as in the following statement which is credited to Dr. Nicholas Murray Butler, President of the Columbia University:

> The extensive and intensive study of natural science, now carried on for more than a full generation, has made no impression whatever on the popular mind. That mind continues to come to its conclusions and to formulate its choices with serene unconcern as to whether any such thing as scientific method exists.

To expect that instruction in the natural sciences would change the habits of thought among urban populations, seems hardly reasonable, but the slight influence of the natural sciences on rural life is more significant of the educational deficiency.

Biology the Key

The war and the depression make it plain that vast problems of readjustment must be solved in our over-developed industrial civilization, if our race is to survive. Biology contains the constructive elements of science which as yet have not been drawn upon, but which cannot safely be longer neglected. Physics and chemistry have entirely outrun the biological applications, creating new conditions of human existence, with new economic and social tensions, so that new adjustments must be made under peril of rapid decline of our racial stock through adverse selection. A vast structure of urban superfluity and parasitism has grown in a few decades, much too large for the over-exploited

remnant of normal people to support. The useful arts of manufacturing, transportation and merchandising have been carried to excess, with the familiar consequences of dissipation, vice and crime, poverty, disease and insanity.

No prospect can be seen that any extension of the urban scholastic system can overtake and correct the destructive misuse of science that already has taken place. The future of the race is not in the city, but in the country. Unless the racial values of country life can be appreciated, maintained and developed, there is no outlook. The urban educationists are astray with the urban industrialists, in their indifference to agriculture.

It is not an achievement to reduce the proportions of capable families engaged in agriculture, but a dangerous delusion. The race is being impoverished more rapidly by extending the urban school system into the country. Adverse selection is a powerful agency of destruction which the urban leadership of our age does not recognize, and which is sailing us close to the rocks.

The over-developed school system is a danger to be escaped, like other urban insanities. Education we need exceedingly, but too much formal instruction becomes a limiting factor in the development of the mind, as a few educational writers are beginning to see. More education and less school, may be a watchword of the future. A biological study of the development of our mental habits would bring out new factors, beyond the range of conventional pedagogy. Formal instruction may need to be restricted to six months in a year, to permit a better carry-over of the practical experience of the race through successive generations. If the schools go bankrupt the parents may reclaim their children and face the educational problems more directly.

Other agencies are required in order to develop, appreciate and apply the natural sciences, for the improvement of agriculture and the well-being of the race. Museums, zoological parks and botanic gardens are centers of "natural history," but mostly are placed in large cities. Various outdoor interests as garden clubs, forestry associations, national parks and playgrounds, bird-refuges and wild-life conservation, are receiving more attention in recent years. The American Genetic Association is a pioneer effort in the field of biological education, projected by a few far-sighted men of the preceding generation who sensed the racial need of a wider understanding of the facts of heredity. The difficulties of establishing and maintaining such an undertaking show even more clearly how great is the need.

If the results of biological research are to serve the public a wider understanding is necessary, in the light of a broader culture than our present system affords. That most of our intelligent and supposedly educated people should remain completely ignorant of biology, is a dangerous situation. It may be alleged that only a few men like Darwin and Jordan have had the necessary mental equipment for entering the biological fields of thought, but this assumption should not excuse the educational system for diverting either talent or interest from the study of the central problems of our racial existence. "By the side of heredity, nothing else is important."

O. F. Cook.

In Defense of Eugenics

WEEDS IN THE GARDEN OF MARRIAGE, *by* George Pitt-Rivers. With an Introduction by Sir Arthur Keith. Pp. 83. Douglas, London. 1931.

IN this little book Capt. Pitt-Rivers endeavors to enlighten his countrymen, who very much need enlightenment, concerning the dysgenic situation which prevails in Great Britain and, in fact, in most civilized countries of the world. He takes occasion to explain what eugenics is not, as well as what it is, and this

leads him to expose some of the fallacies and absurd misrepresentations of G. K. Chesterton and G. Bernard Shaw. Here his task is easy. Then he proceeds to defend the practice of sterilization against the criticisms which have been made upon it on religious and moral grounds. The defenders of the faith in the Catholic church come in for some trenchant criticism on account of their attitude toward racial problems. The absurdity of Mr. Chesterton's railing against the violation of sacred human rights by preventing the propagation of imbeciles, while at the same time he defends the position of his Church in forbidding the marriages of cousins and even less close relatives, is presented in a telling way. The author brings out the peculiar touchiness on race problems manifested by the Jews. Anthropology, for some reason, has come to be largely a Jewish science, and most of its voteries have a very obvious bias toward egalitarianism. Capt. Pitt-Rivers, who is himself a professional anthropologist, is one of the few who have commented upon this rather interesting anthropological situation.

As a defense of eugenics against its varied kinds of critics Capt. Pitt-Rivers' book may be especially commended, since he perceives more clearly than most apologists the peculiar emotional complexes which afford the real basis for most of the opposition. The author cannot be classed as a polished controversialist, but he deals his adversaries some very vigorous and telling whacks. One of the most interesting features of the volume is the description of the eugenic practices of primitive peoples, especially the Polynesians.

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A Hundred and a Quarter Million of Us.

STATISTICAL ABSTRACT OF THE UNITED STATES, Pp. 898. Price, $1.25. Department of Commerce, Bureau of the Census, U. S. Government Printing Office, Washington, D. C. 1931.

If George Washington could return on this, the two hundredth anniversary of his birth, this recent publication of the Department of Commerce could give him the dry statistical skeleton of the amazing development that is said to have been initiated by his fathering of this "Land of the Free."

In 1732 the white man's hold on the eastern edge of what was to be the United States was just ceasing to be precarious. In 1930, twenty-six and a half million automobiles killed on our three million miles of hard surfaced highways, and on our streets and byways, thirty thousand people—more than the total population of North Carolina in 1732. All of our other national statistics today, even statistics of depression, are on the same expansive scale.

Having arrived at the year 1930, just who are we people of the United States?

We are 62,137,080 males; 60,637,966 females. Our sex ratio is thus 102.5; in 1920 it was 104; in 1910 it was 106. The men seem to be "weakening" under the strain of modern existence more rapidly than the women.

Eleven million and a half of us were under five years of age and twenty-eight million of us were over forty-five. 108,864,207 of us were white; 11,891,143 of us were negro. There were 2,019,696 of other races. Sixty-four per cent of us were of native parentage, sixteen per cent were of foreign parentage, twelve and three-tenths per cent were foreign born. Of the population over fifteen years of age, (43,881,021 ♂; 42,837,149 ♀) about 15,000,000 males and 11,000,000 females were single. 26,327,109 men were married, and 26,170,756 women (proving that Brigham Young seems to have a large following among the ladies!) There were 2,025,036 widowers and

4,734,207 widows; about a half million of each sex were divorced; though more women than men had been legally separated from their mates.

About 5,000,000 of us could not read; (the number who knew *what* to read is not given); 69,000,000 of us lived in the city and 54,000,000 of us lived in the country. (Ten years earlier 54,000,000 of us lived in the city and 51,000,000 of us lived in the country); 49,000,000 of us were gainfully employed, 11,000,000 of us so employed being women. (This does not include housewives, who from the census point of view, spend their time with folded hands.)

Fifty-four and a half million of us belonged to a religious organization; 57,084 of us were deaf-mutes; 63,489 of us were blind; 272,527 of us were in mental hospitals; 63,642 of us were in state institutions for feeble-minded; 116,626 of us were prisoners or being "reformed"; a millon of us were born; 2,674 of us were murdered; 4,996 of us committed suicide; 1,232,889×2 of us got married; 380,969 of us entered the country as immigrants, and 290,985 left the country for other parts.

Over twenty million of us were going to school at a total cost for the rest of us of two and a tenth billion dollars. The value of college grounds, buildings, and dormitories was a billion and a half dollars. It cost us a lot to be governed in 1930: about $70 per capita for state governments and $33 per capita for the National Government. (If England had tried to levy such a tax in 1730 the revolution might not have waited foı George Washington to grow up!)

We did a lot of banking and a lot of railroading, and a good deal of foreign commerce, and a great deal more domestic commerce, and a lot of other things. We manufactured many things (too many!), and raised many things, and we seem to have had too large a cotton crop and very much too much of a wheat crop.

Anyone with a stub pencil and a gift for figures can find an enormous amount of interesting material in this dry volume. Perhaps if the pencil holds out he can find out what happened to those 200,000 missing wives and why there are two million and a half more widows than widowers. Marriage *does* seem to be rather a problem for the male!

R. C.

Statistical Mountains—Correlational Molehills

CHILDREN'S BEHAVIOR PROBLEMS, A Statistical Study Based upon 5,000 Children Examined Consecutively at the Illinois Institute for Juvenile Research, *by* LUTON ACKERSON, Research Psychologist, Institute of Juvenile Research and Behavior Research Fund, Chicago. Pp. 268. Fifteen Chapters. 57 Figures. Price, $4.00. University of Chicago Press, Chicago. 1931.

ANYONE who has been, as a child, a close and mystified observer of his own conduct, who has regretfully noted the behavior of his offspring, or has been the scandalized witness of the acts of their associates, will appreciate the magnitude of Mr. Ackerson's problem. It is not made any easier by the fact that the data are not his own observations but were gathered by different members of the staff of the Institute for Juvenile Research. Readers are assured, somewhat impressively, by a director, that the individual studies of children—upon which the statistics are based—"represent in every instance the combined analysis of the psychiatrist, psychologist, physician, and psychiatric social worker." A justly retributive alliteration, for the children had not been minding their P's and IQ's.

One gathers that the institutional staff is rather proud of the richness of the compilation of juvenile iniquity which it has assembled, for the round total of 5,000 cases is frequently mentioned. This may be what prompts the author to spread his material out in page after page of tables of notations. One is actually able to apply an old

statistical stand-by, for if the items against two of the most heinous individual cases are laid side by side—and they are—they extend six pages!

One doubts immediately the efficacy of the correlation coefficient as a tool to clean up so heterogeneous an Augean litter, particularly when the information gets to the statistician second or third hand and deals with misdemeanors about which children and their parents habitually lie.

The author comments conscientiously upon the subjectivity of the data and rather näively admits his relief at getting any correlations at all. He comforts himself with the belief that inaccuracies would reduce but not produce correlations.

The opus major of the monograph is a tremendous series of curves and correlations. The material was divided into "personality" and "conduct" problems, the notations of the former group depending upon the definitive skill of staff members. Then for each sex the individual problems, with frequencies over 30, were correlated with chronological age and, subdivided into two age groups, with intelligence quotient. Special subdivisions of the material were tested in many instances.

The presentation of the data is a model of statistical arrangement; results show at a glance. And thus it takes little more than a glance to see the meagreness of the issue of this montanic statistical travail. Most of the coefficients are of negligible size, while some of the most pronounced ones are of negligible informational value. Bed-wetting is shown to be negatively correlated with chronological age, but this fact was long since established by mothers who used merely rule of thumb, or perhaps rule of palm methods. A negative correlation between age and illegitimate pregnancy would have been "news," but naturally a positive one was obtained.

Intelligent children as a group proved to have some more desirable and many more *un*desirable traits than stupid ones, the correlation between I. Q. and bad behaviour being particularly pronounced in children below 13 years. Whether, after this age, they are better behaved, or more careful, or given up as a bad job and not taken to a juvenile clinic, it is hard to say. The author recognizes selection in the cases as a "formidable limitation," although he finds no evidence of a pronounced distorting effect from selective factors. Judging by what they did, they were a fairly average group of children, and an honest reader, after a reminiscent blush, would be inclined to say about even the worst of them: "There but for the grace of God go I."

It seems to the reviewer that a fair conclusion to come to with regard to this monograph is that the author has done a well planned and painstaking piece of work upon very unfavorable material and that the results obtained are not particularly impressive. More valuable information will be obtained when the combined analysis of the psychiatrist, psychologist, physician and psychiatric social worker is as ineluctable as it sounds.

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Origin of the Slavs

L'ESPANSIONE DEGLI SLAVI, by Haskel Sonnebad. Comitato Italiano per lo Studio dei Problemi della Popolazione, Serie I, Vol. I. Pp. 238. Rome, 1930.

Placing the origin of the Slavs in the region north of the Carpathians, the author holds that they resulted from a fusion of three elements—a pre-Slav group, a Baltic race, and the Nordics. Since this origin, it appears that there has been a progressive tendency toward round-headedness. The diffusion of the group eastward, southward, and westward, is traced in detail with a wealth of reference.

Paul Popenoe.

PARTLY FERTILE HYBRIDS OF COMMON WHEAT WITH KHAPLI EMMER*

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ORDINARILY common wheats which have 42 chromosomes, may be crossed readily with wheats of the Emmer group, which have 28 chromosomes. The hybrids are vigorous and fairly fertile and several investigators have succeeded in combining a desirable character from a wheat with the lower chromosome number with desirable characters of a common wheat to produce a variety of common wheat of considerable promise agronomically.

Khapli Emmer (*Triticum dicoccum*) is an early maturing variety which possesses very high resistance to most physiologic forms of wheat stem rust. Attempts to transfer this resistance to common wheats however have proved almost completely unsuccessful because of the death of most F_1 hybrids before maturity. Puttick[6] reported that crosses between the common wheat variety Marquis (*T. vulgare*) and Khapli set no seed. Hayes and Stakman[2] report disappointing results from attempts to cross Khapli with Marquis wheat. Hynes[3] however secured four F_1 plants from crosses of Federation (*T. vulgare*) with Khapli which were quite normal and produced heads bearing plump grain. Thompson and Hollingshead[9] reported that of 80 hybrid grains obtained from crossing Khapli with *T. vulgare* many failed to germinate and all but two of the seedlings died before reaching a height of eight inches. Only those two ever formed heads.

Waterhouse[10] made a long series of attempts to secure hybrids between different varieties of *T. vulgare* and Khapli Emmer. He used Federation, the variety used by Hynes, as the *vulgare* parent in over half the crosses but from 162 grains set he did not obtain a single mature plant. Including all varieties 1,860 pollinations were made, 284 hybrid seeds were obtained and only two of these produced tiny ears with two spikelets on each side. In one spikelet on one ear three small stamens were produced, and in one spikelet of the other ear a small pistil was formed. No seed was obtained. The other plants were stunted, but it was noted that there was a difference in degree of development attained by the hybrids, according to the *vulgare* parent used, the plants dying at heights averaging from 6 to 12 inches depending on the *vulgare* parent.

The Hybrids

It occurred to the writer that it might be possible to produce more vigorous hybrids between Khapli Emmer and a variety of common wheat if the latter were itself a derivative of a cross between a common and an Emmer wheat. For this purpose H-44-24, a variety produced by McFadden[5] from a cross between Marquis and Yaroslav Emmer, was chosen. Preliminary attempts to obtain this cross in 1929 resulted in several seeds from which three supposed hybrids were obtained but they died in a period of dry weather before maturing.

In 1930 Mr. Julian Hessel pollinated H-44-24 and Hard Federation with Khapli pollen for the writer. Table 1 gives the results obtained. The percentage of successful pollinations when H-44-24 was the *vulgare* parent were

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† Cereal specialist.

PARENT AND HYBRID FORMS

Figure 7

At the left is one plant of the female parent variety, H-44-34. The other two plants are hybrids of this form with Khapli Emmer. The Emmer wheats (*Tricitum dicoccum*) have twenty-eight chromosomes; ordinary wheat (*T. vulgare*) has 42 chromosomes. Khapli Emmer, an early maturing strain with high resistance to rust, does not produce fertile hybrids with most of the varieties of ordinary wheat. The variety H-44-34 is a form which has been originated from a hybrid of common wheat and another variety of Emmer. The presence of this Emmer "blood" (or, more accurately, of Emmer *chromosomes*) facilitates the obtaining of fertile hybrids.

as high as in several crosses made by Mr. Hessel in the same season between varieties of wheat with the same chromosome number. The difficulty in obtaining hybrid seed when Hard Federation was the *vulgare* parent is in agreement with Waterhouse's results, for this was one of the varieties he used.

Most of the hybrid seeds (46) were sown in pots in the greenhouse and practically all of them germinated and grew into normal vigorous seedlings. Infected with stem rust, *Puccinia graminis tritici* form 21, some of the hy-

HEADS OF PARENT VARIETIES AND OF HYBRIDS

Figure 8

At left, two heads of variety H-44-24; right, two heads of Khapli Emmer; and, center, two heads of the F_1 hybrid between these forms. Fertile seed has been obtained in the green house, but under more rigorous conditions the hybrid scarcely reaches maturity.

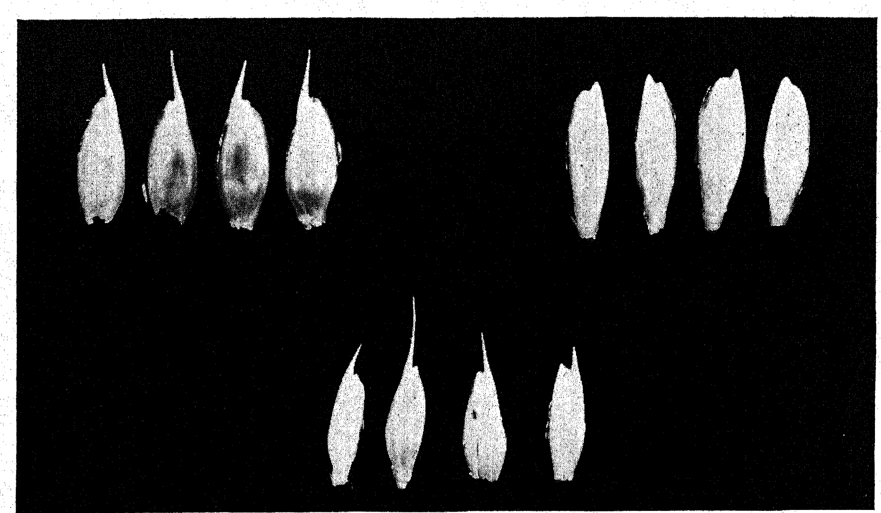
brid seedlings gave a reaction of 1 +. Khapli under the same conditions gave a 1— while H-44-24 gave a 3=c to 3c+ reaction. The hybrids were therefore not quite as resistent to this form of rust as the Khapli parent, but much more resistant than the H-44-24 parent. The inoculation with the rust and the readings were kindly done by Dr. T. Johnson.

As the plants approached the heading

TABLE 1.—Results obtained from pollinating two *vulgare* wheat varieties with Khapli Emmer pollen.

Vulgare Parent	Number of Florets Pollinated	Number of Grains set
H-44-24	125	58
Hard Federation	149	0

stage some of the leaves began to turn yellow and this yellowing increased as the plants matured. Figure 7 is a picture of one pot of H-44-24 and of two pots of the hybrids, each with three plants. The hybrids were not as far advanced as the H-44-24 plants when this picture was taken. The mature hybrids usually had several well developed heads per plant and a number of stunted tillers. Figure 8 is a picture of typical heads of the hybrids and of the parental varieties. The hybrids resemble the Khapli parent in shape of spikelets and heads. The outer glumes (Figure 9) of the hybrids are like those of Khapli in shape, but



GLUMES OF PARENTS AND HYBRIDS

Figure 9

Above, at left, are the glumes of H-44-24 and (right) of Khapli Emmer. Below are the glumes of the F_1 hybrid. The glumes resemble the Khapli parent in shape, but they resemble the other parent in smoothness and in length of apical tooth.

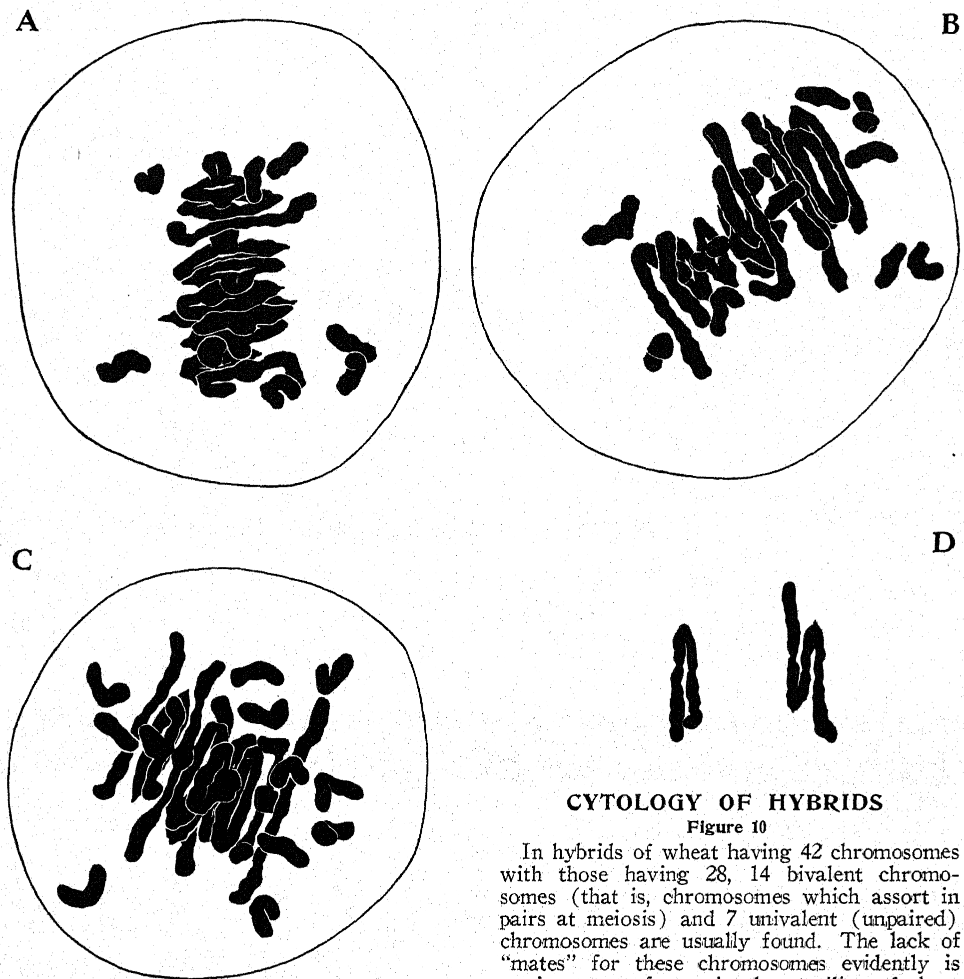
they resemble the H-44-24 parent in smoothness. In the length of the apical tooth, which is quite variable, they are more like the H-44-24 parent.

There were sometimes none, but usually one to several grains per head; sterility is therefore considerably higher than in other previously described hybrids between wheats with 42 and 28 chromosomes (pentaploid hybrids). These grains germinated well and the F_2 plants varied widely in appearance as is characteristic of this generation of pentaploid wheat hybrids.

The reserve of the hybrid grains (to grow into F_1) was planted outside early in May, 1931. They germinated well and the plants grew normally until a period of dry weather occurred in early June when the leaves began to turn yellow. In spite of artificial watering and later adequate rain, the plants produced none or few tillers, headed when very short, and on the whole were considerably poorer and weaker plants than their greenhouse sisters had been. It would seem that these hybrids are unfitted to withstand unfavorable weather conditions.

Cytological Studies

Meiotic divisions of the pollen mother cells of the F_1 were examined from paraffin sections of anthers fixed in chrom-acetic formalin preceded by a short time in Carnoy, and from anthers fixed in Carnoy, washed in alcohol and crushed in acetocarmine, as described by Thompson[8]. Most of the cytological work was done on greenhouse material. As in other pentaploid hybrids 14 bivalent and 7 univalent chromosomes were usually to be found at the first meiotic metaphase. Figure 10*A* is a side view of such a cell but some of the bivalents are hidden. However more than 7 univalents (Figures 10*B* and 10*C*), and a corresponding decrease in the number of bivalents could also be found. In cells which showed more than 7 univalents the common number was 9, and the highest, which was seen rarely, was 13. The proportion of cells showing more than 7 univalents varied markedly in different samples of material but in typical anthers it was estimated to be about 20 per cent. This estimate is based on the examination of 136 cells



CYTOLOGY OF HYBRIDS

Figure 10

In hybrids of wheat having 42 chromosomes with those having 28, 14 bivalent chromosomes (that is, chromosomes which assort in pairs at meiosis) and 7 univalent (unpaired) chromosomes are usually found. The lack of "mates" for these chromosomes evidently is an important factor in the sterility of these hybrids.

At *A*, above, is shown a cell at first metaphase, with the fourteen paired chromosomes in the center, and the seven unpaired chromosomes around this group. Four of them are across the lower edge of the picture, and three across the top, one of them projecting over the central group. At *B* is shown another first metaphase, with nine univalent chromosomes in addition to the paired chromosomes in the center. Eight of these are distributed around the central group and one lies over it. At *C* is a similar figure showing eleven univalent chromosomes in first metaphase cell. At *D* are shown trivalent and tetravalent chromosomes (i. e., three and four chromosomes united together) found in the cells of these hybrid forms.

from several anthers in particularly favorable areas of slides from aceto-carmine material where slight flattening of the cells and good staining made it possible to determine the number of univalents in most of the cells with a fair degree of accuracy. An estimation based on a larger number of cells from groups in which the number per cell could not be distinguished in a greater proportion of cases was about 24 per cent. An even higher proportion of cells with more than 7 univalents, conservatively estimated at 50 per cent, was found in anthers from two late tillers.

In the pollen mother cells of most pentaploid wheat hybrids described by

various writers, 14 bivalents and 7 univalents seems to have occurred very regularly (Watkins[11]). Sax[7] however, found 3 cells with more than 7 univalents at first anaphase in a total of 36 cells in a *T. vulgare* × *T. durum* hybrid. Kihara and Nishiyama[4] report 13 bivalents and 9 univalents to occur rarely in *T. vulgare* × *T. durum* hybrids, and Aase[1] found 7 to 11 univalents in the same hybrids. It would appear that H-44-24 × Khapli hybrids show more deviations from the usual combination of bivalents and univalents than do other pentaploid hybrids reported. The writer does not know of any reported observations on other *T. vulgare* × *T. dicoccum* hybrids however, but has made preliminary studies of *T. vulgare* (var. Marquis) × *T. dicoccum* (var. Vernal) hybrids. Pollen mother cells with more than 7 univalents were to be found in all samples of the material studied and the proportion of such cells estimated from very good material was about 18 per cent. This may be high, however, for it was partly based on anthers from some short early tillers which were produced shortly after the plants had been transplanted from the greenhouse to the open. Anthers from later more vigorous tillers had a smaller proportion of such cells. More extensive work on both hybrids would have to be done before a decision could be reached whether the H-44-24 × Khapli hybrids are more irregular than the Marquis × Vernal hybrids.

As in the *T. vulgare* × *T. durum* hybrids described by Kihara and Nishiyama[2] and by Aase[1] trivalent and tetravalent chromosomes were to be seen rarely in the H-44-24 × Khapli hybrids (Figure 10*D*). No attempt was made to ascertain their frequency.

Following the first metaphase the meiotic process follows the type so often described for other pentaploid wheat hybrids. The univalent chromosomes usually divide at the first anaphase though they do not always do so, for frequently less than 7 univalents are to be found dividing at this stage. Probably those which do not divide at the first, divide at the second division. Lagging chromosomes at the second division give rise to micronuclei in many of the cells of the young tetrads.

The mature pollen contains a considerable proportion of empty or partly empty grains when examined in lactic acid and fuchsin. The proportion of such "bad" pollen in several counts varied from 34 to 51.4 per cent. Usually the proportion of such pollen in pentaploid hybrids is about 25 per cent (Watkins[11]). The higher proportion in the H-44-24 × Khapli hybrids probably accounts in some degree for their higher sterility.

The fact that these partly fertile hybrids between Khapli Emmer and a common wheat have been obtained, is of some theoretical as well as some practical interest. They differ in several characteristics from the Hard Federation × Khapli hybrids described by Hynes, being considerably less vigorous and fertile. Indeed some doubt has been cast on the real nature of Hynes' hybrids by the fact that Waterhouse was unable to secure normally maturing hybrids using the same two parental varieties. In any case it is certain that hybrids of Khapli with a number of different *vulgare* varieties usually die before maturing. Whatever the cause of this lack of vigor may be, the vigor of such hybrids may be increased, and some grains may be obtained, if the *vulgare* parent is H-44-24, a variety which has part of its genetic make up of Emmer origin. Possibly other varieties of similar origin, such as Hope, may cross with Khapli to give a still more vigorous and more fertile hybrid.

Summary

Partly fertile normally developed hybrids of a common wheat with Khapli Emmer have been obtained. The common wheat parent (H-44-24), is itself derived from a cross between a common (*T. vulgare* var. Marquis) and an

Emmer (*T. dicoccum* var. Yaroslav Emmer) wheat. The hybrids grew best in the greenhouse and appeared to be rather unfitted to withstand unfavorable weather conditions.

Cytologically they are more irregular than other pentaploid wheat hybrids previously described, having more than seven unpaired chromosomes in a portion of the pollen mother cells. It is not certain however, that they are more irregular than Marquis × Vernal hybrids which are hybrids between different varieties of the same two species.

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Physiology of the Cell

A TEXTBOOK OF EXPERIMENTAL CYTOLOGY, *by* James Gray, M.S., F.R.S., Fellow of King's College, Cambridge, and Lecturer in Experimental Zoology, University of Cambridge. Pp. 516. 18 Chapters, 3 Plates, 205 Figures, 75 Tables. Price $7.50. New York, The MacMillan Co. 1931.

THERE are a number of excellent text-books of cytology in which the cell is treated primarily as a morphological unit. In Gray's "Experimental Cytology," however, it is considered rather as a "convenient unit of functional activity."

Students of the cell have too frequently belonged to one or the other of two somewhat opposed camps, the morphological and the physiological. The morphologists, who are more apt to call themselves cytologists than are the physiologists, have shown a special inability or unwillingness to understand and utilize the methods and conclusions of the physiologists, and it is time that a greater degree of familiarity and a larger measure of cooperation between the two groups were established. Gray's "Experimental Cytology" should be of considerable assistance to morphologists as they seek to evaluate the results of physio-chemical investigations upon the nature and activity of protoplasm.

The book is especially well suited to this use, as well as to use as a text-book, inasmuch as it displays throughout a well-balanced judgment with respect to the results already accomplished by the methods of physical chemistry. While the analytical methods of chemistry and physics are utilized as far as possible in interpreting the behavior of cells, both vegetative and reproductive, excessive claims as to past or future achievements are avoided, and there is an evident unwillingness to adopt theories which are not subject to immediate experimental analysis, or for which there is inadequate foundation in fact. The author refers to the "danger of underestimating the complexity of the whole problem," and insists that the

"biologist should continue to regard the situation from his own peculiar angle," as a check, perhaps, upon over-enthusiastic claims of the physical chemist. Emphasis is placed upon the fact that all analogies between protoplasm and purely inanimate systems sooner or later break down, and that there is no real parallel between them. Thus, we read,— . . . "we can only ascribe to nuclei the properties of a highly instable and delicately poised system, whose instability is probably an essential feature of the living state and which finds no adequate parallel in inanimate systems." Or again,—"Try as we will, it seems impossible to point to any inanimate system (endowed with the known chemical or physical properties of the nucleus) which in any real way possesses the requisite complexity whereby it might reflect even feebly the biological facts." And once again,—" It seems more rational to regard living material as a state of matter where the constituent molecules are organized in a way quite unknown in the inanimate world, and which will only be elucidated by methods of analysis which have yet to be discovered. The study of protoplasmic structure clearly illustrates the limitations of a purely physical conception of biological problems. Without doubt the underlying mechanism of cytoplasmic differentiation is of an atomic or electronic nature, but until we have the means to explore the situation in much greater detail, a knowledge of our ignorance is perhaps the most valuable asset we can hope to possess."

While on the one hand, therefore, the methods of physics and chemistry are adopted as far as possible in the solution of cytological problems, emphasis is laid on the other hand, upon the unique complexity of living matter and upon our "very profound ignorance" of the fundamental nature of animate systems. The constantly critical attitude in evidence throughout the book greatly enhances its value. As a summary of the newer point of view in cytology, therefore, this is a worthy contribution, and should meet a great need.

Bibliographies are found at the end of each chapter, and author and subject indexes are placed at the end of the volume. A summary of chapter headings will indicate the scope of the work: The Cell as a Unit of Life; The Cell as a Physical Unit; Cell Dynamics; The Cell as a Colloidal System; The Physical State of Protoplasm; Cell Membranes and Intercellular Matrices; The Nucleus; Mitosis; Cell Division; The Shape of Cells; The Growth of Cells; Cell Variability; The Equilibrium between a Living Cell and Water; The Permeability of the Cell Surface; The Nature of the Cell Surface; The Germ Cells; Contractile Cells; Phagocytosis.

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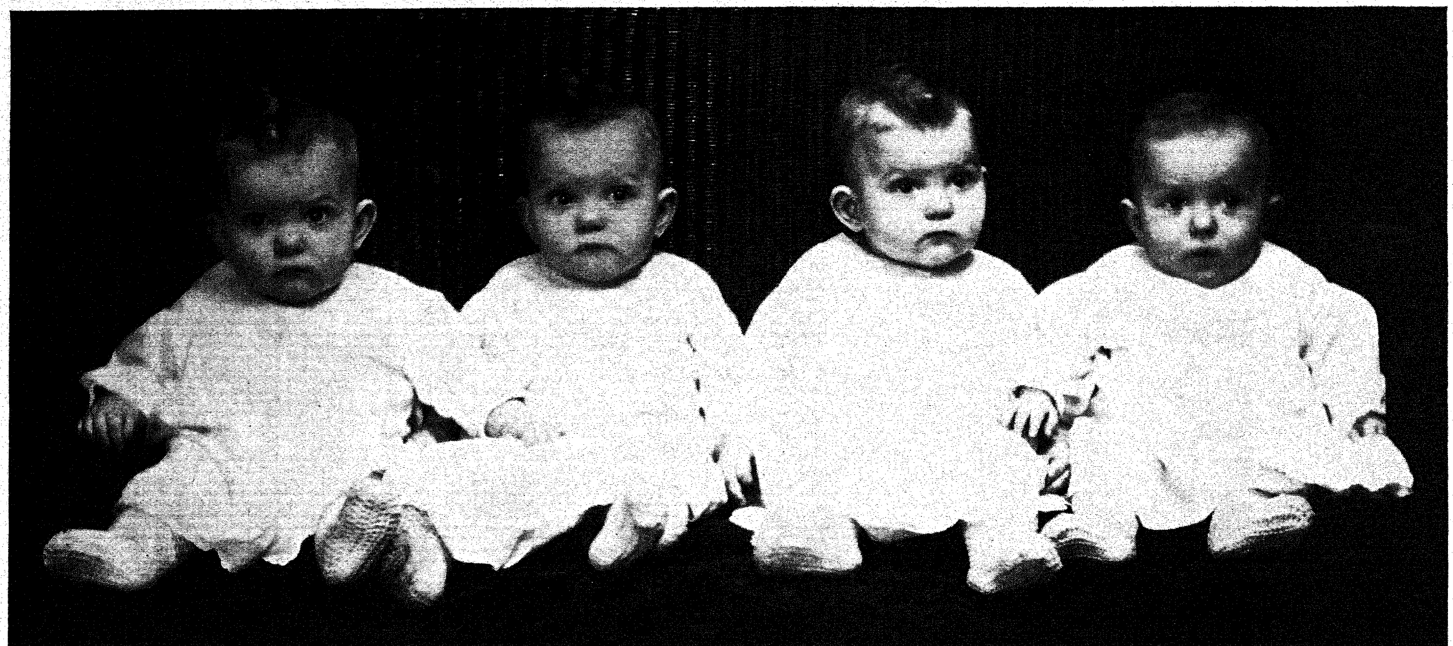

FOUR OF A KIND

Frontispiece

These four young ladies arrived in Michigan on May 9, 1930. While at birth they differed as much as a pound and a half in weight, nevertheless the resemblance which they show in a number of characteristics, and in fingerprint patterns, indicates that they are identical quadruplets. If this is the case, genetically each girl is what the others might have been. If the fortunes of life bring them different environments and experiences during development, these young ladies may, in the years to come, teach us much regarding the still controversial subject of what heredity can do in influencing development,—and what environment.

IDENTICAL QUADRUPLETS*

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HUMAN twins of the identical or monozygotic type are arousing considerable interest at the present time. It appears worth while, therefore, to report the occurrence of a set of quadruplets who are probably monozygotic. Identical triplets have been reported by Clarke and Revell[1] and by Komai and Fukuoko,[6] but so far as the writer is aware no previous study of monozygotic quadruplets has been made.

A, B, C, and *D* are four baby girls born in Michigan on May 19, 1930. Their arrival was rather widely heralded by the daily papers at the time. According to the attending physician, they were monochorial, but unfortunately, the afterbirth was not preserved. Diagnosis of zygosity according to the state of the chorion has been questioned by some recent investigators, including Siemens,[10] v. Verschuer,[11] Curtius,[3] Newman,[9] and Komai and Fukuoko.[6] It has been shown that apparently identical twins may be born with separate chorions and that, very exceptionally, fraternal twins may possess only one chorion. However, if the diagnosis of the foetal membranes is correctly made and secondary fusions of adjacent chorions are not mistaken for the monochorial condition, the data secured thus far favor the view that monochorial twins are almost always monozygotic. This evidence, therefore, renders it probable that the quadruplets under consideration are identical, but it does not furnish conclusive proof that such is the case.

The babies, when seen by the writer early in January, 1931, appeared bright and healthy. Their weights were as follows:

	Weight at birth	*Wt. on Dec.* 25, 1930
A	4 lb. 8 oz.	15 lb. 8 oz.
B	3 lb. 5 oz.	13 lb. 0 oz.
C	4 lb. 4 oz.	14 lb. 12 oz.
D	3 lb. 0 oz.	10 lb. 0 oz.

The babies are listed in the order of their birth, *A* being the firstborn and *D* the youngest. The smallest baby, *D*, was cared for in an incubator after birth, and at eight months of age was still smaller than her sisters. However, according to the mother, *D* was beginning to gain weight more rapidly and the difference between her and the others was becoming less marked. The quadruplets are the only children in the family.

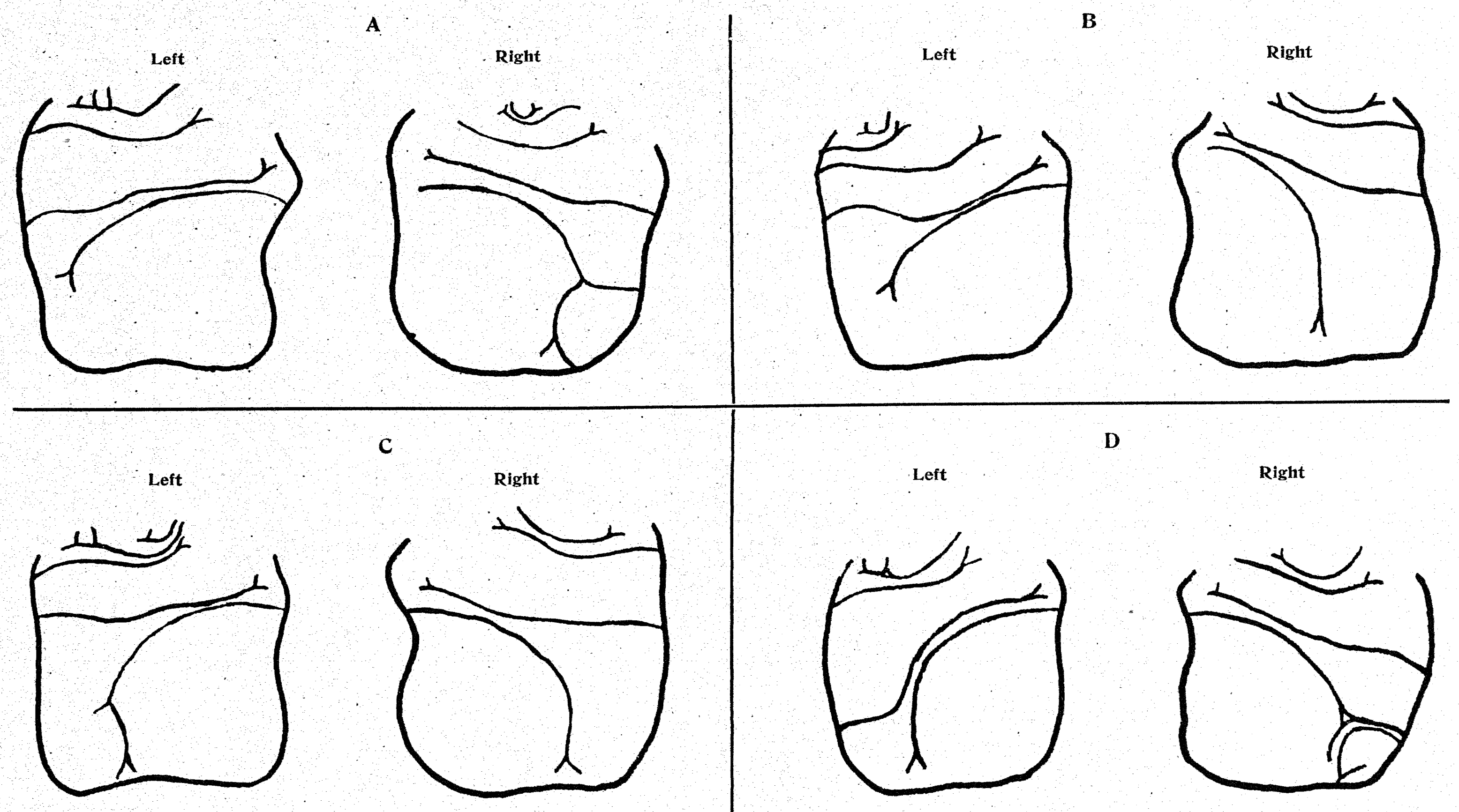
At eight months of age all four babies had light brown hair. While their hair was of the same color, it was noticeable that *D*'s hair was not as long as the others.' This probably accounts for *D*'s hair appearing less curly than that of her sisters, as shown in Frontispiece. In a more recent photograph *D's* hair is very curly. In color of eyes they show no difference. At eight months of age, according to their mother, the eyes of each were becoming darker. No differences were observed in the size and shape of the quadruplets' ears.

On March 11, 1931, all were stricken with influenza, being ill for about one week.

Palm prints and sole prints of the

*Papers from the Department of Genetics, Agricultural Experiment Station, University of Wisconsin, No. 81. Published with the approval of the Director of the Station.

†The author wishes to express his thanks to Dr. L. J. Cole, Professor of Genetics at the University of Wisconsin, for the interest which he has shown in the study and for the many helpful suggestions he has made. He is also indebted to the American Genetic Association for a grant to defray travelling expenses.



PALM PRINT PATTERNS OF THE FOUR GIRLS

Figure 1

The cross-resemblance of palm patterns is striking. Thus the four right hands are more alike than are the right and left hands of any one individual. Detailed cross-similarities are also remarkable, as in the case of the right hands of *B* and *C*.

babies were obtained although considerable difficulty was experienced in securing satisfactory prints. The digital patterns are all loops. The palm pattern formulae, using the method of classification developed by Cummins et al,[2] are shown below.*

A study of the palm print formulae and the diagrams in Figure 1 shows that the left hand of each individual is more like the left hand of her sisters than it is like her own right hand. The right hands of *B* and *C* are remarkably similar, and the resemblance is fairly close between the right hands of *A* and *D*.

The sole patterns also exhibit evidences of direct cross-resemblance.

Cross-resemblance between identical twins has been previously described by Newman [7,8] and Komai [4,5] and between identical triplets by Clarke and Revell[1] and Komai and Fukuoko.[6] Close correspondence of patterns of the hands and feet of the same side is the type of resemblance found in the most similar pairs of identical twins.

The mother expressed the opinion that the two larger babies, *A* and *C*, resemble one another very closely, while the two smaller ones, *B* and *D*, form another closely similar pair. A study of the friction skin patterns, however, fails to set off the children in this way, but strengthens the view that all four are derived from a single fertilized egg.

When one year old the babies were beginning to talk, and were able to say "bye-bye" and "pat-a-cake." *A*, *B*, and *C* were creeping and *D* was beginning to creep.

The close similarity of the four babies in friction skin patterns and other physical characters renders it probable that they are identical. It is hoped that additional data may be obtained as the children grow older.

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***Palm-Print Patterns of the Quadruplets.**

	Left		*Right*	
A	9/7.0.5″.5′-t′-V/A^c.	O.O.M.D	11.7.9.5′-tt′-V/L^u.	O.O.L.O.
B	7. 5″.5″.5′-t′-V/A^c.	O.O.O.L	9.0.5″.5′-t′-A^u.	O.O.M.M.
C	9/7.9.5″.5′-tt′-V/L^u.	O.O.L.D	9.0.5″.5′-t′-A^u.	O.O.O.M.
D	9/7.0.5″.3-t′-A^u.	O.O.O.D	11.0.7.5′-tt′-V/L^u.	O.O.O.M.

Photographed by Wilson Popenoe.

TEOSINTE WILD IN GUATEMALA

Figure 2

Teosinte plants in a field at El Progreso, Guatemala. This plant is the nearest wild relative of maize. Its distribution in nature is thus a matter of considerable interest to those who are trying to discover the origin of that remarkable crop plant, one of America's great contributions to the agriculture of the world. Teosinte was reported in Guatemala in 1869, but has not been found there since that time. The plant is today rather widely distributed about the world. While it has some value as a forage crop it has never been grown to any extent for that purpose.

THE REDISCOVERY OF TEOSINTE IN GUATEMALA

G. N. COLLINS
U. S. Department of Agriculture

Photographed by Wilson Popenoe.

REGION WHERE TEOSINTE WAS DISCOVERED

Figure 3

View near Jutiapa, Guatemala, showing the type of country in which the wild teosinte was found, and a group of "assistant agricultured explorers" in the foreground. This is in the valley of the Rio de Paz, about 133 kilometers from Guatemala City. The altitude is about 3,300 feet. Teosinte was found mostly in old cornfields—ranging cattle apparently prevent its distribution in unfenced ground.

THE following letter recounts the recent discovery in Guatemala of teosinte, *Euchlaena mexicana* Schrad., a plant which has attracted special interest as the nearest relative of our cultivated maize.

Prior to this discovery Euchlaena had not been reported as occurring spontaneously outside of Mexico, although Guatemala was supposed to have been the source of cultivated teosinte.

Teosinte became known to agriculture in 1869 when Rossignon, Director of Gardens in Guatemala City, sent seed to Paris. The plants failed to mature seed, even in southern France, but aroused much interest. In 1877 teosinte seed was offered by Vilmorin and in 1878 it appeared in American seed catalogs. The source of the seed is not known. As the years passed and botanists who collected in Guatemala failed to find Euchlaena, the source of the seed sent by Rossignon came to be questioned.

Rossignon gave Santa Rosa in Southeastern Guatemala as the source of his seed, but an article by David Guzman states that Camilo Galvan supplied the seed Rossignon sent to Paris and that it came from Esclavos, a town not far from Santa Rosa.

In March, 1931, while with the Al-

lison V. Armour expedition, Mr. James Kempton and I visited these localities in company with Dr. Wilson Popenoe, but failed to find Euchlaena, either wild or cultivated. It was learned that Dr. N. I. Vavilov had recently visited that district on the same quest, and with the same negative result. The natives in these localities knew the name teosinte, but applied it to Tripsacum, a quite different grass. The locality where Euchlaena was finally discovered is in the same general region, but nearer the coast. All the localities mentioned in Dr. Popenoe's letter are in southern Guatemala near the border of El Salvador.

The finding of this important plant in so restricted a locality is another striking demonstration of the need for a detailed botanical exploration of Tropical America.

Since this letter is the original source of information regarding teosinte in Guatemala it is published without change, although it contains some irrelevant personal references. The text of Dr. Popenoe's letter follows:

SERVICIO TECNICO DE COOPERACION AGRICOLA

12 Calle Oriente No. 1—Guatemala, C. A.
January 1st, 1932.

DEAR MR. COLLINS:

I have sent a radiogram to Boston, asking Mr. Rowe to advise you as follows: "Teosinte abundant spontaneous apparently annual Jutiapa region herbarium specimens notes photographs follow." The herbarium specimens are packed and will be shipped by registered mail this week; the photographs will be sent as soon as they can be developed and printed; and here are the notes.

When Dr. N. I. Vavilov was here, just about a year ago, he spoke to Jorge Benitez, who has charge of our little experiment station at Antigua, regarding teosinte. I gather from Jorge that the Rossignon incident was mentioned. Jorge made some inquiries, after Vavilov had left here, in an endeavor to locate seed. It happened that Vicencio Lopez, a youth from El Progreso, Department of Jutiapa, had recently joined the staff of the Servicio Tecnico, and learned of the quest. He told Jorge he was familiar with teosinte; that it grew wild near his home; and that he would get seed.

He did, and we sent the seed to you. When your letter of 7 December arrived, acknowledging receipt of the seed and confirming our belief that it is the thing you are after, we decided to look into the matter more fully; but waited to talk with Vicencio before so doing. When at Panajachel a few days ago, I had opportunity to get further information from him, also a letter to his brother, who lives at the Lopez home in Progreso. Armed with this letter, Dorothy and I started out yesterday morning in Mr. Armour's Ford.

We followed the road on which you, Jim, Bates, and myself came back from the Rio de los Esclavos. We passed through Cuajiniquilapa, and there was the little girl, hanging out of the window of the hotel, with a wistful look in her eye. We crossed the Rio de los Esclavos, passed the point at which we turned back last spring, and shortly afterward began climbing up into the hills. We left the watershed of the Rio de los Esclavos and after a bit of travelling, came down out of the hills onto the Jutiapa plain, which drains into the Rio de Paz.

We passed through Jutiapa, and went on 12 kilometers farther, to El Progreso, a small town 133 kilometers from Guatemala City by the road we followed—which, by the way, is the present main road from Guatemala to El Salvador. We found Vicencio's brother, Benjamin, in a little hut, some hundreds of yards beyond the edge of the town; and after telling him our

mission, he led us across the narrow valley in front of his place, and up the slope a few hundred feet to an abandoned cornfield, where we saw teosinte in abundance—now dry, and most of the seed fallen, but plenty of it left to make collecting easy. It was at this spot that we took the series of photographs we hope to send you shortly, and collected the dry plants which are being sent you in this mail.

The altitude of this spot, as closely as we could determine it with the aneroid barometer, is 3,300 feet. The soil is a heavy residual clay, black near the surface, and light brown to chocolate brown below.

We talked with Benjamin Lopez regarding the occurrence of teosinte in this general region; we talked with some of his neighbors; and we started back toward Jutiapa, observing teosinte at several places along the road, growing always in old cornfields—"rastrojos" as they are termed locally. This morning, on starting homeward from Jutiapa, we again watched the roadsides for teosinte, and found it in a number of spots until we got to the Quebrada Anonal—one of the confluents of the Rio de Paz—after which we saw it no more, nor were we able to find anyone, in the houses along the way, who believed that it grew in the neighborhood.

Summing up the results of the trip, we can say:

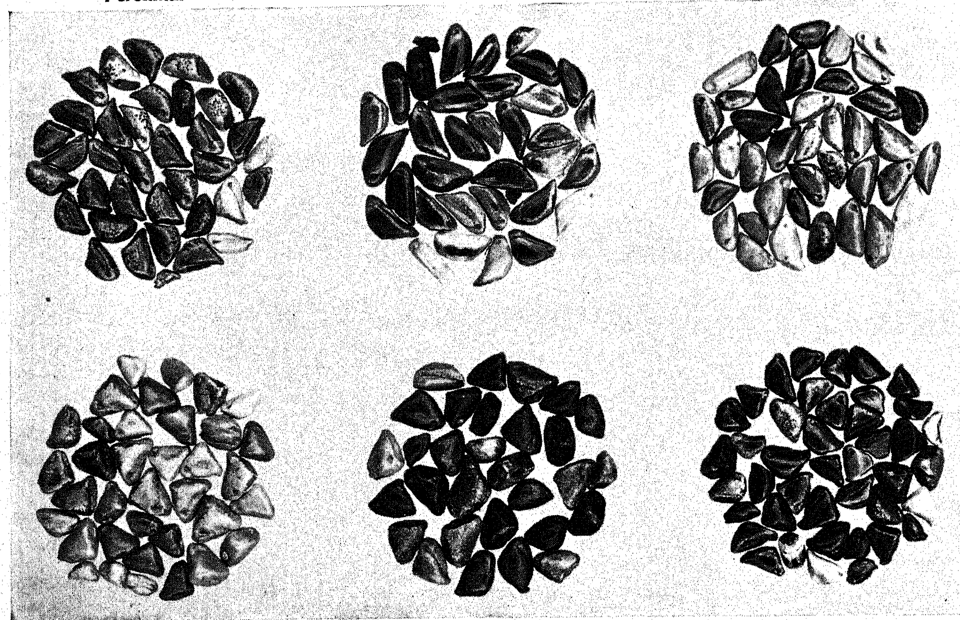
We have personally seen teosinte along the roadside, about ten miles on each side of Jutiapa. Vicencio Lopez says he has seen it northward as far as the Lago de Retana, and at Asunción Mita. Don Julio Drago of Jutiapa says he has seen it very abundantly on the road between Jutiapa and Jalpatagua. We have other accounts of its occurrence in the same general region. But it is to be noted that we found it *only* in the region drained by the Rio de Paz. Once we got across the divide onto the watershed of the Rio de los Esclavos we lost it completely.

By one of those happy coincidences on which Dr. Fairchild loves to dwell, Don Jorge Garcia Salas came in just as I was starting this letter, and told me he had received a quantity of teosinte seed from Don José Marcucci, who lives at Moyuta, about half way between Jutiapa and the Pacific Ocean. Don Jorge says that he has personally seen teosinte growing very abundantly on the plain between Moyuta and El Molino, a small place on the headwaters of the Rio de los Esclavos. This is the first reliable account we have had, which took this plant out of the basin of the Rio de Paz.

As regards the altitudinal range we cannot say much; but we think it safe to estimate that it goes at least as low as 2,500 feet and as high as 3,500. How much these limits will be extended by later investigations remains to be seen.

The plant was not seen by us in any but a dry state. It is said to come up with the rains, which commence about May or June, and to mature its seed at the end of the rainy season, which is about the end of November. As regards your question concerning the annual or perennial nature of the plant, Vicencio told us, the last time we talked with him, that it is perennial; but his brother, Benjamin, says Vicencio has forgotten; and from the appearance of the root system we feel confident it is an annual. As far as we saw, it is not planted; but in talking with natives of the region, we got several vague statements to the effect that it is *sometimes* planted, though no specific cases were mentioned. The people say it just comes up in the "rastrojos"; that it is excellent fodder for cattle, and is sometimes cut for this purpose; and that so far as they know, it has always been in the region. We heard nothing to suggest that it had been introduced.

The word "rastrojo" is used to signify a cornfield which has made its crop, and on which new planting has not yet been carried out. So far as we saw, the occurrence of teosinte (which



SEED OF TEOSINTE

Figure 4

Seed of teosinte from the locations indicated, showing the range of variation in form. The newly discovered teosinte from Guatemala resembles the Florida form more closely than does the Mexican (lower row), probably because the Florida form originated from teosinte sent from Guatemala to France in 1869, and sent from there to America, about 1877.

seems to be called, by the way, "teosinte," "teosinta," and "teosinto") is practically limited to these old cornfields. A rather unusually intelligent old fellow with whom we talked pointed out that teosinte springs up in any fenced area; and it then struck us forcibly that it would be hard for the plant to grow elsewhere, since cattle roam all unfenced lands in this area, and teosinte is relished by cattle.

There is a belief locally, recounted by Benjamin Lopez, to the effect that teosinte, if planted alongside maize, becomes maize, in the third generation. This same belief was mentioned by Don Jorge Garcia Salas this afternoon; and he states that a friend of his actually made the test, and exhibited, some years ago, ears which had been obtained in this way.

So much for our story. Going back to your letter of 4 May last, you recount that one Camilo Galvan, living in Esclavos, brought plants of teosinte from the mountains, grew them for two years, and sent a communication and seed to the Sociedad Economica, where Rossignon finally picked up the seed and sent it on to France. Don Jorge Garcia Salas knows some of the old timers here who may have files of the proceedings of this society, and tomorrow he has promised to begin a search for the information requested in your letter. I do not know that anything will come of this; but in the light of what we now know regarding the presence of teosinte in the Department of Jutiapa, it is not difficult to believe that Camilo Galvan brought seed to Esclavos from some point a few miles east or southeast, and that old Rossignon should have copped onto the bottle when he saw it at the Sociedad Economica. It is still difficult for us to

believe, however, that this seed produced perennial plants.

We shall send you a further quantity of seed—some of our own collection, and some from Don Jorge Garcia Salas —hoping that you will learn more from growing it than we can tell you. And if you will let us know what further data we can get you here, we shall do our best. Our one regret is that we had not dropped onto this recent lead at the time you and Jim were here; for we know that you would have found an even greater thrill in the trip to Jutiapa than have Dorothy and I though we have enjoyed it immensely. Perhaps you can come down again.

With best regards from both of us,

Yours very sincerely,

WILSON POPENOE.

"Oxygen and Everest"—Another Instance of Sense-Differences

FOR many months the correspondence columns of the British scientific weekly, *Nature,* have been spiced with widely divergent views regarding the value of oxygen respirators for high mountain climbing. One school of thought, favoring the use of oxygen apparatus, has been opposed by equally competent authorities who could see no value in such mechanical contrivances, the weight and bulk of which, it was argued, more than offset their value to the climber. The latest contribution to this controversy, by Dr. Yandell Henderson, of Yale University, brings out an interesting point that unexpectedly places this controversy in the field of inherited sense differences, which has recently been discussed in this Journal.

Dr. Henderson points out that there are great differences in the ability of different people to acclimatize to high altitudes. Acclimatization is a matter of altering the amount of acid and alkali in the blood so that the balance remains the same as at lower levels, but with the percentage amounts much decreased. For a person capable of becoming thus acclimated, the oxygen mask, by upsetting this delicate acid-alkali balance in the blood, produces acute distress on the slightest exertion. For the type that does not acclimatize readily, the oxygen-alkali percentage never is reduced to a "normal" high altitude level. For such individuals the oxygen mask is a God-send, and affords amazing relief when exertion is undertaken at high altitudes.

Thus this controversy has as a basis a definite physiological, possibly genetic, difference. Those who can see no value in the oxygen apparatus take this view because to them it is worse than useless. The disagreement, as Professor Henderson says, is not in their minds, but in their respiratory centers. "How can a man of either opinion accept the other when his own is to him as obvious as breathing? The whole science of respiratory physiology would today be different from what it is if its author, Dr. Haldane, had not happened to have had a respiratory center of the type that is most completely controlled by carbon dioxide, yet acclimatizes fairly easily. Those who disagree with him on such matters as acclimatization, do so, not so much because they think differently, as because they breathe differently."

Thus we are again reminded that political equality and biological equality are two distinct things. The former is an ideal of government; we are equal units in the eyes of the law even though one of us is an acclimatizer and one is not. The paradox of "different equals" offers few difficulties until we begin to make unwarranted extensions of the principle, until we try to apply it where the evidence of our senses tells us it does not apply. Neither legislation nor education can make a "non-acclimatizer" into an "acclimatizer," a "non-taster" into a "taster," or, most difficult of all, a "non-thinker" into a "thinker." We

tacitly assume that such miracles can be accomplished; we spend large sums trying to bring them about, and are amazed when the leopard's spots stay very much the same, in spite of our earnest efforts to alter the design to one that we believe to be more "suitable"!

EUGENICS CONGRESS PROGRAM

THE Third International Congress of Eugenics, to be held August 21-23, at The American Museum of Natural History, New York City, has issued the following preliminary program:

NOTE: *Papers preceded by asterisk * will probably be read by title.*

Sunday, August 21, 1932

Excursion to Cold Spring Harbor. Leave from Pennsylvania Station at 9:11 A. M., D.S.T.

Monday, August 22, 1932

8:30 to 10:30 A. M.

Registration.

GENERAL SESSION

10:45 to 11:30 A. M.

IMMIGRATION CONTROL.

Main paper by *D. F. Ramos*, Habana.
Discussion by *H. H. Laughlin.*
Discussion from the floor.

MEASURES TO ENCOURAGE THE FERTILITY OF THE GIFTED.

Leading paper by *Dr. J. Sanders*, Rotterdam.
**Dr. George Schreiber, Dr. Renato Kehl.*
Franco Savorgnan. "Married Fecundity in the Aristocracy."
Discussion from the floor.

11:30 A. M. to 12:15 P. M.

REDUCTION IN FECUNDITY OF THE SOCIALLY INADEQUATE, INCLUDING STERILIZATION AND CONTRACEPTIVE ADVICE.

Main paper by *Sir Bernard Mallet*, London. Leading discussion by *E. S. Gosney* and *Dr. T. R. Robie* on "Selective Sterilization for Race Culture." *J. Sanders* will also discuss. Discussion from the floor.

12:30 to 1:30 P. M.

Luncheon.

1:15 P. M.

Business meeting of the International Federation of Eugenic Organizations, at the Museum.

SECTIONS

I. RACE DIFFERENCES IN RELATION TO EUGENICS.

Taeuber, Mrs. Urene Barnes. Assortative Mating for Color in American Negro.
Holmes, S. J. The Effect of Migration on the Natural Increase of the Negro.
Plecker, W. A. Virginia's Effort to Preserve Racial Integrity.
Rosinski, Boleslaw. The American People of Polish Origin in Texas.
Wallis, Ruth Sawtelle. Harmonic Types among Western European Crania.
**Boldrini, Marcello.* The Unification of the Anthropological Type of Italians and its Eugenic Effects.
**Govaerts, A.* Characteristics of the Belgian Population.
**DuPont, A. F.* Racial and Ethnological Difference between Northern and Southern French.
**Cipriani, Iddio.* The Mental Possibilities of Negroes.
**Pessler, Wilhelm.* Volkstumsverbreitung und ihre Ursachen.

II. MEASUREMENT OF HUMAN TRAITS.

Stanton, Hazel M. Stability of the Seashore Measures of Musical Talent, as shown by Retests.
Frassetto, Fabio. Project for a committee on Standardization, Fertility and Constitution.
Gates, R. R. Plan for Obtaining an International Standard Technique in Physical Anthropology.
Post, R. H. Standardizing Measurements of the Living.
Davenport, C. B. On the Need of Checking in Anthropometry.
Downey. June. Handwriting of Intraverts and Extraverts. (Lantern).
**Bunak, V.* On the principles of the Anthroposcopical methodology.

III. VARIATIONS, BIRTH RATE AND FECUNDITY, AND MATE SELECTION.

Graves, William W. A Note on Inherited Variations and Fitness Problems: I. The Types of Scapulae.
Danforth, C. H. Family Size as a Factor in Human Selection.

Cobb, Margaret V. Evidence of the Rapidly Decreasing Birth Rate in Families in which Highly Intelligent Children Occur.

McGee, Mrs. Anita N. Merging of Ancestral Lines.

Crampton, C. W. Aristogenics.

Hodson, Mrs. C. B. S. The Results of Contra-Selection in England: A survey of available statistics.

Grove, Chas. C. The Inadequacy of Census Data for Certain Eugenic Investigations.

**Freudenberg, Karl.* die neusten Wandlungen der Fruchtbarkeit in Berlin nach sozialen Verhaetnisse.

6:30 P. M.

Dinner will be served at the American Museum of Natural History.

GENERAL ASSEMBLY

8:00 P. M.

Introductory remarks by the President of the International Federation of Eugenic Organizations.

Message of *Leonard Darwin,* to be read by *R. A. Fisher,* Rothamsted Experimental Station, England.

Address by *Henry Fairfield Osborn,* "Birth Selection vs. Birth Control."

John Alfred Mjoen, Oslo: "Leading Principles and Guiding Lines for a new Population Politic—a First Step.

9:30 P. M.

Reception at the American Museum of Natural History.

Tuesday, August 23, 1932

GENERAL SESSION

9:30 to 10:30 A. M.

Eugenics and Education; also Education in Eugenics.

Leading paper by *G. P. Frets,* Rotterdam. Leading discussion by *C. C. Little.* Discussion by *Dr. Madge T. Macklin,* "The Need of a Course in Genetics in the Medical Curriculum"; *O. W. Caldwell:* "Some Aspects of Instruction in Eugenics"; *W. Carson Ryan:* "Special Innate Capacities in the Indians." Discussion from the floor.

10:30 to 11:30 A. M.

Marriage Advice.

Main paper by *Paul Popenoe:* "Marriage Counselling."

**Fetscher, Prof. Dr. R.* Eheberatung in Deutschland.

Formal Discussion by *J. A. Mjoen.* Discussion from the floor.

11.30 A. M. to 12:30 P. M.

Eugenics and War.

Main paper by *C. Gini,* Rome. Discussion introduced by *Harrison Hunt:* "Is War Dysgenic?" Discussion from the floor.

12:30 to 1:30 P. M.

Luncheon.

SECTIONS

1:30 to 4:30 P. M.

IV. The Socially Inadequate in Relation to Eugenics.

Kemp, Tage. A Study of the Causes of Prostitution, especially concerning Hereditary Factors.

Myerson, Abraham. The Heredity of Various Groups of the Feeble-Minded.

Powdermaker, Florence. Considerations on the Social Factors in Mental Development.

Hackbusch, Florentine. Pennsylvania's Problem in Eradicating Foci of Mental Defect.

Landman, J. H. Eugenic, Cacogenic and Socially Inadequate Tendencies in our Population.

**Ferreri, Prof. Guilo.* Il Sordomutismo nel Campo Eugenetico e Sociale.

Pollock, Horatio M., Benjamin Malzberg and *R. G. Fuller:* Is Heredity a Causative Factor in the Manic-Depressive Psychoses?

**Antonini, Giuseppe.* La Criminalita in Rapporto All' Eugenics Sociale.

De Porte, J. V. Murder in the State of New York.

V. Physiology of Reproduction.

Rowe, Allen W. and associates: Symposium on Human Infertility (Lantern).

1. Medical Aspects by *C. H. Lawrence.*
2. Gynecological Aspects by *Dr. S. R. Meaker.*
3. Endocrine Aspects by *Dr. A. W. Rowe.*
4. Urological Aspects by *Dr. S. N. Vose.*

Guyer, M. F. Ovarian and Pituitary Modifications resulting from Serum-Induced Sterility.

**Bluhm, Dr. Agnes.* Uber Beeinflussung des Zahlenverhaltnisses der Geschlechter durch Behandlung des Weibchens.

VI. Sanitation and Disease in Relation to Eugenics.

Campbell, C. G. The Physical Factors in Race Survival.

Herman, Charles. Selective Elimination as a factor in Increasing the Immunity of Population.

Sterling, E. Blanche. Child Hygiene in Human Ecology.

VII. Geography and History in Relation to Eugenics.

Lorimer, F. Types of Data Available for Eugenic Research in the United States.

Perkins, H. F. Contributory Factors in Eugenics in a Rural State.

Key, Wilhelmine E. Race and Family in the History of American Institutions.

Wheeler, L. R. Heredity and Environment—their Relative Rôles in the Development of East Tennessee Mountain Children.

VIII. Genetics and General.

Richards, M. H. and R. M. Balyeat. The Inheritance of Allergy with Special Reference to Migraine.

Hutt, F. B. Sex Differences in the Expression of Autosomal Genes affecting Human Dentition.

Hurst, C. C. Genetics of the Human Mind.

Frets, G. P. Heredity in Psychoses.

Johnson, Roswell H. Inheritance of Mental Test Abilities.

Binder, R. M. The Soong Family—an Example of Great Ability in the Common Man.

Gini, Corrado. Remarks on the Explanation of Heterosis.

Artom, Cesare. Allo e Antropoliploidismo negli studi di Genetica.

Steggerda, M. Some results of a Family History Study.

Muller, Dr. H. J. The Dominance of Economics over Eugenics.

Cook, R. C. Is Eugenics Half-Baked?

Ehrenfried, Dr. Albert. Hereditary Deforming Chondrodysplasia (Lantern slides).

In addition to the formal program, the Exhibit of the Congress will be open during the month of September, at the American Museum of National History.

Membership dues in the Congress are $5.00. Application should be made to Dr. H. H. Laughlin, Cold Spring Harbor, Long Island, New York.

Studies in Human Fertility

STUDIES ZUR FERTILITAT, *by* G. L. Moench. Pp. 4 + 143. Ferdinand Enke Verlag, Stuttgart. 1931.

An interesting discussion is given of human sterility based on clinical and laboratory observations obtained from 141 marriages. These include 37 cases of normal fertility, 63 definitely sterile cases and 41 which exhibited various grades of fertility.

The first part of the report is devoted to discussion of the various grades of fertility, the possible influence of diet upon sterility, physical variations among women, and certain obscure causes of sterility.

The second part of the report is concerned largely with microscopic studies upon the seminal fluids and extensive microscopic studies of the spermatozoa. Extensive measurements were obtained upon the spermatozoa in all but 15 of the 141 cases. The data are presented in various tabulations and graphs throughout the text, and a frequency tabulation for the measurements of each individual case is conveniently placed in the appendix. There is an interesting double page figure showing 50 different morphological forms of spermatozoa, and a full page figure illustrating the microdissection of the spermatozoön. The report ends with a bibliography of 216 numbers and a brief clinical report on each of the 141 cases considered.

This publication will be of greatest interest to those who are interested in the physical variations of spermatozoa and the possible relation of these to fertility. The listing of the frequency distributions for the measurements of each individual case, together with the brief clinical description, makes it possible for the reader to pick out individual cases and make such comparisons as he desires.

H. C. McPhee.
U. S. Department of Agriculture.

INHERITED (?) DWARFISM IN THE FOWL

A Preliminary Report of An Apparently Hereditary Condition of Dwarfism in the Rhode Island Red Fowl, Suggesting Cretinism

Roy L. Mayhew and Charles W. Upp
Louisiana Agricultural Experiment Station

THE chickens which developed the conditions described in this report appeared among those reared by the senior author while he was carrying on a study of coccidiosis at Louisiana State University. The eggs from which all these chickens were hatched were obtained from his flock of single comb Rhode Island Red hens, incubator hatched, and all lots reared under as near the same conditions of temperature, feed, etc., as possible, except the one in which numbers 488 and 495 appeared. For a further description of the details of management the reader is referred to Mayhew[12].

Chicks which posses these unusual characteristics do not show them, at least to any marked degree, when hatched, but by the age of about two weeks it is usually possible to separate them from their normal sibs. Generally the first point to be noted is the relative shortness of the legs, the outer toes usually begin to turn more or less outward and backward and the body is carried in a nearly horizontal position. The peculiarities of the head and beak do not develop to a marked degree until sometime later, although they may be detected at three to four weeks. The head as well as other characteristics vary much in the degree of their development in different individuals.

Most of these chickens have not lived to be very old, and many of them did not seem to have much resistance to the common diseases. Some have died with colds and bronchitis, some became paralyzed, while one died suddenly without any apparent symptoms of disease. Many of them have been isolated shortly after detection and have been given special care. No. 23, it will be noted, survived the longest and was 83 weeks old when she died, after having been isolated at about 5 or 6 weeks of age, and kept on a wire floor and under screen all her life. Thus on account of its effect on viability the character might be considered a semi-lethal.

Discussion of Literature

Numerous inherent dwarf forms of various types are known in the plant kingdom. Relatively few cases of dwarfism have been reported as occurring among the higher animals and the inheritance of some of these is not known.

Sollas[16, 17] described a dwarf form which appeared in a strain of guinea pigs. This form was remarkable in the shortness of the body and limbs and the unusual form of the head. This dwarfing proved to be inherited as a simple recessive. A few dwarf guinea pigs that lived were sterile and were about one-half normal size when mature. Snell[15] discovered a simple recessive type of dwarfism in mice in which the proportions of the animal as a whole were those of a normal adult. These animals were somewhat subnormal in vigor and were entirely sterile. Mature dwarfs were only about one-fourth the weight of their normal brothers and sisters. Smith and MacDowell[14] have proven this type of dwarfism to be due to a deficiency of the anterior lobe of the pituitary. East[7] mentions three main types of human drawfs. Cretinism, the type in which the thyroid does not function, is inherited as a simple recessive. This type of human dwarfism seems to resemble the dwarfism in fowls which is discussed in this paper. Craft and Orr[3] described a certain

Hereford calf in which the thyroids, parathyroids, and pituitary body were markedly deficient. The inheritance of the condition was not given. The inheritance of short-leggedness has been reported in fowls by Dunn and Landauer[6, 10], and by Landauer[9]; in sheep and dogs by Plattner[13] and Wreidt[18]; and in cattle by Crew[4, 5], Lush[11], Wreidt[18] and others. Aside from the "bulldog" calves[4, 5, 18] these types appear normal in respects other than extremely short legs. They are thus distinctly different from the dwarfism under consideration in this paper.

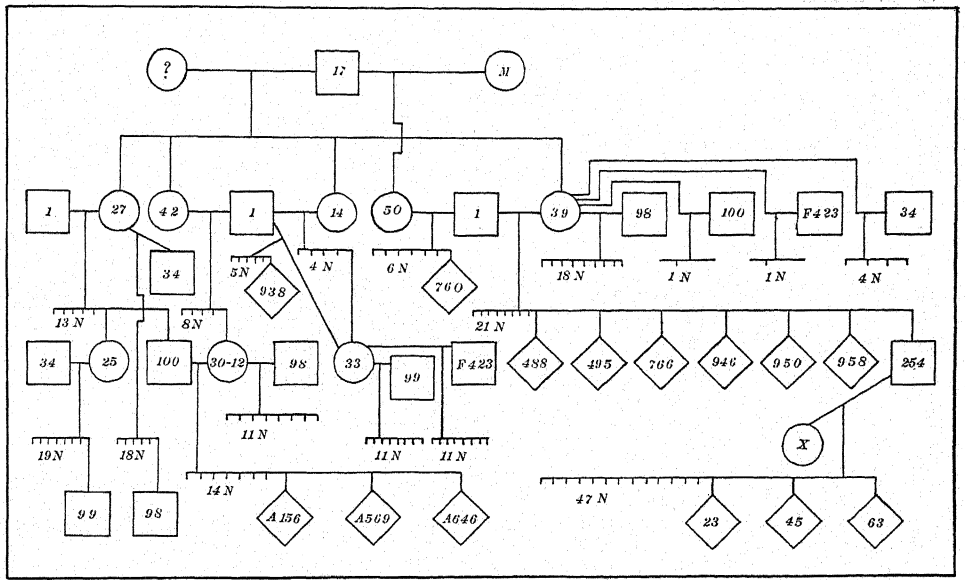
Two miniature individuals have occurred in the Buff Plymouth Rock flock of the Poultry Department, Iowa Agricultural Experiment Station (Waters[20]). These birds appear to be normally proportioned, but are smaller than the average Buff Rock. Information as to whether or not this is an inherited condition is not available. Byerly[1] in work with the recessive lethal character "stickiness," noted the appearance of five chicks which had all of the appendicular bones much shortened. It is not stated as to whether or not these individuals were retarded in growth. This author[2] states, "The relationship, if any, between stickiness and thyrogenous dwarfism remains to be established." Landauer[8] has described a condition of thyrogenous dwarfism (*Myxoedema infantilis*) occurring in Rhode Island Red fowls. It has also been observed by Warren[19] in the Rhode Island Red flock of the Kansas Agricultural Experiment Station. Landauer presented no data as to the inheritance of this form of dwarfism. From such observations as we have been able to make to date, the condition reported in this paper appears to be the same as that reported by Landauer and Warren.

General Description

In general appearance these chickens resemble bantams somewhat, but on closer observation several differences are noted. The legs are much shorter in proportion to the body size as compared to the normal fowl, the outer toes turn more or less outward and backward, the beak is somewhat parrot-like, the upper being bent downward while the lower is nearly straight, the head is much wider in the region of the eyes than is the normal, and the eyes have a more or less puffy ring surrounding them. Measurements indicate that the depth of the body is less than in the normal and the base of the tail is carried about on a level with the middle of the body while in the normal chicken it is lower. The tail feathers also are directed backward and downward while in the normal they are more upright in position. There is considerable variation in the degree with which these various characteristics develop in different individuals and in the rate at which they develop. They are cheerful and sociable little chickens, and appear to be normal in activity in a general way. It has been possible to rear but one, No. 23, a female, to the age of sexual maturity. This one, while she had much the appearance of a female, yet never laid an egg nor showed any other characteristics of either male or female. The sex of but five have been determined by postmortems. Four of these were females, and the other was a male.

Pedigree Records

Eleven of the eighteen individuals showing these characteristics have been pedigree hatched and the dam and sire both known. An examination of the pedigree records shows that these have been hatched from four of the 44 hens that have constituted the flock during the period of the observation. Three of the seven male birds used in the various matings during this time have been the sires of dwarfs. The relationships of these various males and females are shown in Figure 5. An examination of this diagram shows that the first male concerned in the production of dwarfs is No. 17 which was obtained, along with a hen, designated *M* in the chart



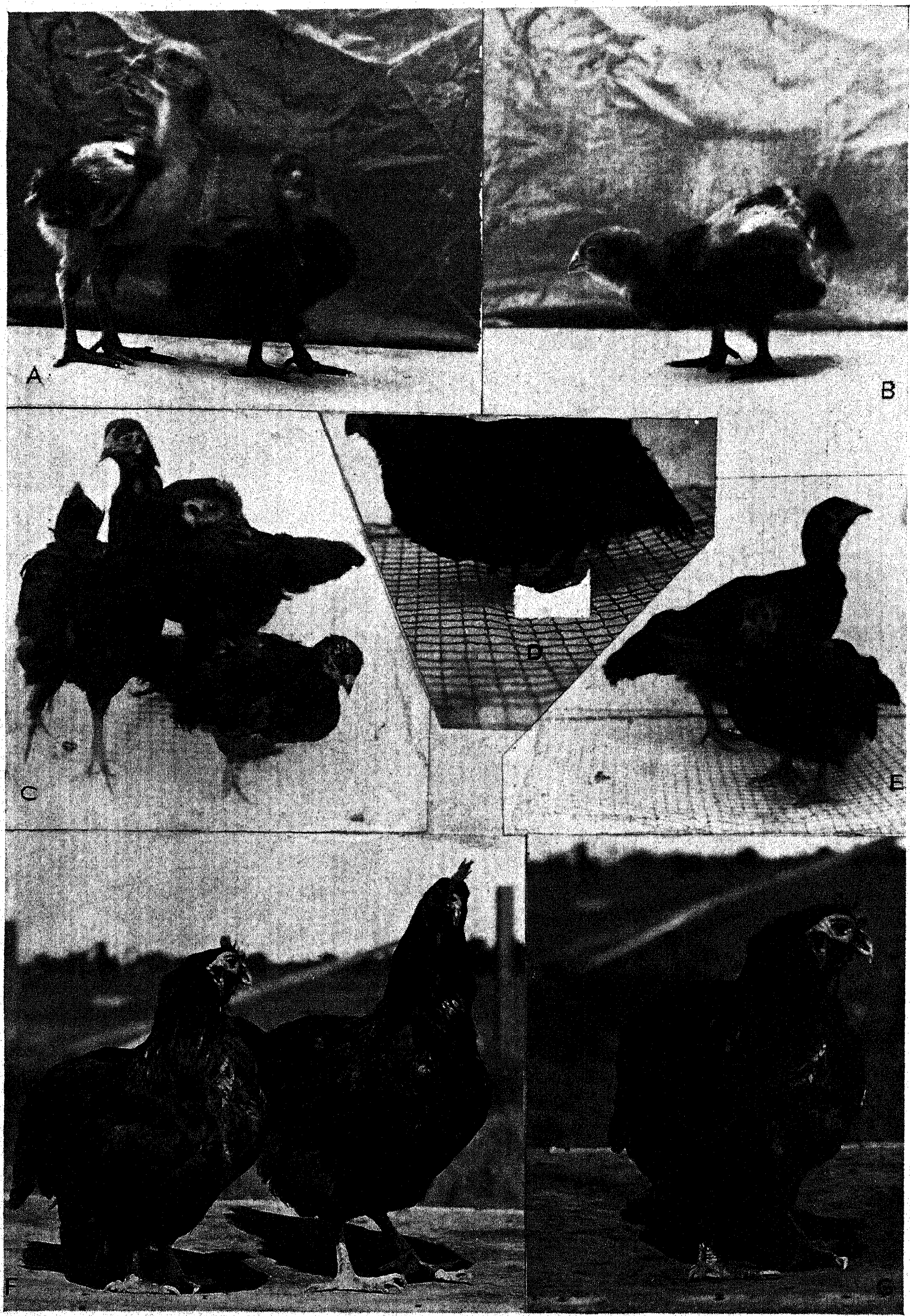
RELATIONSHIP OF DWARFS

Figure 5

Pedigree chart showing the relationships of the dwarfs and normal chickens and their parents. Explanation of symbols. O=female; □=male; Diamond = Dwarf; *X*=pen of 13 hens in Louisiana Egg Laying Contest including 30-12 who has since produced three dwarfs as indicated in the left portion of the chart.

(which became the mother of No. 50), from a breeder in Massachusetts in the fall of 1927. This male became the sire of hens No. 39, 50, 42, and 14. The mother of No. 33 was No. 14, and it may be observed that when No. 33 is mated to her sire she produced one dwarf, but none when mated with male No. 99 and No. F-423, although 11 chickens were reared from each. In the fall of 1928 a cockerel, No. 1, was obtained from a breeder near Baton Rouge, who had obtained the dam and sire from a breeder in Mississippi. It will be observed that in the matings of this male with No. 39, six dwarfs were obtained, and with No. 50 one dwarf was obtained. In the fall of 1930 a second cockerel, No. 34, was obtained from the same Massachusetts breeder from whom No. 17 was obtained, and another, No. F-423 from Texas A. & M. College. Two separate pens were maintained and no dwarfs appeared in either from any hens of the flock while mated with these males. It will be noted that No. 39 was mated to one of these males during the first part of the season and to the other during the latter part. Although the numbers that were hatched and that lived beyond the third week from No. 39 are small in each case as noted, no dwarfs appeared. No. 33 produced 11 chickens which lived beyond the third week while mated with No. F-423 and none of them developed dwarf characteristics.

During the same season the senior author's pen of 13 hens in the Louisiana Egg Laying Contest, designated *X* in Figure 5, were mated to a cockerel sired by No. 1 and whose dam was No. 39. In one unpedigreed lot from this mating three dwarfs appeared. In this pen was No. 30-12, a daughter of No. 42 whose sire was No. 17, and she was herself sired by No. 1. This hen has since produced three dwarfs while mated with male No. 100 whose sire was also No. 1.



DWARF AND RELATED NORMAL CHICKENS

Figure 6

A—No. A569, dwarf, right, and No. A567, normal, age 4 weeks. *B*—another view of A569. *C*—dwarf lower right, normal male lower left, and normal female in the background, all the same age. *D*—dwarf with very abnormal feet, the background has been cut away to show how the toes curve backwards. *F*—Dwarf No. 23 left, age 14 months, weight 1,260 grams. The normal hen, right, is not quite erect, but is the same age, has the same sire, and weighs 2,300 grams. *G*—Dwarf No. 23 in a somewhat different position, and showing the very abnormal head and nearly normal feet.

This is especially interesting since 30-12 was mated with No. 100 in the early spring, producing dwarf No. A-156. No. 100 was then replaced with No. 98, who died after 11 normal chickens were hatched from 30-12. No. 100 was then returned to the pen with the result that two more dwarfs were produced. No. 98 is evidently not a carrier since he produced only normal chickens, 18 in number, while mated with No. 39 as shown in the upper right hand portion of Figure 5.

Two other males, Nos. 98 and 99, whose pedigrees are shown in the diagram, have been used in the season of 1930-31, and no dwarfs have appeared although all three of the dwarf-producing females have been mated with them as indicated, with a goodly number of chickens produced in each instance.

Hatching Records

An examination of the hatching records of all hens in the flock during the period of the observation yields certain data which may be summarized as follows:

There have been a total of 44 hens from which chickens have been hatched between February, 1929, and July, 1931, or covering three hatching seasons.

A total of 1,094 chicks have been hatched from these hens.

A total of 229 chicks have been hatched from all hens mated with dwarf-producing males.

The available data show that 55 chicks died in the shell, of the eggs set from hens mated with dwarf-producing males while from hens mated with other males 244 died in the shell. Data on this point are incomplete because fertility records were not kept on all lots. Also records as to the total number of eggs set were not kept in all lots.

The principal data with reference to the dwarf-producing females is given in Table I.*

The total number of normal chickens hatched and living beyond the third week by these females mated with dwarf-producing males is thus found to be 46, while 11 pedigreed dwarfs have appeared.

The total number of deaths from one to three weeks of age is nine when these hens were mated with dwarf-producing males and 11 from non-dwarf-producing males.

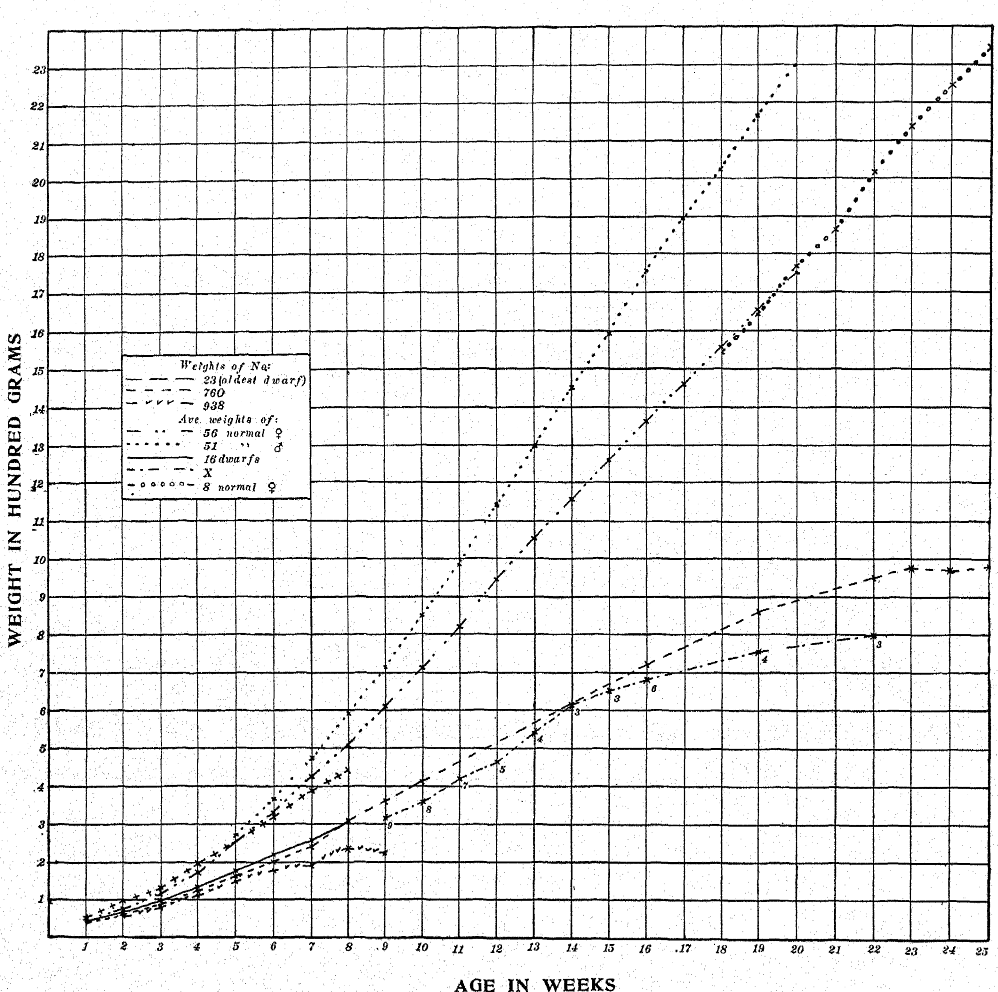
Egg Production and Egg Weight Records

An examination of the egg production records of the dwarf-producing hens shows that No. 39 laid 254 and No. 33 laid 204 eggs in their pullet years and No. 50 laid 139 in the first nine months of her pullet year. No. 30-12 has a record of 209 eggs in her pullet year. The mothers of the dwarf-producing males have pullet year records as follows: No. 1, 186 eggs in eight months; No. 17, 206 eggs, and No. 100, 268 eggs. The approximate weights of the eggs laid by the dwarf-producing females is as follows: No. 39, 24 oz. per dozen; No. 33, 22 oz. per dozen; and 30-12, 24 oz. per dozen. Egg weights on No. 50 are not available since none were kept during the first season of these observations. Thus it is seen

*** TABLE I. Progeny Records of Dwarf-Producing Females Mated to Normal-Producing and to Dwarf-Producing Males.**

	Mated to	Total Hatched	No. Living Beyond 3rd Week	No. Normal	No. Dwarf	Deaths 1-3 Weeks
33	D	6	6	5	1	0
	N	28	22	22	0	6
39	D	33	27	21	6	6
	N	30	25	25	0	5
50	D	8	7	6	1	0
	N	5	5	5	0	0
30-12	D	20	11	14	3	3
	N	11	11	11	0	0

D=Dwarf-producing males; N=Normal-producing males.



GROWTH CURVES OF DWARFS AND NORMAL CHICKENS

Figure 7

No. 23 was the oldest dwarf so far obtained. She lived to be 83 weeks old and attained her maximum weight of 1,300 grams at the age of 61 weeks. No. 760 was one of the heavier and No. 938 was one of the lighter weight dwarfs. The curve designated *X* represented the average weights of such dwarfs as were living and weighed at those particular weeks. The number of individuals entering into each average is indicated on the curve. The eight normal females designated by circles and dashes were kept under the same general conditions as the dwarfs, and were from the same lot as No. 23.

that the fact that these hens were apparently carriers of this unusual characteristic does not seem to have influenced in any way their production qualities.

Weights of Dwarfs

The chickens were weighed weekly as a matter of routine in connection with the coccidiosis experiments. Figure 7 shows these data graphically. The observations are recorded in detail in Table II. Weekly weights are available of 16 of the dwarfs (of unknown sex) and their averages are plotted in Figure 7 in comparison with 56 normal females and 51 normal males which were reared under the same conditions. No weights of No. 488 and 495 were kept as they were

among the young chickens reared at the senior author's home. Beyond the eighth week weights were not taken regularly but such as were kept are given and the curve designated *X*. It will be noted that the average of the dwarfs runs lower than that of the normal females. The weights of one of the heaviest and one of the lightest dwarfs are also plotted. It will be noted that the heaviest dwarf is higher than the average of the females during the earlier weeks. The weights of the dwarf, No. 23, which has lived the longest are also plotted, and it will be noted that these are a little below the average of the 16 dwarfs for the first eight weeks.

Summary and Conclusions

Data on the general characteristics, pedigree records, and weights of 18 cases of dwarfism are presented.

The hatching records of the hens of the flock from which the dwarfs were hatched, together with the egg production and egg weight record of these dams are discussed.

The pedigree records seem to indicate that the characteristic is a simple recessive, which appears when two heterozygous individuals are mated.

The ratio of normal to dwarf progeny, where both parents are known definitely to be dwarf-producers, is 46 normals to 11 dwarfs. The expected ratio if the characteristic is a simple mendelian recessive is 42.75 to 14.25.

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TABLE II.—Weights of dwarfs in grams and the average weights of 51 normal males and 52 normal females reared under the same conditions.

	Age in Weeks					
No.	1	4	6	8	12	16
760	51	197	318	440		
766	54	174	294	380		
938	40	112	175	240		
946	44	156	202	290		
950	39	146	246	340	540	720
958	30	120	190	300	500	660
23	41	124	202	313	·	720
45	47	125	206	303		740
63	39	134	238	395		630
A569	50	170	280	350	530	610
A156	42	126	166	200		
A646	42	134	200	......		
A651	39	131	230	270	370	
A731	34	100	188	300	390	
A797	40	90	170	260		
A804	30	94	152	220		
Ave.	41.3	133.3	221	306	466	680
Ave. 51N ♂	46	190.8	366	590	1143	1753
Ave. 52N ♀	43.6	176.1	332	505	943	1363

N—Normal.

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The data herein reported was obtained and compiled by the senior author and the literature reviewed by the junior author. The authors wish to express their appreciation to Dr. W. H. Gates of the Dept. of Zool., Louisiana State University, for certain suggestions in regard to the manuscript.

The United States Becomes An "Immigrant Exporting" Country

Probably for the first time in its history, the United States failed to gain in population through immigration during the fiscal year ending June 30, 1931. During that period 97,139 immigrants and 193,540 non-immigrants were admitted to the country and 61,882 alien emigrants, 229,035 non-alien emigrants left the country. The total admitted 280,179, was 10,000 less than the total leaving, 290,916. The change in the immigration laws accounts for a considerable portion of the reduction in immigration, but economic conditions during the past two years has been a very important factor.

Racially, the groups showing a positive balance of emigration over immigration perhaps tends to increase the proportion of North-European stocks. The largest immigration surplus is of Germans. The largest emigration quota is of Mexicans, with 11,769 less in the U. S. at the end of the year. The racial trend of the pluses and minuses is, however, not very definite, and the most significant fact is that the totals are so nearly balanced that for the first time in its history, perhaps, immigration for the year might be said to have had no effect on the racial constitution of the United States.

Why Native Populations Decline

IL PROBLEMA DEMOGRAFICO NELL'AFRICA EQUATORIALE, by Carlo Valenziani. Comitato Italiano per lo Studio dei Problema della popolazione, Serie II, Vol. I. Pp. 109. Rome, 1929.

A decreasing native population in equatorial Africa is one of the serious problems confronting almost every colony. The author believes the decline was already under way before the period of white domination, but the latter has hastened it by the effect produced on native mentality. In the course of a few years cannibals in the stone age were thrown into the era of the automobile and radio. Details of vital statistics and analysis of the causes of depopulation make this a valuable and interesting contribution to a study of world-wide importance.

PAUL POPENOE.

HEREDITARY ATAXIA

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ONE of the rarer affections of the nervous system is an ataxia first described in 1861 by Nikolaus Friedreich (1825-1882), and hence usually called Friedreich's ataxia[3, 4]. If the basis of it is present in the germ-plasm, symptoms may manifest themselves early in life. The gait becomes uncertain and hesitating, with the legs wide apart. Tremors appear, frequently accompanied by the convulsive jerkings and twitchings that are oftener associated with chorea. Within a few years, the patient is usually confined to a wheel chair, though he may live for a long time. No cure is known.

Typical manifestations may be seen in the family (hitherto unpublished) which one of us (K. B.) studied at an eastern institution some years ago. The relationships are shown in the accompanying pedigree chart.

Though resident in America for a quarter of a century at the time the investigation was made, this family came from the Italio-Swiss frontier, speaking a mixed patois of French-Swiss-Italian origin. This is more significant, as a large part of the hereditary ataxia recorded has been shown to have originated in Switzerland,[6] where forty-six cases in 21 families were traced back to four original families. It is probable that these could have been related to each other, if adequate genealogical data had been available. It may well be, therefore, that the family which we here present was of the same origin. The father of the patients, a hatter by trade, had records of his ancestors for 300 years, and is positive that there had been no appearance of ataxia during that time. Only four generations are shown on our chart, as those were the only ones for which he could give details. Less is known of the mother's family, but as she and her husband came from the same village, it is not a rash surmise that the families were interrelated by marriages at some time in the past. Her father died at the age of 72, her mother at 71 was still living in Italy.

This couple had 10 children, equally divided in sex. The oldest girl (23) had married a year previously. Two boys (21 and 19) were normal.

The first affected child was Emma, who attended school until 8, when ataxia appeared. At 17 she was admitted to an institution, and died two years later. The death certificate gave the cause as gangrene of the lung, with Friedreich's ataxia as a contributory cause. Autopsy led to change of this finding to "double pneumonia and pericarditis."

The next child, Jennie, born 15 months after the preceding, attended school until 9, was admitted to the institution at 15, and died at 17. Autopsy established the cause of death as ascending paralysis, contributing cause Friedreich's ataxia.

A third affected child followed,—the boy Julius, who went to school until 10, taking manual training for the most part. At 15, when seen, he had lost the power of locomotion, and almost all coordinated movements of arms and fingers.

The course of the affection followed similar lines in the three. All seemed to be normal until about 7 years old; they learned to read; then they began to lose the power to walk, and soon deteriorated mentally as well as nervously. Emma at 14 could not even stand up, could not feed herself, and had become almost an imbecile. Jennie at 13 had lost her ability to read, as well as to stand or to hold herself in a chair, and was rapidly growing more stupid. Her legs were covered

with an erythema from the knees down, increasing to the toes which were almost purple. She could not extend the lower legs without pain. Reflexes were all absent, though she had full control of the sphincters. Julius at 11 walked awkwardly, had difficulty in balancing, fell forward or backward easily, did not respond to the usual tests of reflexes, but was still as bright as an ordinary boy.

The next to the youngest child, Dora, at 6 was already showing signs of ataxia. At 8 she staggered around only by placing her hands on some piece of furniture, and was fast becoming helpless both physically and intellectually.

Here, then, are four out of 10 sibs affected with one of the most serious nervous diseases known. While there is no record of a similar occurrence in the ancestry, the picture fits perfectly the workings of a pair of recessive genes, if one makes the reasonable assumption that it was in both sides of the ancestry because of intermarriage in some remote generation. It would be much more unreasonable to suppose that these two old families in a stable peasant community had never intermarried in the past. One could hardly ask for a better illustration of a defect behaving as a simple recessive.

Variable Dominance

The same defect would not be expected to behave in the same way in every other family, and study of published cases shows that it does not[2]. In some instances it is a typical dominant, frequently a dominant with some irregularities of expression. Still others resemble ours in being typical recessives, even more perfectly demonstrated because intermarriage in previous generations is proved.

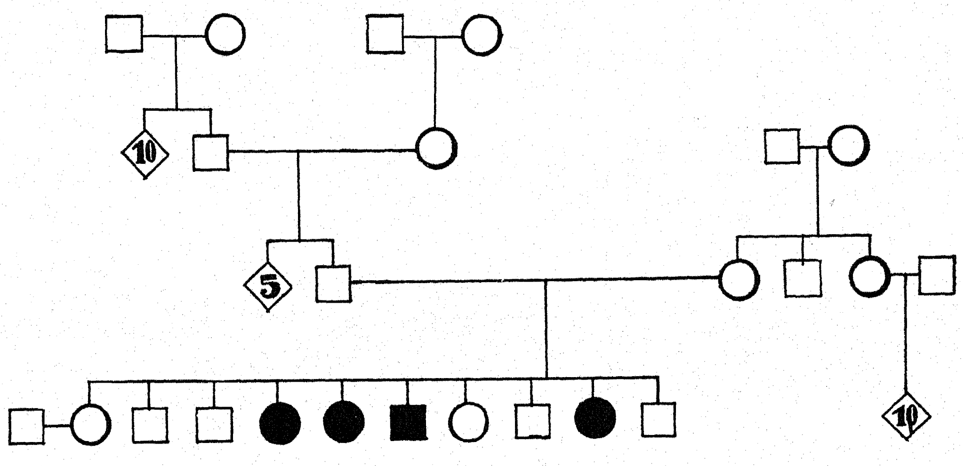
If one seeks more light on the action of this gene by studying the peculiarities of the patient, the difficulty is found to originate in the spinal column, and to result from the degeneration of various parts of this—the dorsal columns, the spinocerebellar tracts, the pyramidal tracts, the dorsal root fibres, and so on. Presumably these parts of the nervous system were made of inferior materials—to speak somewhat figuratively—so that they functioned fairly well for a few years, and then began to deteriorate. A crude mechanical comparison may be derived from an automobile in which some important part, say the gears, has been made of cast iron instead of steel. It will look sound at the outset, but it will not stand up under the wear and tear of daily use, and in a short time the car will be acting badly because of this defective part, although the rest of it may be in good condition.

Other Inherited Abnormalities

The number of cases in which an inherited human abnormality is not manifest at birth, but appears only some years afterward, is large. Conspicuous examples are pattern baldness, progressive deafness (otosclerosis), various eye defects, and nervous affections such as Huntington's Chorea[10]. The picture presented by hereditary ataxia is therefore a familiar one.

While the expression of every inherited trait is subject to wide variations, through the action of modifying genes, this is particularly conspicuous in the case of ataxia,—to the extent that attempts have been made to break it up into two or more distinct diseases. Thus cases involving the spinal cord alone are sometimes called Friedreich's ataxia, and those involving the cerebellum alone are called Charcot-Marie ataxia. But apart from the question whether in these cases the involvement is actually limited to the tract mentioned, there are so many intermediate types, including every possible transition, that it has gradually become clear to clinicians that the disease is one. With this conclusion the geneticist would agree.

Attempts made to distinguish different types of transmission in the different clinical pictures seem to depend merely on inadequate observation.



RECESSIVE FRIEDREICH'S ATAXIA

Figure 8

The chart above would hardly establish that Friedreich's Ataxia is hereditary. Many other pedigrees have, however, been published which have demonstrated uniquevocally the hereditary nature of this disease. In some families the trait behaves as a dominant, in others as a recessive, as in this instance. (In this chart squares represent males and circles females, the diamond-shaped symbols represent normal fraternities of undetermined sex.)

Any type may behave typically as either a dominant or as a recessive, or more frequently as variable. The assertion[7] that the less serious forms are dominant, the more serious recessive, is likewise contradicted by a survey of the whole field.

If the gene responsible for ataxia is recessive to a marked degree, it will produce an effect only when present in duplicate, coming from father and mother alike (hence it is found that there is an unusual amount of consanguineous marriage in family histories of this affection). It is also evident that many persons must be merely carriers; that is, they have a single gene, which does not produce marked results in the lack of its mate.

But since the gene in other cases does behave as a dominant, it would be expected that even a single dose of it would produce some effects. Thus in a California family where the disease is irregularly dominant, the observer[8] reports that many of the relatives of a patient show nervous symptoms (tremors, uncertain gait, nystagmus, absent knee jerks and Achilles' reflexes, and the like). The most reasonable assumption is that these are individuals who have only a single gene, and that the modifiers tend to minify its effects. In cases where the single gene is attended by modifiers that tend to accentuate its effects, it may produce definite symptoms of ataxia; and of course it does so when it is present in duplicate.

"Abortive forms" of ataxia have been recognized clinically[11]. From a genetic point of view, these may be the result of modifiers, or they may represent heterozygotes—that is, persons who have a single gene, not a pair of them. They must be due to the same gene that produces the typical form of the disease, in any case.

The effects of modifying genes are really remarkable in this disease, and have been brought out strikingly in a recent study[1]. No two families are alike, and almost every conceivable part of the body may be affected concurrently in this way: muscles, bones, skin, hair, internal glands, and special senses have all been implicated. The age of onset is also modified characteristically. Thus in the pedigree chart reproduced herewith, the typical age of onset is about 7. In a family of Georgia whites[5], it is pretty regu-

larly 12. In the California family mentioned[8], the patient with whom the study began did not consult a psychiatrist until he had reached 37.

Early Evidence of Defects

Again, there are differences in the premonitory indications of onset. In the family we studied, there seemed to be no evidence of weakness until the actual breakdown began. As little children, Emma, Jennie, Julius, and Dora were as active and healthy as any of their playmates. In the Georgia family, on the contrary, the parents "in practically every case from late infancy through early and late childhood" noted "a certain nervous instability," which varied from "infantile convulsions to headache, or from easy fatigue to excessive nervousness." This again is the typical picture of family differences due to the presence of a different set of modifiers in each family.

Perhaps because it affects such an important part of the organism—the spinal cord and cerebellum—the gene responsible for hereditary ataxia produces more striking and far-reaching results than do many other single defective genes, and is more subject to the influence of modifiers. The spinal cord is one of the earlier parts of the embryo to be laid down. A defective gene in this may continue, through a long series of cell divisions, to exercise an influence on later stages of embryonic development. This has resulted in a picture of heredity which has confused almost every writer on the subject; simply because these writers did not have an accurate idea of normal inheritance, and expected a regularity which they did not, and could not, find.

In particular, they have attached an importance to dominance, which it does not have in fact. Dominance is not a part of the mechanism of inheritance at all. It is merely a developmental phenomenon, hence all the more subject to modification in the course of development[9]. Of this fact hereditary ataxia is a striking illustration.

Prevention

As there is virtually no treatment possible in cases of this disease, the real problem is one of prevention. This depends on the prevention of reproduction in affected families. Appearance of a single affected child might well be considered an indication for sterilizing the parents.

This would have prevented the birth of only one of the four children in our pedigree chart, however, because the affection did not become manifest in the first child until two more affected ones had already been born. Hence earlier possibilities must be considered. In the first place, all persons from families characterized by hereditary ataxia (particularly if there is also cousin marriage in the ancestry) should be warned of the probability of affected offspring. They might well be urged to undergo sterilization before marriage, unless they have compensating traits of social value to pass on, in a much more conspicuous degree than do any of the members of the families here studied.

In the second place, an individual from an affected family should be examined with particular care before marriage, by a competent neurologist. In some instances, it is probable that a thorough examination would reveal symptoms, in an impaired nervous system, which indicate the presence of the gene in question. Such persons should not marry at all, unless to some one who understands the possibilities; and in any event only after sterilization.

Summary

1. A new family history of hereditary ataxia is published, in which the affection behaves as a typical recessive.

2. A study of other published material shows that dominance is particularly variable for these genes, and the action of modifying genes is unusually conspicuous.

3. From a genetic point of view, all

the various clinical types of ataxia may be traced back to the same source, differences in symptoms being due to different modifiers, and not to different gene mutations.

4. Since the disease is incurable, it is a problem of eugenics rather than of neurology; and calls for sterilization of individuals who are likely to transmit it.

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The Increase of Mental Deficiency

THE burden of mental deficiency and defect is borne by the general population in a variety of ways. It takes its toll in accident and in reduced efficiency. It also takes a toll through general taxation—paid for the support of institutions for the defective and the deficient. It takes a heavy toll in happiness and well-being, which it is almost impossible to estimate.

Data on the extent and on the rate of increase in the tax-burden is given in a report recently issued by the Bureau of the Census, on the present status of state and private care of the feeble-minded and epileptic.* The data cover the years 1926 and 1927, and contain comparisons with earlier figures. On account of differences in methods of enumeration and in presentation it is emphasized that exact comparisons are difficult. The outstanding facts are these: In 151 state and private institutions in the United States in 1928 there were 60,419 feeble-minded and epileptic patients. In 1923 there were 42,954; in 1904 there were 14,347. The ratio of patients per 100,000 of total population has risen from 17.5 in 1904 to 53.1 in 1928. What part of this quadrupling in actual numbers is due to an increased incidence, and what part to better facilities and more accurate diagnosis is not clear. Of the total number of inmates in 1923 only 7.3 per cent were in private institutions, 90 per cent were in state institutions, and 2.6 per cent were in other public institutions. The burden of the support of institutionalized mental deficiency is borne by the public.

In 1927, 7,288 new feeble-minded patients were admitted to these institutions, and 1,950 epileptics. The distribution of male and female feeble-minded is about equal (3,824 ♂ to

* Feeble-minded and Epileptic in State Institutions, 1926 and 1927. Admission, discharges and patient population for State Institutions for feeble-minded and epileptic. Pp. 61. 26 tables. Price 15c. Washington, Government Printing Office. 1931.

3,464 ♀) but there is a definite preponderance of male epileptics: 1,140 males to 810 females. In recent years a more rapid increase in male epileptics has been noted.

In 68 state institutions in 1927 the staff personnel totalled 9,019 persons. Of this number 244 were physicians, 30 were psychologists, 110 were graduate nurses, 4,155 were attendants. The total cost of operating and maintaining these institutions was $22,425,135, the cost per inmate being $342.71. This compares with similar costs in prisons of $334.75, and in hospitals for mental disease of $307.81, per inmate per year.

"It is well known that only a small part of the feebleminded in the United States are in institutions established for their care," so that this report gives only an inkling of the total economic burden of feeblemindedness. The three-hundred-per-cent increase in ratio of institutional feeblemindedness to the total population is probably the most alarming statistic presented. The tax cost of the care of these unfortunates is at present only about a dollar per family for the population as a whole. If this amount more than triples every quarter century, as has been the case since 1904, it will rapidly become a very tidy item of taxation in the not-so-distant future. Probably the acceleration in tax-costs for this purpose is not as great as for more vocal "causes." Nevertheless it is rapid enough to be viewed with alarm by many who are today wondering where next year's taxes are coming from. If the rate of increase since 1900 is maintained, it will represent three dollars per family by 1953; $9.00 per family by the turn of the century. This assumes no acceleration of the rate of increase, which has been considerable in the past, and which if maintained might easily double or triple this already rather staggering figure.

Even though we fully agree with some of the rabid anti-eugenicists, abetted by a refinement of statistics, that feeblemindedness could probably not be altogether eliminated in a score of generations, it is nevertheless a fact that its *rate of increase* can be checked, not in a decade, but if we wish it sincerely enough, and are willing to apply existing knowledge, next year. Its incidence in the general population could be very materially reduced in 25 years, and with little inconvenience even to those concerned. Rapidly increasing feeblemindedness is a luxury that we appear willing to afford because we prefer to consider the problem emotionally rather than objectively. Perhaps the feebleminded are happier than the normal and if this is so their multiplication might be considered an advanced form of altruism. Those who have seen them "in action," trying to compete in a too complicated world, or in the rather dreary restraint of the average institution, can only feel amazement that such a view can seriously be advanced. At the present time only one state, California, is making any active and intelligent attempt to meet the problem. The rest of us are content to pay a dollar a year now and to look forward to paying three times as much in 1960 and nine or eighteen times as much by the turn of the century. One wonders, was Linnaeus really serious when he chose *Sapiens* as a designation for the fellow members of the species to which he belonged?

Economics of Mental Disease

THE term "mental disease" covers a great variety of conditions due to a large number of causes. Genes and germs and many other things have a part in initiating the events that cause people to be placed in institutions for the insane. Dr. Horatio N. Pollack, of the New York Department of Mental Hygiene* has attempted to place a value on the economic losses due to mental disease in New York State and

*POLLACK, HORATIO N. Economic Loss to New York State and to the United States because of Mental Disease. 1931. *Mental Hygiene* 16:(62)289-299.

has extended the computation to cover the entire United States.

The costs of mental disease are of two kinds: the cost of maintaining the institutions for the care of such sufferers, and the loss due to disability and death of the patients. In New York State the capital costs and costs of administration in 1931 were $44,914,504. Based on Dublin and Loka's research on the *Value of a Man*, Dr. Pollack concludes that the loss of productive capacity in 1931 amounted to $84,425,269, making the total cost of mental disease to people of the state of New York of $129,338,773.

For the United States as a whole, basing his computations on Federal statistics, Dr. Pollack places the figures for last year at $742,145,956, or roughly, three-quarters of a billion dollars.

Dr. Pollack classifies the various kinds of dementia causing this loss. The least important of these, numerically, is Huntington's Chorea, causing a loss of about a hundred thousand dollars in New York State. The most important is dementia praecox, causing about a third of the total loss. There is no question about the heritability of Huntington's Chorea. Identical twins with dementia praecox, and other evidence, suggest that this form of insanity also is largely determined by heredity. In these two items alone human genetics offers a possible economic saving of a quarter of a billion dollars a year, leaving out of account the less tangible but more tragic destruction of human happiness. R. C.

Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

FOUNDATIONS OF ABNORMAL PSYCHOLOGY, *by* FRED A. MOSS, PH.D., M. D., Professor of Psychology, George Washington University, and THELMA HUNT, PH.D., Assistant Professor of Psychology, George Washington University. Pp. 548. Price, $4.50. Prentice-Hall, Inc., New York. 1932.

Abnormal heredity, too, in spots.

KLINISCHE UND VERERBUNGS-MEDIZINISCHE UNTERSUCHUNGEN UBER OLIGOPHRENIE IN EINER NORDSCHWEDISCHEN BAUERNPOPULATION, *by* TORSTEN SJORGEN. Pp. 121. Levin & Munksgaard, Copenhagen, 1932.

Genetics of feeblemindedness in a rather circumscribed peasant community, with unusually complete family records.

ANTHROPOLOGY AND MODERN LIFE, *by* FRANZ BOAS, PH.D., Professor of Anthrpology, Columbia University, and President of the American Association for the Advancement of Science. New and Revised Edition. Pp. 255. Price, $3.00. W. W. Norton & Company, New York. 1932.

Dr. Boas is one of those anthropologists who does not believe in Eugenics.

ZOOLOGY, A Textbook for College and University Students. *By* F. E. CHIDESTER, A. M., PH.D., Professor of Zoology, West Virginia University. Pp. 581. Price, $3.75. D. Van Nostrand Co., Inc., New York. 1932.

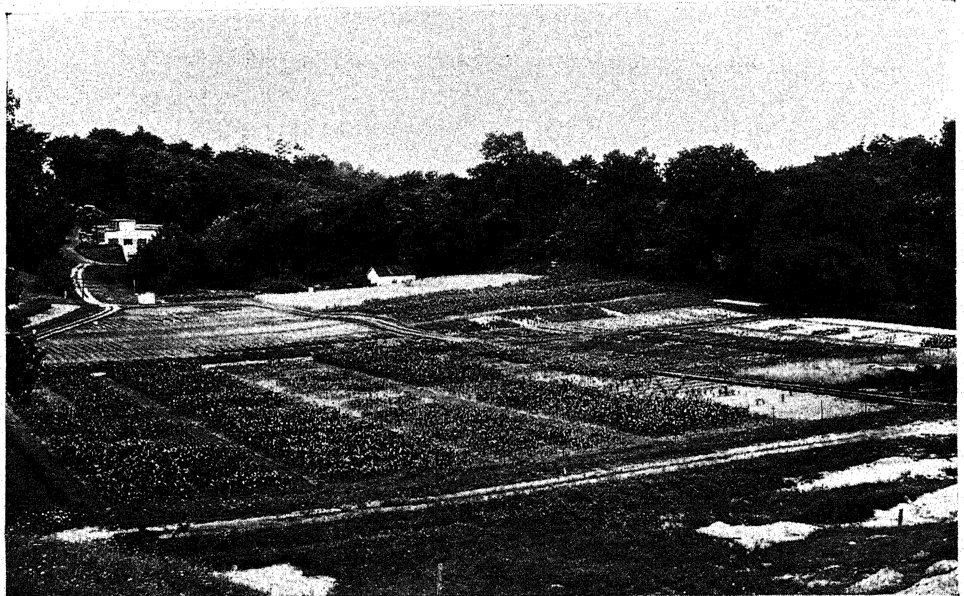
Our knowledge of the world that lives and moves has grown so great that 550 pages give space to cover it only rather breathlessly.

SEX TALKS TO BOYS (10 Years and Older). *By* IRVING DAVID STEINHARDT, M. D. Pp. 187. 12 Illustrations. Price, $1.75. SEX TALKS TO GIRLS (14 Years and Older). Pp. 193. Price, $1.75. 6 Illustrations. Lippincott, Philadelphia. 1914.

"Male and Female he created them," and the more civilized we get the more of a nuisance it seems to be for purposes of enlightenment. What does an aphid do, part of whose offspring are parthenogenetic and part not?

THE GENETICS CONGRESS

Arrangement of Program Nearing Completion—Live Plant Exhibit in Excellent Condition—Accommodations and Camp Sites at Ithaca



THE LIVE PLANT EXHIBIT
Figure 8

One of the outstanding features of the forthcoming International Genetics Congress will be the exhibit of growing plants, showing the forms used in Genetic research. It is probably not an exaggeration to say that no such varied and extensive a range of genetic types has ever been assembled in any one place.

RECENT reports from Ithaca indicate that the preparations for the Sixth International Genetics Congress are rapidly nearing completion. A Local Committee, headed by Dr. R. A. Emerson, has gone far toward making final arrangements for the entertainment of the Congress at Ithaca. A most interesting scientific gathering, truly international in character, is assured, and a varied social program will make the week one long to be remembered.

Program

The Congress will open Wednesday, August 24, with registration of members in Willard Straight Hall in the morning and an inspection of exhibits in the afternoon. That evening will be held the opening plenary meeting of the Congress, after which will be held an informal reception.

Five general invitation programs will be held on the mornings of Thursday, Friday, Saturday, Monday, and Tuesday. Speakers expected for these programs are: F. A. E. Crew, Institute of Animal Genetics, Edinburgh; C. B. Davenport, Carnegie Institute of Washington, Cold Spring Harbor; R. A. Emerson, Cornell University, Ithaca; H. J. Muller, University of Texas, Austin; O. L. Mohr, Anatomical Institute, The University, Oslo; N. Timofeeff-Ressovsky, Kaiser Wilhelm Institute fur Hirnforschung, Berlin-Buch; L. J. Stadler, University of Missouri, Columbia; M. Nawaschin, Timiriazeff Institute, Moscow; Karl Sax, Arnold Arboretum, Harvard University, Forest

Hills; C. Stern, Kaiser Wilhelm Institute fur Biologie, Berlin-Dahlem; O. Winge, Royal Veterinary and Agricultural College, Copenhagen; A. F. Blakeslee, Carnegie Institution of Washington, Cold Spring Harbor; Harry Federley, The University, Helsingfors; G. D. Karpetchenko, Botanical Institute, Leningrad; R. Goldschmidt, Kaiser Wilhelm-Institut für Biologie, Berlin-Dahlem; N. I. Vaviloff, Institute for Applied Botany, Leningrad; R. A. Fisher, Rothamsted Experimental Station, Harpenden; J. B. S. Haldane, John Innes Horticultural Institution, Merton; and S. Wright, University of Chicago, Chicago.

Six independent sessions covering in greater technical detail six different fields of genetic research will be held simultaneously on the afternoons of Saturday, Monday, and Tuesday. Demonstrations of the exhibits (see below) will be held on every other afternoon.

Thursday evening the address of welcome and response will be given, after which Dr. T. H. Morgan will give the Presidential Address, on the subject "The Rise of Genetics."

Friday evening, August 26, a picnic is planned for all members at Taughannock Falls. This is an unusually beautiful and interesting spot and arrangements are being made for camp fires, music, swimming, and other enjoyable features.

One of the most attractive features of the week's program is the excursion, on Sunday, to Niagara Falls. Even though one has seen the Falls before they are well worth many visits, and most advantageous arrangements have been completed with the railway company for this trip. Those who wish to take the trip in its entirety will leave around 7:30 Sunday morning, have lunch at an inn near the station on arrival at the Falls, take the Gorge Trip for two hours, have several hours to wander about at will, across the bridge, on Goat Island and other places of interest, have dinner at the inn and start back to Ithaca at seven in the evening. The entire expense for this trip, meals, Gorge trip and all, will be $5.50 and can be paid at one time. Those who prefer to arrange for their own meals and side trips can pay $3.00 for just the railway fare. In addition to seeing the Falls and the beautiful Gorge, this trip will give the members an unusual opportunity for informal visits and the small group discussions that form such an important part of the benefits to be derived from meetings of this nature.

Exhibits

The interest shown in the exhibits by members of the Congress is such that it is now almost certain that the exhibits will be one of the outstanding features of the Congress. More than 350 geneticists are taking part in the exhibit program. Exhibit material is promised from twenty-three countries located on six continents.

The garden of the Department of Plant Breeding, where the living plant exhibits are located, is already attracting the attention of visitors, as several exhibits are already beginning to bloom. Due to the efforts put into the garden exhibits, and the favorable weather conditions, the exhibit material is in excellent condition. The Oenothera exhibit, which is probably the largest collection of material of this interesting genus ever collected in any one place, is especially thriving.

The material for the indoor exhibits is beginning to arrive. These exhibits will be shown in thirty-nine laboratory rooms located in five adjacent buildings.

Meetings at the New York Agricultural Experiment Station at Geneva

The last day of the Congress, August 31, the activities of the Congress will be transferred to the New York Agricultural Experiment Station at Geneva. In the forenoon, papers will be read relating to fruit and vegetable breeding. During the whole day, fruit genetics and breeding exhibits will be open for inspection and they will be demonstrat-

ed in the afternoon. Members of the Congress will have the opportunity to visit the well equipped laboratories of the Station to view the unique collections of the living fruit and vegetable material and to attend the demonstrations of the experimental work carried on at the Station.

Accommodations at Ithaca

To enable members to obtain the kind of lodging accommodations they desire during the week of the Congress the Local Committee has recently sent out a statement regarding facilities at Cornell. This Committee is especially anxious to know by August 8 the names of all those who plan to attend the Congress.

The available lodging facilities in Ithaca, with rates for various periods, are indicated below:

Willard Straight Hall

Willard Straight Hall, the student union building, on the lower Campus, is headquarters for the Congress. Here members will register the first day. The evening reception and afternoon teas will be held there. Its cafeteria and dining rooms will accommodate the entire membership. Several small rooms are available for evening group conferences.

There are in Willard Straight Hall a limited number of bedrooms, mostly with running hot and cold running water, but without private bath, at the following rates:

Single rooms, $3.50 per day; $19.00 per week
Double rooms, $6.00 per day; $32.00 per week

One suite of two double rooms with connecting bath can be had for $14.00 per day or $72.00 per week, for four persons. Another suite of one double and one single room with connecting bath is available at $10.50 per day or $54.00 per week for three persons.

A dormitory on the top floor, accommodates 23 persons at $2.00 per day or $10.00 per week.

Prudence Risley Hall

This is one of the University residential halls. It is on the Campus a little over one-half mile from Willard Straight Hall. Mostly single rooms. The double rooms are large and have two complete sets of furniture. Adequate bath and toilet facilities on each floor. The rates are:

1-3 days, $2.00 per day
4-7 days, $1.75 per day
8 or more days, $1.50 per day

Sheldon Court

A private University dormitory just off the Campus at the south entrance about one-third mile from Willard Straight Hall. Sheldon Court accommodates approximately 100 persons, *men only*. Mostly single rooms. Adequate bath and toilet rooms on each floor. Free parking space off the street for about 30 cars. The rates are:

$1.50 per day or $8.75 per week

Student Rooming Houses

There are a number of good rooming houses, accommodating from five to twenty persons each, within two or three blocks from the south entrance to the Campus. The rates are:

$1.00-$1.25 per day according to length of occupancy.

Camping

Camping facilities are available on the University Campus. The Cornell Athletic Association offers free camp sites on a well drained grass plot back of the stadium. Tents may be pitched under the stadium if shelter is desired. Water, toilet facilities, and firewood are available. The College of Agriculture will furnish a guard from 8 A. M. to 11 P. M.

The Finger Lakes Park Association will reserve, on request, camp sites at Enfield State Park and Taughannock State Park. Upper Enfield, above the gorge, is eight miles from the Campus. Taughannock camping ground is in the woods by Cayuga Lake, below the falls. Camp site, water, toilet, firewood, and garbage disposal are provided for fifty cents per day, or $2.50 per week, per camp unit.

Sports

Free use of facilities for tennis, soft baseball, and quoits, are provided on the Campus. There are bathing beaches or pools at Taughannock, Lower Enfield, Stewart Park, two miles from the Campus, and Beebe Lake, on the Campus. Arrangements are being made for reduced greens fees for members of the Congress on the Country Club golf course.

All who expect to attend the Congress are urged to communicate with the Local Committee in time to make final application for accommodations by August 8, or as soon thereafter as possible. Address:

DR. R. A. EMERSON,
Cornell University,
Ithaca, New York.

The

Journal of Heredity

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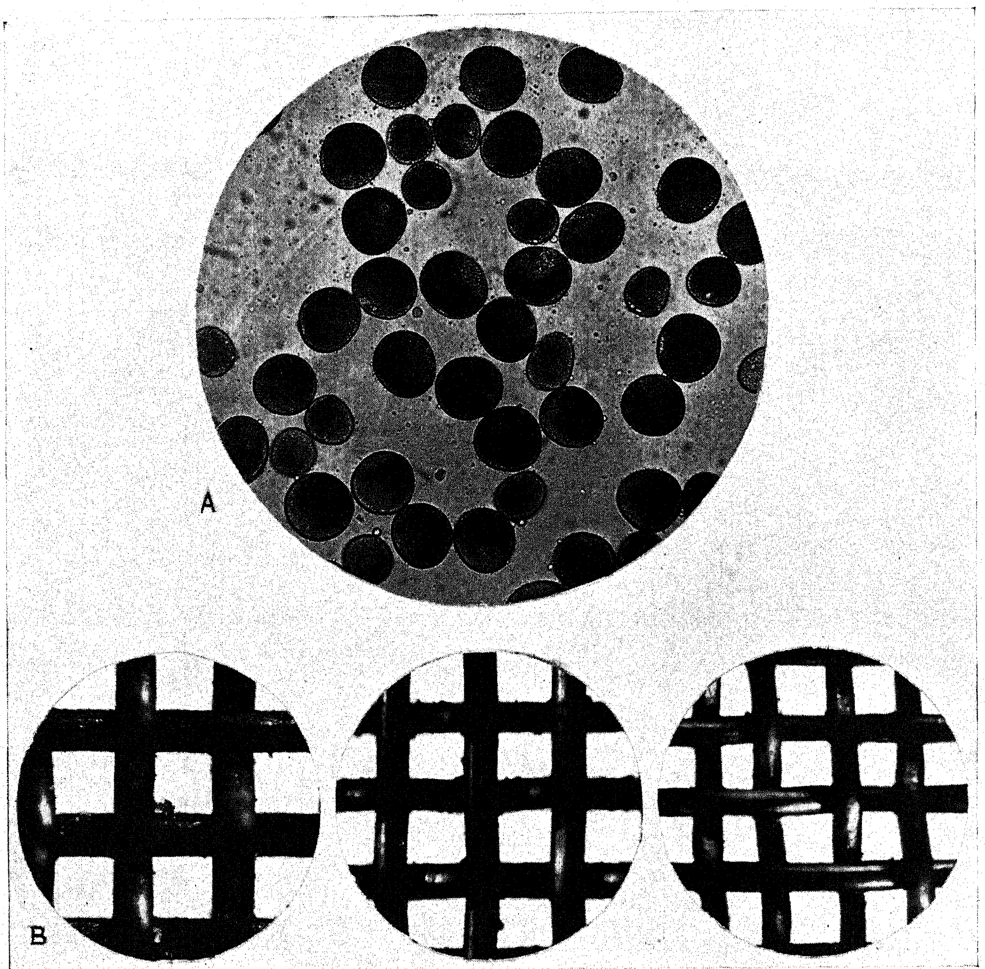


"HIGH SUGARY" POLLEN, AND SIEVES USED TO SEPARATE GAMETES

Frontispiece

Photomicrographs showing, magnified sixty-five times, a sample of "high sugary" pollen (*A*) and sieves used to separate the large grains from the small (*B*). The existence of a linkage between the "sugary"—"starchy" gene-pair, and a pair of genes (or some other genetic condition) affecting size of pollen has made it possible to separate by mechanical sifting the sugary and the starchy gametes with a considerable degree of accuracy. This is demonstrated by the ears shown in Figure 1, which were produced by pollination with the fractions of pollen passing through the three sieves. The sieves are of the kind used for testing the fineness of cement, etc.; the specified openings of those shown are .088 mm., .074 mm., and .062 mm.

MECHANICAL SEPARATION OF GAMETES IN MAIZE

P. C. MANGELSDORF

Texas Agricultural Experiment Station

THERE are numerous cases in which gametes from the same individual, differing in the genes or chromosomes which they bear, also exhibit a corresponding diversity in physical or chemical characteristics, or in their response to various conditions which they encounter.

Physical diversity is illustrated by spermatozoa of some animal species which show a dimorphism with respect to length of head, presumably associated with differences in the sex chromosomes and the amount of chromatin. In plants, the addition or loss of a chromosome frequently has a marked effect on the size of the pollen grains.

Chemical differences, readily detected by a simple staining reaction, are apparent in pollen grains from the same plants when the character known as *waxy* is involved. This differential staining reaction has been found in three different genera of grasses, *Oryza, Zea,* and *Coix,* and probably occurs also in *Andropogon.* Many other chemical differences in gametes undoubtedly exist, which are yet to be detected.

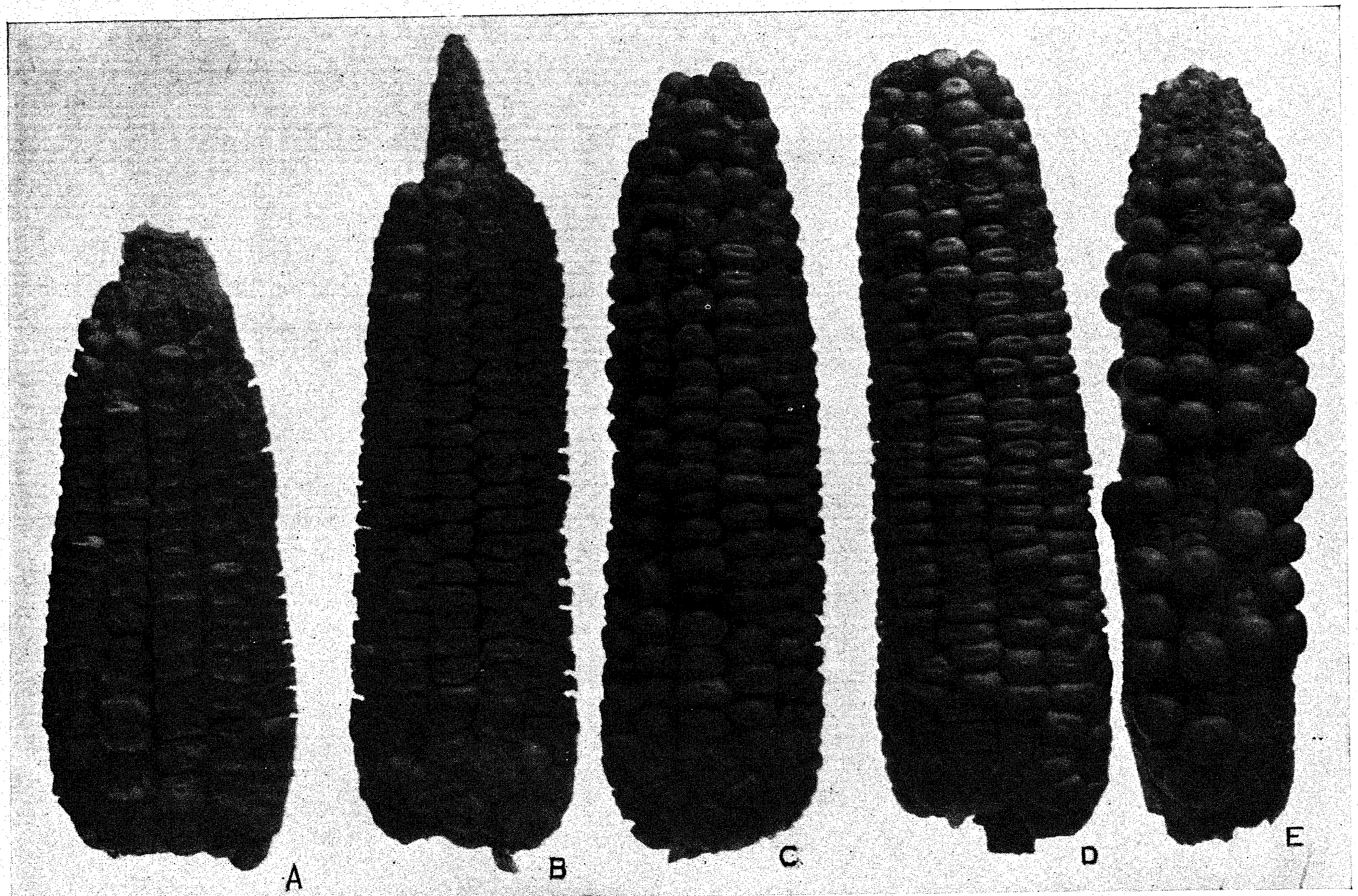
Differences in response of the gametes to various conditions are especially common in plants, and the literature is replete with reported cases of differential pollen tube growth, longevity, resistance to desiccation, etc., all of which may contribute to disturbances in Mendelian ratios. The literature on this subject has been ably reviewed and summarized by Jones[2]. The reader is referred to his book for further details.

Though diversity of the gametes is a well established fact, their actual separation into distinct classes on the basis of physical, chemical, or other differences, has seldom been undertaken. I am familiar with only one serious effort to accomplish this, though there may be others which I have overlooked. Lush[3] attempted to separate the male determining and female determining spermatozoa in rabbits and swine by centrifuging. In the experiments with rabbits the sex ratio was not altered. In the case of swine no breeding results were reported, but some difference in size was found between spermatozoa from the inside and from the outside fractions of the centrifuge tube.

The present paper is written to report the mechanical separation of starchy and sugary gametes from heterozygous maize plants. This separation was accomplished by virtue of close linkage of the starchy:sugary genes with a pair of genes, or some other genetic condition, affecting the size of the pollen grains. It suggests, therefore, that separation of the gametes, when no physical or chemical differences are displayed, may, nevertheless, be accomplished if close linkage with variations in size can be discovered.

Unusual Segregation for Sugary Endosperm

The stock in which this linkage occurred is one in which the writer, in collaboration with Dr. D. F. Jones and Dr. W. R. Singleton, has, for a number of years, been studying the inheritance of a peculiar condition which we have termed "high sugary." Plants of this stock which are heterozygous for the well known recessive character, sugary endosperm produce about 66 per cent of sugary seeds when self-pollinated, instead of the expected 25 per cent. When the heterozygote is backcrossed on the recessive, approximately 94 per cent of the seeds, instead of the usual 50 per cent, are sugary. The ability to produce these aberrant ratios is transmitted from generation to genera-



EARS RESULTING FROM POLLINATION WITH SIFTED POLLEN

Figure 1

A—Ear resulting from pollination with untreated pollen of the "high sugary" strain. Only a few starchy grains are to be seen. *B*—Ear pollinated with fraction of pollen remaining in larger sieve. *C*—Ear resulting from pollination with fraction in medium sieve. *D*—Ear from pollen fraction remaining in smallest sieve. *E*—Ear pollinated with fraction passing through smallest sieve. The average percentages of sugary seeds resulting from the pollinations were (*A*) 94.0, (*B*) 93.3, (*C*) 86.7, (*D*) 41.8, and (*E*) 25.9, demonstrating conclusively that the sifting process has significantly altered the distribution of "starchy" and "sugary" pollen in the various samples. This represents one of the first, if not the first, successful attempt to separate mechanically, gametes differing in their physical and genetic characteristics.

tion, but only through about 15 per cent of the ovules, and very rarely through the pollen. In our experiments the "high sugary" condition has been transmitted through the pollen only four times in 386 trials, an incidence of only slightly more than one per cent.

All plants which produce high sugary ratios have variable pollen. Measurements of the pollen grains show that the distribution, with respect to length, is bi-modal and we have estimated that approximately half of the grains are smaller than normal, though quite souna and well packed with reserves, while the remainder are normal. We have found that the gene, or other condition responsible for the production of *tiny* pollen is located in the third chromosome, to the left of sugary, and that the crossing-over is approximately six per cent. Other characters in this linkage group, including defective endosperm, de_{16}, tunicate ear, *Tu,* and tassel seed, Ts_5, are also affected.

There are many other peculiar features involved in the inheritance of the "high sugary" condition which need not be mentioned here since a complete report, to be published jointly with Dr. Jones and Dr. Singleton, is now in preparation. The facts already presented are probably sufficient to serve as a background for the experiments herewith reported.

Variation in Size of Pollen

Figure 2 shows the frequency distributions, with respect to length, oi 1,350 pollen grains from eleven "high sugary" plants and 1,100 grains from seventeen normal plants heterozygous for sugary. It is at once apparent that the distribution representing the pollen grains from "high sugary" plants is distinctly bimodal, with one mode at 99 microns and the other at 84. Since the two distributions represented by these modes are over-lapping it is impossible to ascertain exactly the proportion of the population that should fall into each. A rough approximation may be arrived at, however, by making each of the over-lapping curves symmetrical. If the left curve were made symmetrical, without regard to the right one, it would include 60.66 per cent of the observations. If the right curve were made symmetrical, without regard to the left one, only 37.22 per cent of the observations would remain in the latter. The average of these two extremes is 48.94 per cent, which does not differ significantly from the 50 per cent expected in a 1: 1 distribution. Furthermore, it is possible, in some samples of pollen from "high sugary" plants, to classify the grains as *normal* and *tiny* by observation and in such cases the distribution usually approaches a 1: 1 ratio. In this connection it must be remembered that length, which involves but one dimension, is not always an adequate measure of size or volume, which represents three dimensions, especially when there is some variation in length due to shape alone. The pollen grains of corn are usually ovoid in shape, but some grains are almost spherical and these will be shorter though the volume remains the same.

With the assumptions, (1) that the pollen from "high sugary" plants comprises approximately equal numbers of *normal* and *tiny* grains, (2) that the *tiny* grains rarely or never function in accomplishing fertilization in competition with the normal grains, and (3) that the sugary gene is associated with normal pollen while the allelomorph, starchy, is linked with *tiny* grains, the peculiar ratios obtained from back-crossing are readily intelligible. As already noted, the back-crosses produce, on the average, 94 per cent sugary seeds and 6 per cent starchy seeds. The latter represent the cross-overs and the former the non-cross-overs.

It is evident that if the *tiny* grains could be isolated, and if they would accomplish fertilization when competition with *normal* grains is eliminated, the starchy: sugary ratio would be completely reversed and the backcrossed ears should bear 6 per cent sugary seeds and 94 per cent starchy. With these considerations in mind we have attempted to separate the *normal* and

tiny pollen grains by passing the pollen through a series of fine-meshed sieves, a procedure originally suggested by Dr. L. J. Stadler. After a number of fruitless trials, involving different kinds of screens and other variations in procedure, very decisive results have finally been obtained.

Sorting the Pollen According to Size

The sieves used are of a type commonly employed in testing the fineness of cement and similar materials, known as the U. S. Standard Sieve Series. The three sieves used had specified openings of .088 mm., .074 mm., and .062 mm. These openings are surprisingly uniform in size, at least in the two larger sieves, as is shown by the photomicrographs in Frontispiece, which also illustrate the relation of the size of the openings to the size of the pollen.

Pollen was collected on each of five different mornings from a group of fifteen or twenty heterozygous sugary plants, from which pollen had previously been examined and found to be variable. To avoid including any more shriveled pollen than necessary, the collections were made when the anthers first began to dehisce in the morning. The pollen was immediately taken into a cool, moist basement and sifted. The sieves, nested into each other, were shaken by hand for a period of about five minutes, after which the fractions were poured out separately on sheets of smooth paper, transferred to small glassine envelopes, and immedately taken to the field where the pollinations were made on various stocks homozygous for sugary endosperm.

Examination of the pollen from different fractions, under the microscope showed that there were some tiny grains remaining in the coarsest fraction but that no large, sound grains had passed through the finest sieve. Such large grains as passed through the finest sieve were all desiccated and shriveled. The following observations, made on the fractions sifted on June 5, are typical:

Fraction remaining in sieve............ .088
  All sound, few small.
Fraction remaining in sieve............ .074
  All sound, large and small.
Fraction remaining in sieve............ .062
  About half are shriveled; all sound grains are small.
Fraction passing through sieve....... .062
  Mostly shriveled; a few sound grains, all small.

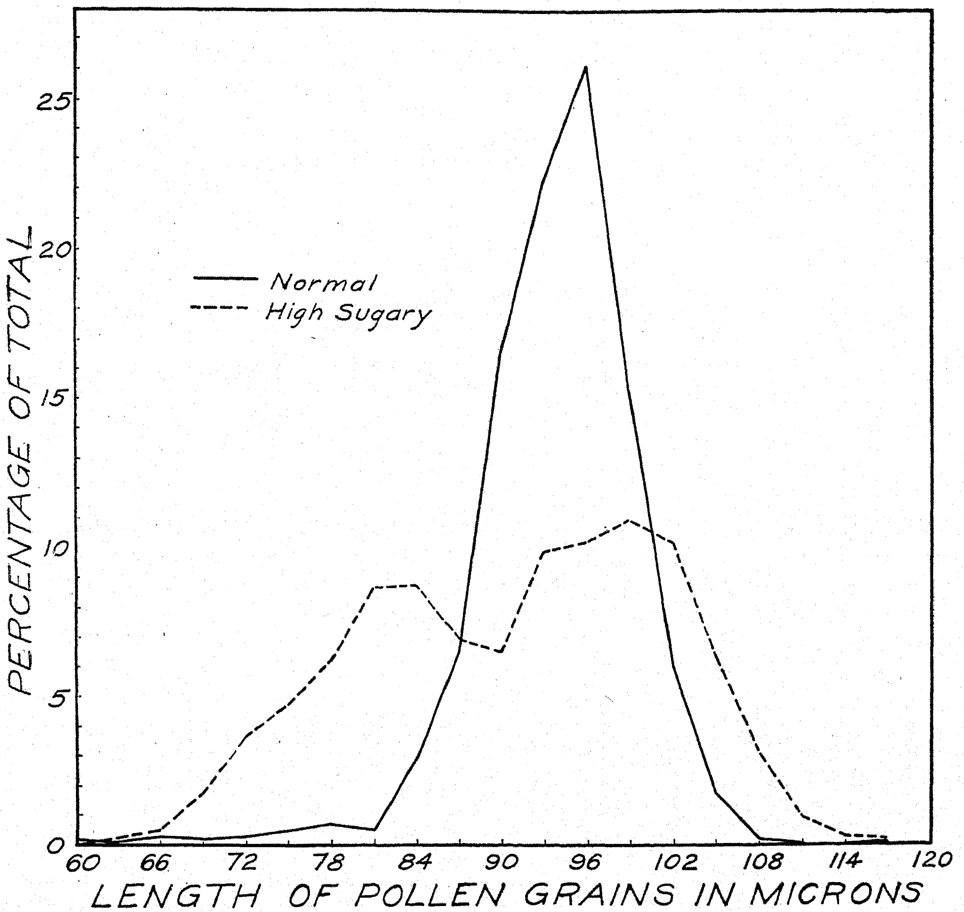
The results of pollinating homozygous sugary plants with these various fractions are shown in Table 1. The fraction from the largest sieve was not used in the first test because of a shortage of homozygous sugary plants on which to make the pollinations. Untreated pollen was restricted, for the same reason, to only two of the five tests. We have found, however, from other extensive experiments, that there is very little variation from day to day in the percentage of sugary seeds in back-crosses resulting from pollination by "high sugary" plants.

Effect of Pollen Separation on Ratios

It is noted that Lot 1, representing the unsifted pollen, which was tested on two days, produced 94 per cent of sugary seeds on both occasions, indicating 6 per cent of crossing-over between the two factors involved.

Lot 2, pollinated with the fraction remaining in the coarsest sieve, which consisted mainly of large grains, yielded almost identical results, 93.3 per cent, showing that the removal of the small grains from the sample had little or no effect on the gametic ratios, and indicating further that the small grains ordinarily do not accomplish fertilization in competition with the larger grains.

The next three fractions, represented by the ears in Lots 3, 4, and 5, produced 86.7, 41.8, and 25.9 per cent sugary seeds, respectively. All of these values differ significantly from the percentage in the first fraction, from the untreated pollen, and from each other. It is noted that, though there is some variation in the percentage of sugary seeds in the different tests, the lower fractions always produced a lower percentage of sugary



DISTRIBUTION OF NORMAL AND "HIGH-SUGARY" POLLEN

Figure 2

Frequency distribution, with respect to length, of 1,350 pollen grains from eleven "high sugary" plants and 1,100 grains from plants heterozygous for sugary.

seeds than the higher fractions from the same collection of pollen. This is true in each of the five separate experiments. The results are so consistent and decisive that calculation of statistical odds seems almost superfluous.

Ears resulting from pollination with the different fractions are illustrated in Figure 1.

In none of the tests was the percentage of sugary seeds reduced to the 6 per cent expected if only *tiny* grains had been included in the lower fraction. In the experiment of June 6, however, which averaged 15.3 per cent sugary for the lowest fraction, there was one ear which produced only 8.1 per cent sugary seeds, a figure that does not differ significantly from the expected 6 per cent.

There are two possible explanations for the failure to reduce the proportion of sugary seeds regularly to 6 per cent. The first is that some of the large, shriveled grains, which pass through the finest sieve, may recover their turgidity on contact with the moist silks, and may germinate and function. Examination of a few of the pollinated silks under a microscope, 24 hours after

pollination, showed that germination was confined to the small, sound grains. There is, however, still a possibility that a few large grains, shriveled enough to pass through the sieve but not enough to have lost their viability, would be included in the lower fractions, and in contact with a moist stigmatic surface, would recover sufficiently to accomplish fertilization.

A more probable explanation, however, is that some of the small grains, which pass through the sieve, are genetically *normal* and not *tiny*. Any sample of normal pollen contains some small, sound grains. The distribution in Figure 2 shows that the smallest of the *normal* grains are as small as the *tiny* grains from "high sugary" plants. These small, *normal* grains, passing through the finest sieve, would, presumably, be 94 per cent sugary and 6 per cent starchy and would tend to raise the percentage of sugary seeds produced by the fraction in which they are included. However, if the small *normal* grains functioned no better than the genetically *tiny* grains of the same size, we must assume that almost one-fourth of the fraction which produced 25.9 per cent sugary seeds consisted of small normal grains.

There is some evidence that the *tiny* grains do not compete on an equal basis with *normal* grains of the same size, or, to be precise, with *normal* grains in the same fraction or size range. This evidence consists of several ears which were poorly filled, indicating that the supply of functional pollen had been insufficient. With a limited supply of pollen, so that there is, in many cases, only one functional grain per style, competition would be greatly reduced and it might be expected that a larger percentage of the *tiny* grains would function, and hence a lower percentage of sugary seeds would be produced.

Of three ears resulting from pollination with the fraction passing through sieve .062 on June 6, one poorly filled ear yielded 8.1 per cent sugary seeds while two well filled ears produced 17.5 per cent sugary, or more than twice as many. In Lot 3, on the same date, one poorly filled ear produced 23.5 per cent sugary while three well filled ears bore 37.1 per cent sugary seeds. These results, though by no means conclusive, may be suggestive of unequal competition between *normal* and *tiny* grains even within the same size range.

That normal pollen grains, capable of accomplishing fertilization, will pass through at least two of the sieves used, is shown by another experiment in which pollen from a heterozygous sugary plant, which segregated normally, was sifted. Incidentally, this experiment also shows that the separation accomplished in the first experiment is due to genetic association of the starchy: sugary genes with differences in size of pollen and not the result of inherent size differences between the starchy gametes and the sugary gametes. The results are shown in Table II.

It is noted that the percentage of sugary seeds resulting from pollination with the different fractions are alike within the limits of random sampling. None of the values differ significantly from the percentage in the untreated pollen or from the expected 1: 1 ratio.

In addition to the pollinations represented in Table II, one pollination was made with the fraction passing through the finest sieve. This ear, unfortunately, was lost, either through carelessness at harvest time or because the bag had blown off previously.

Possible Uses of Gametic Separation

The fact that *tiny* pollen grains, which are ordinarily non-functional, will accomplish fertilization when competition with *normal* grains is reduced or eliminated suggests the possible usefulness of gametic separation in certain types of genetic studies, particularly where some classes of gametes are regularly eliminated because of extra or lacking chromosomes. It may also have some rôle to play in plant breeding operations, especially when dealing with species crosses, which often exhibit great pollen variability. Harrington[1]

has recently shown that, in crosses of emmer and common wheat, *Triticum dicoccum*×*Triticum vulgare*, seeds with shriveled endosperm are more likely to produce plants with *vulgare* characteristics than plump seeds. Hence the condition of the endosperm may be used to some extent as a basis of selection for certain types, before the seed is planted. Perhaps selection among the gametes might yield even greater returns, especially in material where artificial pollinations are easily made, such as crosses of maize and teosinte.

Summary

1. In a stock known as "high sugary," heterozygous plants produce pollen of two sizes, *normal* and *tiny,* in approximately equal numbers.
2. The *normal* grains are linked with the sugary gene with approximately six per cent of crossing-over.
3. The *tiny* grains, which are linked with the starchy gene, do not function in competition with *normal* grains, hence all but six per cent of the starchy gametes are eliminated, and backcrossed ears yield 94 per cent sugary seeds.
4. It has been possible, by passing the pollen through a set of fine-meshed sieves, to separate the two classes of pollen, and separation on the basis of size has also separated the starchy and sugary gametes.
5. Different fractions of pollen from the same plants produce markedly different results when used in pollination. The percentage of sugary seeds has been reduced from 94.0 to 25.9.
6. Separation of the pollen grains on the basis of size has no effect on the ratios when pollen from normal heterozygous sugary plants is used.
7. Possible uses of gametic separation in genetic studies and plant breeding are suggested.

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TABLE I—Results of Pollinating Homozogous Sugary with Various Fractions of Pollen from Heterozygous "High Sugary" Plants.

Pollen used	Date of test	Seeds produced: Total	Sugary	Per cent sugary
1. Not treated	6/5	613	576	94.0
	6/6	557	524	94.0
	Total	1170	1100	94.0
2. Fraction remaining in sieve .088 mm.	6/4	436	401	92.0
	6/5	540	510	94.4
	6/6	585	557	95.2
	6/9	400	362	90.5
	Total	1961	1830	93.3
3. Fraction passing through sieve .088 mm. but remaining in sieve .074 mm.	6/3	467	403	86.3
	6/4	583	520	89.2
	6/5	327	263	80.4
	6/6	336	301	89.6
	6/9	495	428	86.4
	Total	2208	1915	86.7
4. Fraction passing through sieve .074 mm. but remaining in sieve .062 mm.	6/3	489	259	53.0
	6/4	130	63	48.5
	6/5	362	111	30.7
	6/6	1027	363	35.3
	6/9	558	277	49.6
	Total	2566	1073	41.8
5. Fraction passing through sieve .062 mm.	6/3	103	34	33.0
	6/4	221	65	29.4
	6/5	57	16	28.1
	6/6	268	41	15.3
	6/9	263	80	30.4
	Total	912	236	25.9

TABLE II—Results of Pollinating Homozygous Sugary with Fractions of Pollen from Normal Heterozygous Sugary Plant.

Pollen used	Seeds produced: Total	Sugary	Per cent sugary
1. Not treated	2010	987	49.1
2. Remaining in sieve .088 mm.	1214	612	50.4
3. Remaining in sieve .074 mm.	781	393	50.3
4. Remaining in sieve .062 mm.	210	110	52.4

SEPARATED SINCE CHILDHOOD

Figure 3

Twins *D* (left) and *B* (right), taken when they were thirty-four years old. There was less difference then in physical condition than at present (four years later) when *B* weighs 8½ pounds more than *D*, who is decidedly underweight.

Separated at fourteen months of age, these twins did not know of each other's existence until they were sixteen. When they were re-united then, their best friends could not tell them apart. One of them married a man in comfortable circumstances and the other a farmer who was homesteading a claim in North Dakota, so that their environment has been quite different during adult years. This is reflected in some differences in physique, but much less in intelligence and emotional tests, which still classify them as extremely alike.

MENTAL AND PHYSICAL TRAITS OF IDENTICAL TWINS REARED APART

Case V. Twins "B" and "D"

H. H. Newman
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IN the report of our last case, Case IV, we expressed some pessimism as to the likelihood of securing any more cases of identical twins reared apart. Evidently this pessimism was ill founded, since, largely as the result of a radio talk on identical twins by the writer, four or five new cases have come to light. The first of these new cases will form the basis of the present report, and the second will be published in September.

Twins *B* and *D* are older than any of our previous twins, being now thirty-eight years of age. Both are married women with children, *B* having four children, *D* six, none of them twins. The twins are the first children of a very young couple, the father being only eighteen years old and the mother seventeen when the children were born. The parents tried for fourteen months to get along with the children, but had to give them up at that time because of financial difficulties. The twins were put in a children's home in Iowa. A wealthy woman wanted to adopt them both, but when the authorities insisted that they be separated, she lost interest and would not accept either. Shortly after this they were adopted by two different persons and were separated, never to see or hear of each other again until they were sixteen. Their first meeting was brought about somewhat dramatically.

While *D* was assisting in her foster-father's store, a salesman visiting the store saw her and began a friendly conversation with her as though he knew her well. When she rebuffed him, he asked her if she were not *F— R—*. She said she knew of no such person. The salesman then asked her foster-father if she were not *F— R—*. He replied that she was not, but that she had a sister of that name. The girl *F— R—* was the adopted daughter of the next door neighbor of the salesman and the two families were on friendly terms. Hence the mistake in identities was, to say the least, quite excusable. It was only natural that an arrangement should be made to bring these sisters together. When *B* got off the train *D* said it was as if she saw herself getting off, so extremely similar were the two girls. They spent a happy three weeks together wearing each other's clothes and pretending to be each other. They had no difficulty in deceiving their best friends.

They developed at once a very strong mutual affection, which has never diminished though they were forced to separate for another period of several years. They corresponded a good deal during the period of separation. Both twins moved around from place to place a good deal, but when they were twenty years old *D* decided to go to *B*'s town and live with her. They secured situations in two different business firms, but often took each other's place for a joke, without being discovered. It was at this time that the photographs shown in Fig. 5 were taken.

The twins lived together for about a year, until *D* married. About two years later *B* married also. Since then they have never lived together and have only visited occasionally. It should be noted that the twins were entirely apart from fourteen months of age to sixteen years

PALM PATTERNS OF THE TWINS

Figure 4

Palm patterns of *D* (above) and of *B* (below). Note that the right palm of *D* is like the left palm of *B*, and that the left of *B* is like the right of *D*. These cross-resemblances are greater than the resemblance between the twins' own right and left hands.

of age, and were together for less than two years after that.

Up to the time that the marriage of *D* caused their separation, they were so similar in every way as to be practically indistinguishable. What differences, especially physical ones, that are now so strikingly apparent must have arisen since they were twenty-one years of age.

There was very little difference in educational experience between the twins, *B* going through the eleventh grade, *D* through the tenth. *B*'s education was somewhat less interrupted and possibly in somewhat better schools, for she went to school in good sized towns, while *D* went to country schools.

They both had the usual run of children's diseases in light form and were in equally good health until they were twenty-one.

The outstanding environmental difference may be summed up in the statement that after marriage, *B* had an easy life, *D* a hard one. *B* married a man who made a good income and was able to engage help for his wife whenever she needed it. *D*'s husband was at first a farmer in North Dakota and then a railroad brakeman, the family living either in the country or in rural communities. During the war *D* had to carry on largely alone while staking a claim on a new farm in North Dakota. During this time she had two children. Food was hard to get at times and of poor quality and variety. She also had a very serious attack of "influenza" during this time and took about three years to accomplish complete recovery. It may be that she has never fully regained her health since that illness. At all times *D* has worked hard and has reared a large family with great credit to herself.

There was also a great contrast in the social environment of the twins. Twin *B* has always been very socially inclined, being interested in women's clubs, musical organizations and amateur dramatics. She says she has always had "heaps of friends." Twin *D*, on the other hand, has been too busy for much social life, even had her rather meager social environment offered her the opportunity to participate in it. *D*'s six children, as compared to *B*'s four, must also have kept the former more closely confined to family life than was the case with *B*.

Are "B" and "D" Monozygotic Twins?

There is no question about the monozygotic origin of these twins. The fact that during their short periods of living together they were indistinguishable by their best friends and often successfully impersonated each other is sufficient without further evidence to classify them as monozygotic, but it seems well to offer what further evidence is available. Table I gives the physical characteristics in brief form. It will be noted that there are no significant differences in eye color; hair color, form and texture; skin characters; features; ears; height and width of head, etc.

The palm and finger patterns of the two are extraordinarily similar, almost as similar as any of our fifty pairs of identical twins reared together.

The finger-print formulae are as follows:

B Left: W.W.Wlu.Wlu.U.
D Left: W.Wlr.U.U.U.
B Right: W.Wr.U.U.Wlu.
D Right: W.Wr.U.Wlu.U.

The two right hands are more alike than either is like each twin's own left, being especially similar in having closely similar radial whorls on index fingers of both. The quantitative values of the finger prints are as follows:

		Right	*Left*	*Total*
Bonnevie method	B	45.5	36.5	82
	D	41	43	84
Newman method	B	56	59	115
	D	56	58	114
Actual counts	B	103	99	202
	D	99	102	201

It is readily seen that there is distinct mirror imaging in the quantitative values of the ridges, *B*'s right hand being more similar to *D*'s left hand than

like her own left hand. The Bonnevie method shows the total values of the two hands of *B* and *D* only two units apart, which is in itself clear evidence of monozygosity, for no fraternal twins could ever have so close a correspondence.

The palm pattern formulae are as follows:

B Left: 9(8). 7(6). 5.'5'—?—Au.O.O.O.L.
B Right: 7(8). 5"(6). 5.'5'—?—Au.O.O.O.L.
D Left: 7(8). 5"(6). 5.'5'—?—Au.O.O.O.L.
D Right: 9(8). 7(6). 5.'5'—?—Au.O.O.O.L.

Here we have a condition very common among identical twins and absent among fraternal twins, namely, mirror-imaging. The right hand of *B* is almost exactly like the left hand of *D*, more so than like her own left hand. Similarly, the left hand of *B* is like the right hand of *D*. All four hands are, however, very similar in pattern. Thus the dermatoglyphics alone would constitute almost conclusive proof of monozygosity.

General Physical and Behavioral Differences between the Twins

The most striking differences between these twins are those of physical condition. *B* is much better preserved in every way than *D*. Though both are of slender build and both small women, *B* weighs 8½ pounds more than *D*, who is obviously decidedly under weight. All the circumferential measurements of *B* are greater than those of *D*. *B*'s hair is thicker and in a little better condition. Most striking of all are the differences in the condition of the teeth. *B*'s teeth, though she is 38 years old, are absolutely perfect, not a bit of decay being present in any tooth and all being very white. This is quite unusual in a person of that age. In striking contrast is the fact that *D*'s teeth are in bad condition, the upper incisors being discolored and worn down to nearly one half their original length. The two lower premolars have been extracted, both right lateral upper incisors are hopelessly decayed, and other signs of incipient decay are present in other teeth. Since the teeth of *D* have been cared for as well as those of *B*, the poor condition is probably due to inadequate food during pregnancy.

In general, *B* looks hardly over thirty years old, while *D* looks nearer forty, even older than her real age. *D* has aged most decidedly during the last few years as is evidenced by Figure 3, when the twins were 34 years old and were much more similar than they are now.

B's behavior is much more confident and self-reliant than *D*'s. She takes the lead in all social relations. In some respects she seems to possess a stronger character, but *D* has shown courage and energy to a high degree.

I. Intelligence Tests

1. *Stanford-Binet Mental Test.* *B* took this test more quietly and easily than *D*, who seemed to worry over it somewhat. The results of the test were as follows:

	Mental Age	*I.Q.*
B	14 yrs., 10 mos.	93
D	14 yrs., 3 mos.	89
Difference	7 mos.	4

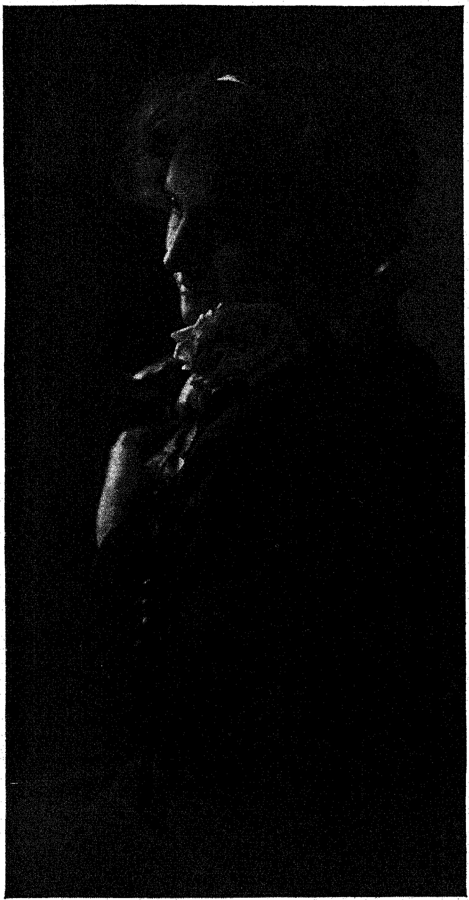
The differences were small, slightly less than the average differences of 50 pairs of identical twins reared together.

2. *Thurstone Psychological Examination.*—This test is designed to test the mental rating of college freshmen. Since neither of these twins had finished high school, the test was too difficult for them. *B*'s score was 30, *D*'s 20. This gives them a percentile rank of .007 and .003, respectively, that is, they come seventh and third from the bottom of a typical group of a hundred college freshmen. What difference there is favors *B* over *D*.

3. *Otis Self-Administering Test.*—On this test the relative scores were about the same as on the Stanford-Binet test:

	Score	*Mental Age*	*I.Q.*
B	31	13 yrs., 10 mos.	89
D	28	13 yrs., 1 mo.	86
Difference	3	9 mos.	3

Note that *B* is again slightly superior to *D*.



THE TWINS AT TWENTY
Figure 5

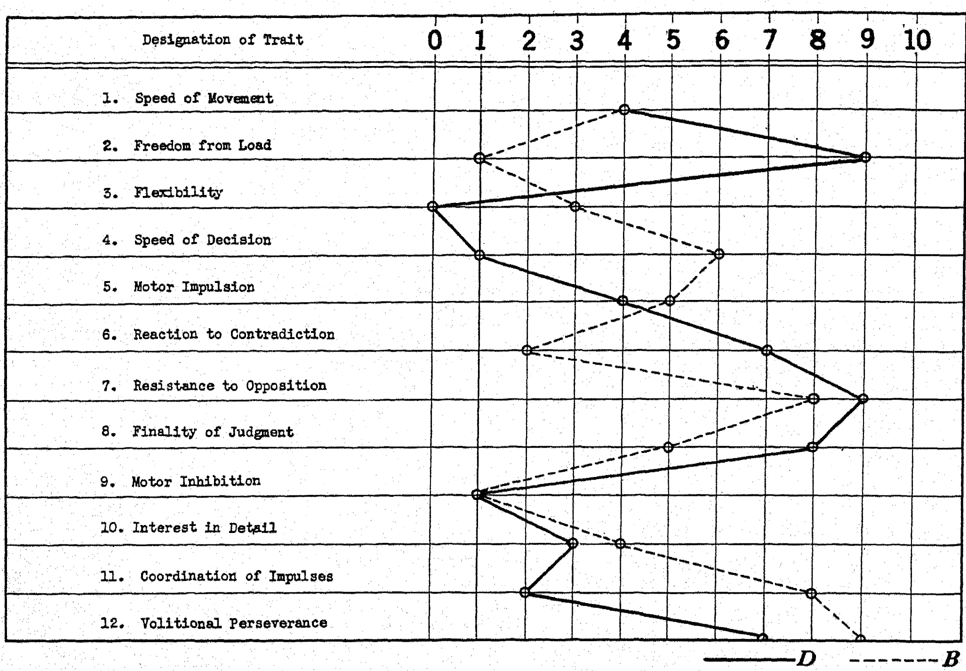
Twins *D* (left) and *B*, taken in the same gown and ornaments. At that time they were so much alike that they took each other's places frequently, and the deception was never discovered. Physical appearance and finger-prints prove that they are identical twins.

4. *International Test.* — *B*'s total score was 74, *D*'s was 69. These are very low scores. Both women were very tired when they took this test, and both would have done better after a rest. Nevertheless, the test shows the same slight superiority of *B* over *D*.

5. *Stanford Achievement Test.*—The results of this test are expressed in educational age, *B*'s educational age being 15 years, 2 months; *D*'s being 14 years, 8 months. There is a difference in favor of *B* of six months, which is only about a month greater than the average differences of 50 pairs of identical twins reared together. The scores on the different parts of the test correspond rather closely. Both did relatively well on reading, nature study, science and spelling, and relatively poorly on arithmetic, history and literature. There is about the same difference between the two women in educational age as in mental age, and the difference is in the same direction, *B* being slightly superior.

II. Temperamental-Emotional Tests

1. *Downey Individual Will-Temperament Test.*—The results of this test



THE "WILL-TEMPERAMENT" PROFILE

Figure 6

The results of this test indicate similarities rather than differences.

are shown graphically in Figure 6, the will temperament profile. There are four items in which there is a difference of five or more points out of a possible ten, namely, freedom from load, 8 points; speed of decision, 5 points; reaction to contradiction, 5 points, and coordination of impulses, 6 points. These differences, while they give a markedly different pattern, concern themselves with individual items within classes or groups, rather than with whole groups. They indicate therefore only a moderate difference in will-temperament characteristics.

High scores on the first five traits indicate a mobile, rapid-fire disposition; low scores, the opposite. The scores were:

B ....4,1,3,6,5=19÷5=3.9+
D ....4,9,0,1,4=18÷5=3.7+

They are both below average in these traits taken as a whole, though *D* is high in one of them, namely freedom from load. High scores on the last four items indicate a controlled, deliberate, careful person; low scores, the opposite. The scores were:

B ....1,4,8,9=22÷4=5.5
D ....1,3,2,7=13÷4=3.25

In these respects *B* is somewhat superior to *D*.

High scores on items 5, 6, 7 and 8 indicate a strongly aggressive and forceful personality. The scores were:

B ....5,2,8,5=20÷4=5
D ....4,7,9,8=28÷4=7

In these respects *D* was above average and *B* only average.

A high total score, around 90, would indicate a forceful personality and a score around 40 or 50 points would mean a rather striking deficiency in will-temperament qualities. *B*'s total score was 56, *D*'s was 55. They are both therefore about equal in ranking and just above average.

2. *Kent Rosanoff Association Test.*—In this test there appeared very little similarity between the twins. There

were only 6 identical responses out of 100. This is much the greatest dissimilarity we have yet found in identical twins and may be due to the very different environments. This test does not reveal any more similarity between *B* and *D* than might be expected between two persons picked at random.

3. *Pressy X-O Tests.*—These tests are designed to bring out resemblances and differences in emotions.

On the first test *D* marked 54 items. *B* only 33, indicating much greater susceptibility to feeling in *D*. On the second test *B* marked more items than *D*. On the third the differences were slight and on the fourth somewhat ambiguous. On the whole test the scores were *D*, 184; *B*, 167. These tests by themselves indicate *D* somewhat more emotional than *B*, but the difference is not very significant.

4. *Woodworth Mathews Personal Data Sheet.*—*B* marked 15 unfavorable answers, *D* marked 18. They agreed in 15 items. This is an unusually close similarity. It also indicates that the two women are about normal in their reactions.

Summary

B is slightly superior in general intellectual ability and in educational attainment, which may be due to the fact that she went through the eleventh grade, while *D* went through the tenth. On the whole, they are very similar. In temperament-emotional traits they are very different in some respects, very similar in others. On the whole, they are much more similar than different. In physical condition there is a very marked difference. This is the first case out of the six cases of identical twins reared apart where there was so slight a difference in both intelligence and temperament.

TABLE I—Physical Characteristics of "B" and "D"

Character	*"B"*	*"D"*
Height	156.6 cm.	156.4 cm.
Weight	110.5 lbs.	102.1 lbs.
Length of fore arm	R, 39 cm: L, 38.8 cm.	R, 38 7 cm: L, 38.9 cm.
Circumference of fore arm	R, 19.5 cm: L, 19.5 cm.	R, 18.8 cm: L, 18.7 cm.
Circumference of calf	R, 32.3 cm: L, 32.1 cm.	R, 31.5 cm: L, 31.7 cm.
Length of foot	R, 23.2 cm: L, 23.3 cm.	R, 23.1 cm: L, 22.8 cm.
Head length	17.2 cm.	17.2 cm.
Head Width	12.3 cm.	12.3 cm.
Cephalic index	71.5	71.5
Eye color	greenish gray	same
Hair color	golden brown	same
Hair texture	soft and wavy	same
Complexion	fair and smooth	not quite so smooth
Ear shape	small and shapely	same
Feet	slightly pigeon-toed	same
Voice	peculiar tone quality, very nasal	same
Features	*B*'s face a little broader than *D*'s	same except face a little narrower
Teeth	white and without any decay	front teeth worn and discolored, several teeth extracted and others decayed
Hair whorl	clockwise	same

A Perverted View of Eugenics

THE FAMILY, *by* E. B. REUTER, Professor of Sociology at the University of Iowa, and JESSIE RIDGWAY RUNNER, Pp. 616. McGraw-Hill Book Co., New York and London, 1931.

This book is a compilation of articles from a great variety of sources on all phases of family organization and disorganization. The compilers

have attempted, in the material they have quoted, to cover both or all sides of every question. Obviously, in so broad a subject, the choice of the quotations is all-important, and one cannot avoid the feeling that the mental attitude of the compilers has definitely influenced the selection of material. One gets the feeling in many of Professor Reuter's writings that he has a distinctly anti-biological outlook, and this feeling is strengthened by this book. What the reviewer has in mind is, perhaps, set forth by Professor Reuter in the Introduction to the chapter on "Birth Control and Eugenics" which closes with the following amazing paragraph:

> Attention should be called in passing to the conflict between the monogamous family ideal and the eugenic program for racial improvement. It is doubtless possible to make some minor improvements in the racial stock by preventing the reproduction of defective and inferior individuals where the condition is one of an heritable nature. It is quite probable that the struggle for existence in the early cultures eliminated a considerable percentage of the defective and natively inferior, and it is quite certain that infanticide as practiced among the Greeks, Romans, Chinese, and other peoples improved the quality by removing weak and defective individuals. But the selective removal of a small percentage of inferiors in each generation can have no appreciable effect on the average quality. In breeds of stock, improvements are made by selecting a few superior males to be the fathers of the next generation. In human society it would be possible to improve the quality by the same method. A real eugenic improvement would be possible with a polygamous type of family; it is not a possibility with a monogamous family system.

Naturally we looked through the quotations that followed to find some substantiation for so surprising and wholly absurd a statement. Since this matter is not discussed at all elsewhere, it is evident that the writer actually considers that it requires no proof. We are reminded of Aaron Burr's cynical definition of law: "whatever is boldly asserted and plausibly maintained." The very absence of any discussion gives these statements a certain false appearance of veracity.

It can hardly be questioned that the traits of a superior sire can be multiplied and fixed more rapidly by polygamous matings and by inbreeding (which Reuter does not mention) than by monogamous matings. He overlooks entirely the fact that the use of polygamous matings in herd improvement is largely economic rather than biologic; he ignores altogether the fact that the aims of herd improvement and eugenics are fundamentally different. The statement that eugenic improvement is not a possibility with a monogamous type of family remains a falsehood whether boldly or diffidently asserted. That he is willing to sponsor statements of this character brands Reuter as so amazingly ignorant of, or emotionally biased against, biological and genetic concepts that his extensive writings on racial matters scarcely deserve to be dignified by serious consideration.

ROBERT COOK.

A Table for Computing Elapsed Time

AS the calander is now arranged, weeks and months go neither uniformly into each other nor into years, so that problems dealing with elapsed time are always something of a minor nuisance to the statistician. Pearl and Minor, in the *Milbank Memorial Fund Bulletin* for April, 1932 (page 151), present a table which gives for any date the decimal values of that date for the elapsed part of the year and for the remaining part of the year. We are accustomed to consider June thirtieth the end of the first half of the year. As a matter of fact, this table shows at a glance that the second half begins at noon July second; and the last hours of October first represent the end of the third quarter.

Any person dealing with problems involving elapsed time should find this table extremely useful.

HEREDITARY BLINDNESS

LEWIS H. CARRIS
Managing Director of the National Society for the Prevention of Blindness

ANY attempt to catalog the major causes of blindness cannot fail to include the classification "Hereditary." It is not the purpose of this paper to review the scientific researches which have been made from time to time to determine the number of those who are blind or who have suffered serious loss of sight due to this cause. Such estimates vary from ten to fifteen and even a greater percentage.

The late Dr. Lucien Howe of Buffalo devoted much of his time, especially during the later years of his life, to research in the field of hereditary blindness. He proposed and advocated legislative measures to prevent the marriage of any person blind from an inheritable cause; but as far as I can learn, no such legislation has ever been placed on the statute books.

The question of the reduction or elimination of the number of persons blind from heredity is perhaps the most perplexing problem of the movement for the prevention of blindness—much more complicated than the reduction of blindness from such other causes as trachoma, accidents, ophthalmia neonatorum, etc. As a matter of fact, such activities as society may undertake to bring about a reduction of blindness from this cause must of necessity be a part of a larger eugenics program. From the beginning of the present century there have been undertaken a vast number of genetic researches which grew out of the discovery of Mendel's laws of heredity. It has been definitely established by these researches that certain physical characteristics are inherited and are inevitable in any scheme of propagation either in the vegetable or animal kingdom.

In the case of all animal life, where mental processes are a part of existence, the same limitations are placed upon the embryo resulting from the union of the egg and the sperm, such embryo resulting from a selection in equal numbers of the chromosomes of the two uniting nuclei.

The laws of inheritance are fixed and immutable and any attempt to improve society such as the eugenists are striving to accomplish, must be based upon the conscious control of the future race through the elimination of the reproduction of the physically and mentally unfit. This is a big proposition and has many implications into which this paper cannot go. It is sufficient to say that it is perhaps the greatest problem of society at the present time.

Considered in many of its aspects the question of inherited blindness is relatively small compared with the handicaps upon society through inheritance of certain other physical and mental characteristics, such as insanity, feeblemindedness, and epilepsy. The aim of society is to produce individuals who can successfully adjust themselves to the environment in which they must live. In one respect at least the question of hereditary blindness does not have the sociological importance which the other inherited characteristics do have; that is, blindness does not of necessity, imply diseased or impaired mind, and, after all, the greatest handicap which an individual can have insofar as its resulting in a permanent inability to adjust to a given environment, is that of a high degree of feeblemindedness or insanity.

Again, considered with reference to the amount of inherited blindness in comparison with other inherited handicapping physical characteristics, blindness is relatively limited. The number of blind people from all causes in the United States is approximately one out of every 1,000, or one-tenth of one per

cent. Assuming that 10% of those who are blind are blind from an inheritable cause, we have one out of every 10,000 or one one-hundredth per cent of the total population so affected.

As a general proposition, modification of the human race through selective parenthood or through the exercise of will on the part of the potential parent is very hard to accomplish. The perpetuation of animal life is dominated by instinct rather than reason. Any such control through reason and deliberation presupposes a high degree of intelligence and understanding and control over instinctive forces and this is found in an almost negligible proportion of the human race. The attacks which eugenists have been making through the advocacy of legislative restrictions and through sterilization have so far been quite limited in their application and such as have taken place have been an attempt to prevent the propagation of those who will be beyond question drags on society. It is probable, as time goes on, that a greater and greater amount of attention must be given to this problem if there is to be conscious control of the quality of future generations. There is some evidence to show that public health and welfare activities and the accumulation of wealth have militated somewhat against the natural law of the elimination of the unfit, and that the proportionate number of unfit is greater now than ever before. However, it may be that with the growth of humanitarian ideals and practices the number of individuals who are receiving the attention of society because of their inability to adjust themselves is greater than ever before.

What has been said above is not intended to deprecate the importance of reducing the number of blind from inheritable causes, but rather to give it its proper setting in the whole problem of heredity insofar as that problem affects the future welfare of the human race. However, those who work for and with the blind, and the blind themselves, have a responsibility for efforts which will tend to reduce the amount of inherited blindness.

Types of Hereditary Blindness

First of all, every blind person should know, insofar as he is able to ascertain, the exact cause of his blindness.

Dr. Clarence Loeb, of St. Louis, Missouri, prepared in 1909 the best statement that I have come across on hereditary blindness in his article on "The Marriage of the Blind from the Standpoint of a Physician." This paper gives the following principal forms of blindness which may be inherited:

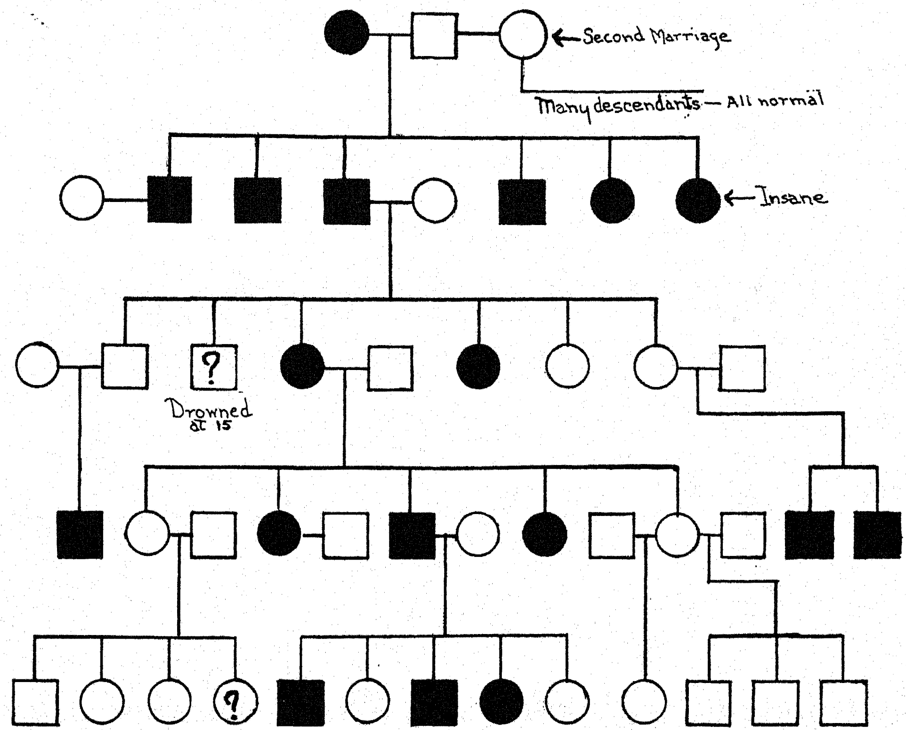
1. *Anophthalmus and Microphthalmus.*—That is to say, a total absence of the eye or an eye so small as not to provide useful vision. After a careful research, Dr. Loeb concluded that of such cases studied 74% of the offspring were affected through a direct, indirect, or collateral inheritance of this form of blindness.

2. *Buphthalmus.* This is the opposite of the foregoing and means an eye too large to focus. Here, again, the percentage of collateral inheritance exceeds 70%.

3. *Albinism,* or the absence or great shortage of pigment from the tissues of the eyes and especially in the iris which serves to shut out the excess of light. Dr. Loeb concludes that direct inheritance from this cause is only 12% but that collateral inheritance is very marked. In 8 families with 43 children he found 21 albinos, or 48%.

4. *Family Degeneration of the Cornea.* For this cause Dr. Loeb collected the histories of 18 families, 10 fathers and 8 mothers, with 62 children, of whom 26 were diseased, or 42%. Thirteen families with 50 children were reported, of whom 34, or 68%, showed collateral inheritance.

5. *Aniridia, and Coloboma Iridis.* This means the absence either wholly or in a large part of the iris. Dr. Loeb states that this condition shows marked direct heredity. The statistics which he gathered cover 59 families, with 29 affected fathers and 30 affected moth-



DOMINANT INHERITANCE OF GLAUCOMA

Figure 7

Chart showing inheritance of juvenile glaucoma simplex (Adapted from Courtney and Hill, *Journal of American Medical Association,* November 28, 1931). The family traces back to a Scotch immigrant, who arrived in Virginia about the middle of the Eighteenth century. He married a woman suffering from glaucoma, and the defect has appeared in every generation since that time. An accurate record has been kept of the members of this family who suffered from glaucoma. In spite of the fact that the hereditary nature of the disease was recognized, the families of glaucomatous individuals have been large. That this should be the case in spite of the painful and tragic consequences of the disease, is apparently due to the fatalistic religious beliefs of the members of this family, for they are obviously well above the average in intelligence and some of them have been leaders in the affairs of the nation in spite of the enormous handicap of this disease which has afflicted several members of each generation. They live now in an inaccessible foothill region of Virginia. What effect will be produced by a clear understanding of the mechanism of the inheritance of this tragic defect remains to be seen. The certainty that half the children of glaucomatous individuals will, on the average, be doomed to inherit this affliction, and that those without symptoms of glaucoma will not transmit it (unfortunately not always the case in this family if the record in the third generation is accurate) will serve both as a warning and an assurance.

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ers; there were 156 children, of whom 116 were affected, making a perecntage of 74%.

6. *Ectopia Lentis.* In this eye affection the crystalline lens is displaced and as a result the rays of light entering the eye are unequally refracted and a disturbance of vision takes place. In this instance 3 cases of direct heredity were quoted. the taint coming from the father in 19 cases and the mother in 24 cases. There were 155 children, of whom 109 were affected, a percentage of 70%.

7. *Cataract.* Dr. Loeb says: "Even if the lens is in its proper position, it

may be opaque, so that the disturbance in vision ranges from very slight to only ability to distinguish between day and night, depending on the amount and location of the opaque spots. This is one of the most frequent forms of hereditary blindness. The cataract may appear very late in life, or may be congenital. In some cases, each succeeding generation is affected at an earlier age, a condition called anticipation." Inasmuch as cataract appears to be the most frequent cause of inherited blindness, it may be well to quote still further from Dr. Loeb, and such extended quotation will also give an example of the thoroughness of this whole investigation:

Berry reported a family where the history was known for seven generations. First generation was normal. There were 2 members to the second generation, both of whom were normal. In the third generation there were 10 children, of whom 5 had cataract. In the fourth generation there were 9 children, all of a cataractous mother, of whom 4 had cataract and 5 were normal. In the fifth generation there were 6 children, all of a cataractous mother, of whom 4 had cataract. In the sixth generation there were 10 children, descended as follows: A cataractous mother had 1 normal and 4 cataractous children; a cataractous father had 2 normal children; a cataractous mother had 1 normal daughter, and a normal mother had a normal son. The seventh generation had 21 children, as follows: A normal father had 10 normal children; a cataractous mother had 2 normal children and 1 cataractous daughter; a cataractous father had 1 cataractous daughter by his first wife and 3 normal children by his second; another cataractous father had 1 cataractous daughter and 2 normal children; a normal mother had 1 normal child. Cooper stated in 1852 that he had been informed by the Duke of Sussex that cataract had been brought into the royal family of England by the marriage of one of his ancestors with a princess of Saxo-Coburg-Gotha. She had cataract and many of her descendants had the same affection. Among them was the Duke of Cumberland, who commanded at Fontenoy; George III, George IV, the Duke of Gloucester, the Duke of Sussex, Princess Sophie, and the King of Hanover.

Including the cases reported in the letters received, I have succeeded in collecting the histories of 304 cases of direct heredity of cataract. The father was affected 145 times, the mother 152 times, not stated, 7 times. There were in all 1,012 children, of whom 589 had cataract, and 423 had normal eyes; percentage of affected children, 58. In other words, in every family of 5 children, where either the father or mother have cataract, 3 of the children will at some time suffer from cataract. The percentage would probably be higher were it not that so many people with cataract die young, and so leave no descendants.

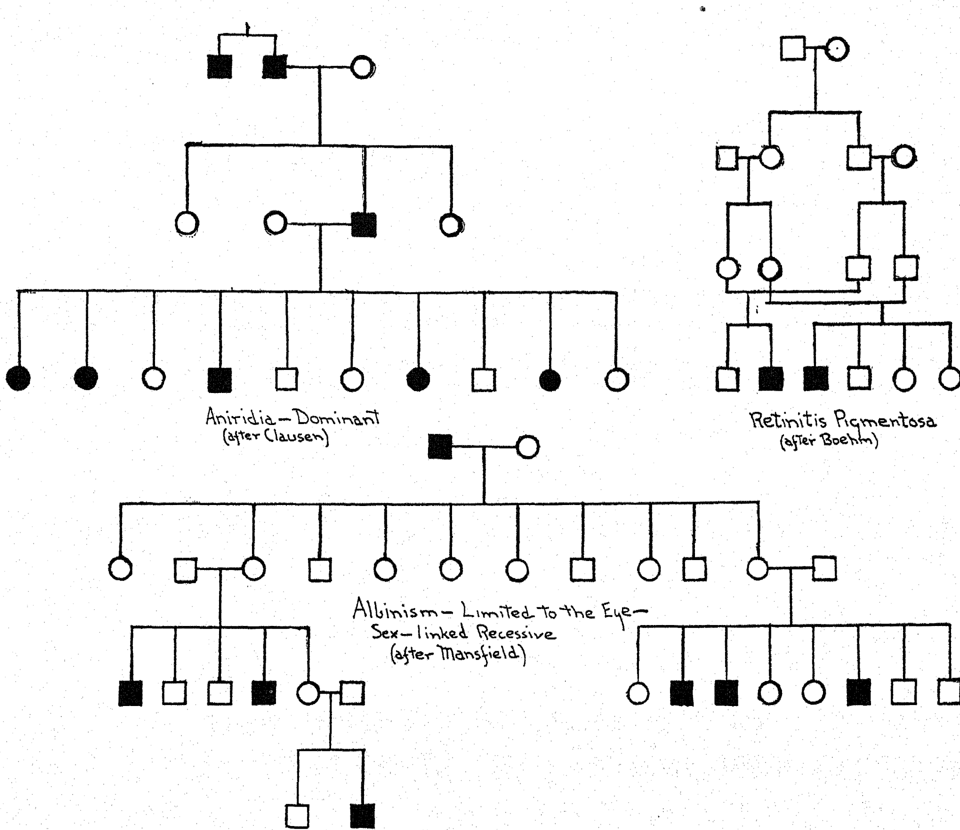
I have found only one case where both the father and the mother had cataract. There were 8 children, 4 of whom were normal, 2 had cataract, and 2 unknown. To these must be added the cases reported by Drs. ——— and ———. In one of these families there were 2 and in the other there were 5 children with cataract, in all 3 families with 15 children, of whom 9 had cataract, making 60%—slightly more than where only one parent is affected. The number of cases is too small, however, for a general rule.

I have found 13 cases of indirect inheritance from grandparent to grandchild, and 16 cases of indirect inheritance from uncle or aunt to nephew and niece. There were in these 29 families 107 children, of whom 62 were normal and 45 had cataract, equaling 42%.

In the literature, 66 cases have been reported of collateral inheritance; that is, cases of two or more children in the same family having cataract, while all other relatives have been free. There were 118 normal and 186 cataractous children. To these must be added the cases reported by Drs. ——— and ——— and ———, making 68 families, with 119 normal and 193 cataractous children, or 61.8% of the latter.

8. *Retinitis Pigmentosa.* This condition, which is practically the only disease of the retina showing hereditary tendencies, is characterized by a diminution of vision especially pronounced on a dark day and towards nightfall, although there may be almost total blindness on even bright days. This affection is subject to direct heredity, but its usual appearance is in the form of collateral inheritance, and it is often found in the offspring of consanguineous marriages. Of 133 cases of collateral inheritance reported, there were 541 children, of whom 349 were affected, or 64%. This is considerably larger than the incidence of direct inheritance, as shown by a record of 126 families, 61 fathers and 65 mothers, with 405 children, of whom 205, or 50%, had directly inherited the condition from the parent.

9. *Atrophy of the Optic Nerve.* This



INHERITANCE OF OTHER FORMS OF BLINDNESS

Figure 8

Three inheritance charts (modified from Crew) showing inheritance of Albinism limited to the eye, Aniridia (absence of iris), and retinitis pigmentosa. The Albinism is inherited as a sex-linked recessive, the retinitis pigmentosa as a recessive, and Aniridia as a dominant.

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is a form of blindness due to the degeneration or death of the optic nerve even though the rest of the eye may be normal. It is usually due to syphilis, but may be an inheritable condition, when it usually occurs in the form of indirect heredity from uncle to nephew; females are said to be rarely affected, although their male children frequently are. In studies made of direct, indirect, and collateral inheritance of this affection, the incidence was respectively 46%, 60%, and 70%.

10. *Glaucoma.* This disease, which in acute cases is characterized by severe pain, is responsible for a more or less rapid loss of sight due to the effect of the disease upon the optic nerve. Not all forms are hereditary. In 44 families where there were 131 children, 72 of them, or 55%, showed direct hereditary glaucoma. In 13 cases of collateral inheritance, 33 out of 53 children were affected, or 62%.

11. *Nystagmus.* By this is meant a more or less continual movement of the eyes. The condition is hereditary. With 8 fathers and 10 mothers affected, 26 out of 56 children, or 46%, had the disease. In 9 cases of collateral inheritance, 29 out of 40 children were affected, or a percentage of 70%. Three

families had 11 children, or 100%, showing indirect heredity.

12. *Strabismus and Ophthmoplegia Externa and Ptosis*. Ophthmoplegia externa, which is a more or less complete paralysis of one or more of the muscles of the eye or eyelid, is usually directly hereditary. The records of 32 cases, with the father affected 24 times and the mother 8 times, showed 121 children, 77 of whom, or 63%, had inherited the condition. In five cases of collateral inheritance there were 17 children, of whom 13, or 76%, were affected.

The above extended references to Dr. Loeb's pamphlet indicate fairly well the type of research which has been conducted to arrive at the probability of inherited eye defects. Some of the causes which are given are the result of syphilis, which is not mentioned by Dr. Loeb as a cause of hereditary blindness. It appears to me that considerable research still needs to be undertaken to determine eye defects which are the result of inherited syphilis. It is not possible within the limits of this paper to enter into any discussion of the question of syphilis as an inheritable disease, or to point out the distinction which can be made between certain forms of eye trouble which persist through many generations which are distinct from those caused by this dread disease.

But when it comes to the question of the responsibility for future generations, every person who is affected with this disease, whether he acquired it through heredity or any other means, must take the same and additional precautions as have been suggested above as the responsibility of anyone who has lost his sight as the result of an inherited physical characteristic.

It has not generally been an activity of organizations interested in the prevention of blindness to engage in a first-hand attack on the eradication of syphilis, although the extent of eye havoc which results from that disease is sufficiently extensive to warrant all possible support in the war for its extermination.

Non-Inheritance of Acquired Blindness

There are still many beliefs in some quarters which are fallacious and are based upon the wrong assumption that an acquired physical characteristic can be inherited. Therefore, any one who is blind from such a cause as trachoma, gonorrheal ophthalmia, or accident need have no fear of marriage from the standpoint of transmitting his lack of sight.

There is also one other confusion of terms sometimes made, that is the difference between congenital blindness and hereditary blindness. Not all here-

TABLE I.—Incidence of Blindness in the Children of the Blind*

	Disease	*Families*	*Total Children*	*Affected Children*	*Normal Children*	*% Affected Children*
I.	Albinism	13	60	23	37	
II.	Aniridia and Colomba Iridis	72	199	147	52	73.8
III.	Anophthal and Microphthal	48	117	87	30	74.4
IV.	Atrophy of Optic Nerve	150	506	312	194	61.6
V.	Cataract	404	1446	836	610	57.8
VI.	Ectopia Lentis	64	212	155	57	73.1
VII.	Family Degen. of the Cornea	32	114	62	52	54.4
VIII.	Glaucoma	58	194	109	85	56.2
IX.	Megalophthalmus	8	28	21	7	
X.	Nystagmus	30	107	66	41	61.7
XI.	Ophthalmoplegia and Ptosis	39	145	94	51	64.8
XII.	Retinitis Pigmentosa	286	1027	611	416	59.5
	Total—All Diseases	1204	4155	2523	1632	60.8

*Based on table appearing on page 38 of "The Marriage of the Blind from the Standpoint of a Physician," by Dr. Clarence Loeb.

ditary blindness or eye defects are congenital; for example, the most common kind of cataract is in many instances an inherited defect which does not appear until middle or late life. In some instances blindness may be both congenital and hereditary, as for example anaphthalmos, or the total absence of the eye. On the other hand, there is a considerable amount of congenital blindness which is not hereditary. This is due to accidents of birth. Fortunately, this is being appreciably reduced due to the great advancement which has been made in obstetrical practice.

Nature provides immunity to the embryo from practically all communicable diseases with the exception of syphilis, and any disease acquired by the child in the process of birth cannot be classified as inherited. Ophthalmia neonatorum, so often erroneously given as blindness from congenital causes, is communicated to the eyes of the child at or after birth, and hence cannot be classified as congenital.

This paper has attempted to deal exclusively with the subject of the sociological aspects and implications of hereditary blindness and has not entered into that larger question of the economic status of the afflicted. This is a general consideration which must be solved by everyone, including those who have lost their sight.

The Genetics Congress

Ithaca, New York, August 24-31

THE preliminary program of the Sixth International Genetics Congress has just been issued (August 1). This adds considerably to the details of the statement regarding the Congress published in the July JOURNAL, but time is so short that it is hardly worth while to publish the program in full. Readers of the JOURNAL who are planning to attend the Congress will have access to the final program by the time that this appears, including the abstracts of all papers and demonstrations to be presented at the Congress. Those who are not going to Ithaca will be more interested to learn of the high points of the Congress in our September number.

In spite of adverse economic conditions, it is certain that the Congress will be truly international in character. Between fifty and seventy-five delegates from Europe have signified their intention of attending, with representation from Japan and other countries also assured. The total enrollment is at present well over seven hundred.

At the five general morning sessions of the Congress, the following topics will be discussed by world authorities: *Thursday, August 25,* General Genetics; *Friday,* Mutations; *Saturday,* the interrelations of Cytology and Genetics; *Monday,* Genetics of species Hybrids; *Tuesday,* Contributions of Genetics to the Theory of Organic Evolution. The speakers who have accepted places on these programs were given in the announcement in the July number.

Our three hundred geneticists from thirty countries are taking part in the program, either with papers or with exhibits or demonstrations. Section meetings will be devoted to the following subjects: General Genetics; Cytology; Animal Genetics; Human Genetics; Methods and Technique; Genetics and Phytopathology; Chromosome structure and Crossing Over; Genetics of Species Hybrids; Drosophila; Genetics and Pathology; Problems Relating to Sex and Fertility; Fruit and Vegetable Breeding. The entire range of genetic research will be covered by the exhibits and demonstrations. These, and the social events, for which time has ingeniously been found by the committees including arrangements, should make the week one long to be remembered.

MUTTON, AND HOW IT GETS THAT WAY

A Review

GROWTH AND THE DEVELOPMENT OF MUTTON QUALITIES IN THE SHEEP, *by* JOHN HAMMOND. 597 pp. + XXVI, 139 tables (exclusive of the appendix), 137 figures, 71 diagrams and 2 text figures. Edinburgh and London, Oliver and Boyd. 1932.

"The object of the present investigation was to make a general survey of the scientific principles involved in the production of meat from the physiological, anatomical and practical points of view."—Introduction.

"As has been pointed out before, the object of the work was a general survey of the principles involved rather than the conclusive proof of the correctness of one or two of them, its aim being the suggestion of profitable lines of experimental work bearing on the development of the animal from the meat point of view."—page 198.

MR. HAMMOND has produced a compendium of information on how the sheep becomes what it is. The data presented are the author's own and the point of view is original and stimulating. Many of the topics discussed are not new but because of the author's original method of attack and original data, he comes to new conclusions with which the reader may not agree but which he can not ignore.

An extensive review of other works is evident in the number of titles cited. Part I, which deals with changes in live weight, ends with a list of 118 references. Part II, about the weights of the carcass and the organs, mentions only 77 titles. Part III, the various proportions of the skeleton and the effects of these on conformation, ends with the listing of 175 other works. Part IV, variations in the proportions of muscle, fat and bone in the carcass, is attended by 117 references. Part V (written in conjunction with A. B. Appleton) is a detailed study of the leg of mutton and includes an especially noteworthy section on the histoloy of the muscles. There are 173 references.

The comments on other works are particularly well arranged, being scattered under the appropriate subjects and along with discussion of the author's own findings, instead of being placed all together in a formal "review of literature," or "previous works" as is sometimes done. The reviewer is inclined to regret that the author has not been more critical in his references to these other works. In some places he seems to have cited with almost equal faith and enthusiasm the statements of those who were reporting their own actual data and those who were reporting hearsay; of those who wrote 50 years ago in the light of the science of their time, and those who wrote in the last ten years in the supposedly brighter light of twentieth century science; of those who worked with one or two animals and those who worked with dozens.

The book is monumental in scope. It is almost unique in the enormous number of questions which it attempts to answer. As an authoritative source-book its most serious weakness lies in the small amount of data bearing on any question except in Part I. Many tables in Parts II, III, IV, and V contain no averages based on more than three individuals. For example Table 36, on the proportions of the carcass and organs in different breeds, contains data on *only 10 individuals* and these are scattered among *six different breeds*. Within breeds this scant material was hardly even comparable: two of the four Suffolks are wethers while two are rams. Throughout the entire book the conclusions about the effects of domestication are mostly based on the data from one Soay and one Shetland ram. What a terrific responsibility to place upon two rams differing in breed and age, that of being truly representative of wild sheep in general!

Where the numbers in each group are as small as they are in most tables in this book, and especially where each group is used as evidence on two

or more different questions, such as age or breed or sex, there is no possibility of estimating in an objective way the significance or non-significance of observed differences. The author expresses considerable confidence in having selected representative individuals for this analysis. It would be pleasant to join him in this confidence, but nowhere in the data themselves does there seem to be any objective way of testing how well this confidence is justified. Indeed on page 357 we find:

> The attempt to pick the leanest and fattest, or the best and poorest, out of a pen of sheep purchased in the market, such as was tried when the investigation was first started, was not very successful, as the differences in degree were not big enough to obtain those wide differences which are necessary for comparison when a limited number of animals are being used.

Nevertheless, on pages 372 and 301 are tables and sections purporting to show variations in proportion due to state of fatness. The data consist entirely of percentages from one lean wether to one fat wether. It is not legitimate to criticize the author for the scantiness of the data since he expressly states that the purpose of the work was to give a general survey rather than conclusive proof for any one point. However it is our distinct loss that so brilliant and tireless a worker did not have available sufficient material to afford the number of replications necessary to check his conclusions and to delimit his generalizations. In general the reviewer finds himself inclined to agree with the author in most of the conclusions drawn, but the inclination thus to agree is based only slightly on the data presented here and mainly on accessory evidence from other data or upon the apparent reasonableness of the logic.

Throughout the book the author has sought to explain everything in terms of as few causes as possible. The history of biological problems and the complexity of the subject investigated makes it seem unlikely that the explanation stated can often be a complete one. Particularly is this the case with the attempts to ascribe to the hormones of the pituitary glands nearly all the still unknown effects of hormone control over growth. Again the author tries to reduce as much as possible of the differences between different kinds or types or breeds of animals, and also between different organs or tissues in the same animal, to degrees of "early maturity" and "late maturity." In part this is an expansion of the biological generalization that "ontogeny recapitulates phylogeny—with variations." Parts of the hormone discussion are not physiologically explicit, as in the continued references to "generative ferment" which seems to be some kind of a hypothetical hormone and which on some pages seems to be identified with the secretion of the anterior pituitary. Yet on page 392 there are the words "—dependent on some other factor, possibly vitamine supply or its metabolic derivative 'generative ferment.'" This term is always used in quotation marks to indicate its entirely hypothetical nature.

Genetics of Mutton

The references to genetic subjects and inheritance occupy only a small part of the book and in general are in the nature of asides. The genetic views expressed were widely held by many biologists, even by geneticists themselves, as recently as the period from 1912 to about 1920 but at present are rather obsolete. Among such views may be listed (1) the idea that gene mutation is limited to large distinct changes, (2) the belief that there is some fundamental distinction in the inheritance of discontinuous and continuous variability, (3) the belief in a real difference between "qualitative" and "quantitative" characters, the former Mendelizing but the latter being inherited in some other way, and (4) failure to appreciate the tremendous store of genetic variance maintained in all cross-fertilized species and breeds by the Mendelian mechanism

of inheritance. (In most such species even intense selection may be continued for many generations before this supply of genetic variance is even approximately exhausted.) That the author's faith in the direct effect of environment in producing hereditary changes which will make the following generation more adapted to that environment is unshaken is shown in the following sentences (pages 251-2):

> As a general conclusion it is considered that the main method of evolution is directive and not dependent on the large mutations which occur. Developmental changes in the animal, directed by the environment form the main basis of evolution, and although mutations give rise to new characters it is not in these characters that the main difference between species lie but rather they consist of developmental changes. Thus in our opinion selection in a suitable environment is the method by which the evolution of domestic animals for productive purposes has proceeded; that is, by the continuous variability in the animals breeding on definite lines induced by the environment and not by the accumulation of separate mutations, which may and do arise independently in the germ plasm.

One can have little quarrel with his belief that selection is the chief means by which the breeder can control or direct evolution (the effectiveness of such selection perhaps being much affected by the use made of progeny tests and inbreeding systems), but it is unfortunate that he thinks it necessary to tie up this practical procedure with the theoretical postulate that the changed environment of itself induces an adaptive change in the germ plasm.

In summary this book may be fairly described as comprehensive and full of detailed information with much discussion *pro* and *con* of various theories to explain the findings. Many of the conclusions are not supported by sufficient data to establish them beyond reasonable doubt. In many cases alternative hypotheses would explain the observed results equally well so far as the present data go. On account of its extensive review of literature and the stimulating nature of the author's own data, theories, and conclusions, the book will furnish an excellent starting place for those wishing to investigate any subject in this field.

JAY L. LUSH.
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Elizabethan Views on Sex in Plants

EDITOR, JOURNAL OF HEREDITY:

I was very much interested in the paper of Professor Praeger in the current issue of the Journal. It is very difficult to discover what Shakespeare really meant. In England during the Sixteenth and Seventeenth Centuries, the word "bastard," applied to a flower, was an epithet pure and simple. It was not the same as the German "Bastard" or hybrid. It only meant that the speaker did not like the flower in question. The term "bastard" was generally applied to common or wild varieties, especially when they were contrasted with cultivated forms. Perhaps Shakespeare was only punning on this word to add spice to the lady's conversation.

Bacon, as you showed, certainly foretold the improvement of plants by hybridization. However, much of his talk on sex in plants was quite confused. To speak of male and female plants was quite common in his day but it did not necessarily refer to sex. If two closely related species grew in the same region, the larger species was called male and the smaller female. Matthiola (1566) even mentions four "Geschlecht" or four varieties of a species (See male and female mandrake).

CONWAY ZIRKLE.
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WORLD MIGRATIONS

A Review

INTERNATIONAL MIGRATIONS, VOL. II, *Edited by* WALTER F. WILLCOX. 1931, pp. 715. $7.00. National Bureau of Economic Research, Inc., N. Y.

THE present volume completes the two volume* inquiry into international migration, a project which originated with the Social Science Research Council and which has been brought to consumation under the auspices of the National Bureau of Economic Research and through the efforts of the International Labor Office and the International Statistical Institute. Whereas the first volume presented the official statistics, the present consists of critical interpretations of the respective statistics of the chief emigrant and immigrant countries. The editor contributes a chapter on world population increase and another on American immigration. The other chapters, prepared by international scholars equipped to handle the matter, deal with Canada, Argentina, Brazil, Australia, New Zealand, France, Great Britain, Ireland, Scandinavia, Germany, Austria, Hungary, Italy, Russia, Mexico, India, Japan, and the Jewish people.

The all-inclusive nature of the present volume makes it impossible to appraise the various parts of this volume. We shall therefore merely try to suggest its contents and leave to individual readers the problem of critical evaluation.

Professor Willcox's study of population growth during the last three centuries indicates that the relative number of persons of European blood has increased considerably and that in modern times man has become less tropical in habitat. A century from now, if one excepts the rapidly increasing Slavs, a different conclusion will probably be in order. "In less than three centuries the population of the earth has nearly quadrupled and the rate of its increase accelerated until about 1900, but slackened after that date. Asia seems to have gained at first more rapidly than any other continent; between 1650 and 1800 its proportion of the world's population increased by 10 per cent. Since 1800 the proportion has fallen and it now holds about the same relative position that it did in 1650. Europe and the Americas lost ground relatively between 1650 and 1750, but in the following two centuries North America gained 8.0 per cent, Europe 6.1 per cent, and South America 3.7 per cent, or together 17.8 per cent, these shifts having been offset by a relative loss during the same period of 11.2 per cent in Asia and 6.8 in Africa. Africa has lost in relative population almost without a break, its present proportion being little more than one-third of what it was in 1650."

Immigration and Population Growth

Professor Willcox examines at some length the theory advanced by F. A. Walker that foreign immigration into the United States constituted not an *addition* to, but a *replacement* of the native population. Walker had contended that foreign immigration threatened the American standard of living and American sensitivity and thot consequently the natives limited the size of their families. Willcox concludes "that the population of each part of the United States grew in response to its own conditions at the time, and that the apparent uniformity in the rates of growth before 1845, upon which Walker rested his theory, disappears as soon as the different parts of the country are

*Concerning Vol. I, see this journal, Vol. 21, 1930, p. 271.

studied separately." Willcox believes that in light of the meagre statistical evidence available it will never be possible to determine accurately the effect of immigration on the birth rate. The truth lies in between the extreme view of Walker and the extreme anti-Walker view. Willcox surmises "that the approximate uniformity in the rate of increase during the early decades of the last century was due largely to the cheapness and accessibility of good agricultural land on the frontier."

The Immigrant-Receiving Countries

The treatments of the chief immigrant-receiving countries are primarily analyses of the composition and the changing trend of the immigrant streams flowing to these countries rather than of the part played by immigration in the national developmental policies of these countries. Four-fifths of the immigrants who go to Brazil and Argentina are Latins, whereas those going to Canada are chiefly from Northern Europe. R. H. Coats states that steady immigration will solve the tax and railway problems of Canada and that an influx "of 200,000 a year would assure Canadian prosperity." France alone of the countries of West Europe has experienced a net inflow of immigrants from her Latin neighbors and Poland. Inasmuch as the French birth rate has been low for over a half century, the immigrant element has become an integral part of the economy of France.

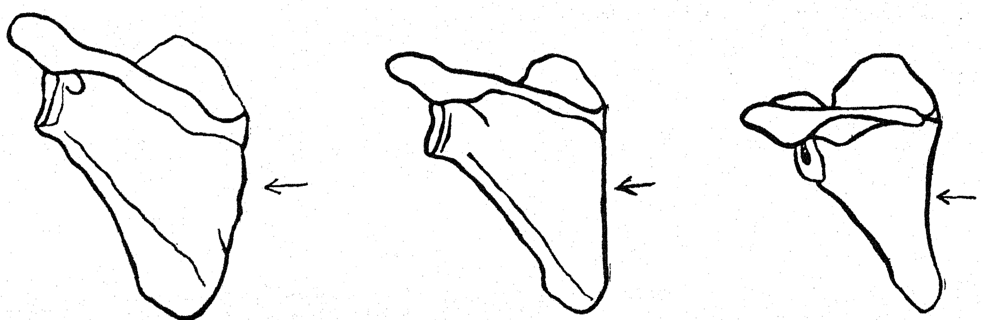
The analyses of migration from the leading emigrant countries of Europe and Asia are quite valuable in that they deal with both the causes of emigration at different periods and with the social and the economic effects of such emigration. The accuracy of the migration statistics of some of these countries is examined in detail. Concerning Jewish imigration, chiefly from Russia, Poland and Lithuania, Dr. Hersch comments as follows: "No people have changed so profoundly as the Jews under the influence of world migration." The largest proportion of the Jews, whose migration became important only near the close of the last century, have entered the United States, with Canada and Argentina next in order. At present more Jews live in the United States than in any other country. While emigration has not reduced the Jewish population of Europe, it has drained off nearly all the natural increase. Migration has greatly elevated the economic scale of the Jew, has changed him from a villager into an urbanite, and in so doing has secularized Jewish religious life, engendered a Jewish lay culture, and shunted many from petty trades into occupations filled by physical laborers and salaried workers. These and many other points are brought out in detail by Dr. Hersch. Equally detailed are the essays of many of the other writers.

While some criticisms will be directed against certain of the interpretations presented in this volume, students of population and history will be grateful to Dr. Willcox who has ably edited both works and to the National Bureau and the cooperating scholars who have made both volumes possible. No reference library can afford to be without them.

Joseph J. Spengler.

University of Arizona.

AN INDEX OF HUMAN FITNESS



THREE TYPES OF SHOULDER BLADES—A POSSIBLE "FITNESS INDEX"

Figure 9

Human shoulder blades (scapulae) have been found to be of three types, classified as convex, straight, and concave. (See arrows above). It is found that there are twice as many straight and concave scapulae among young individuals and twice as many convex scapulae among those sixty years old or over. Scapula-shape apparently does not change during life, so that this difference of distribution at various ages appears to be due to a higher death rate among those having straight and concave scapulae. Thus this character, of no conceivable survival value itself, seems to be of interest as a statistical index of human fitness.

IT would greatly simplify a program of eugenics to have a simple and objective way of separating the better adapted sheep from the less adapted goats of the human family. In the early stages of preventative eugenics there is a large population that even the most confirmed sentimentalist would have some difficulty in convincing himself to be the cream of the race. Beyond such a minimum, definitions of fitness would vary with different people. Thus an index of fitness, based not on human evaluations, but on actual survival is of considerable interest.

For a number of years Dr. W. W. Graves of St. Louis University Medical School has been accumulating evidence that the shape of the shoulder blades is of possible importance as such an index. The scapula or shoulder blade is tied to the back by a large number of muscles. It does not articulate with any of the other bones of the skeleton. The edge of the scapula lying nearest the backbone varies in outline; some scapulae have this edge concave in outline, some are straight, and some are definitely convex. That the contour of this edge should be an index of biological fitness is an astonishing fact quite at variance with most of our concepts of heredity. Nevertheless the evidence that Dr. Graves has accumulated seems to put this character in a unique category as a diagnostic tool. Writing in the *Eugenics Review** (London), for October, 1931, Dr. Graves has summarized his findings to date. The first point about the scapula is that the shape appears to be definitely determined very early in embryonic development. As early as the twelfth week, foetal scapulae are definitely convex or concave. The shape does not appear to vary during later life. It does not appear to be influenced by diet or by the diseases incident to the individual history. Scapula type is a strongly inherited characteristic.

Three Kinds of Individuals

More or less asymmetry in scapula type is found in the same individual. Thus individuals are found with one scapula straight and the other concave, or with one straight and the other con-

* One questions the wisdom of the cover-title under which this article appears: *Stigmata of Degeneracy*. With a correlation considerably less than 1, normal people, (even the highly gifted!) who bear such a "stigma" are not likely to have their enthusiasm for eugenics increased by such a misleading, almost insulting, misuse of terms, not in any way implied in the text of the article.

vex, etc. The straight and concave types are called scaphoid types in contrast to the convex type. The mixed types are classified as scaphoids, as they have one scaphoid component.

The evidence on which Dr. Graves bases his claims for the diagnostic value of this characteristic is of two kinds. One is the distribution of scapula types at different ages; the other is the distribution of scapula types in different components of the population classified according to degree of health. The conclusions are as follows:

(a) The scaphoid types (straight, concave, and mixed) predominate in frequency of occurrence in the young and relatively young, and the convex types predominate in frequence of occurrence in the old and relatively old, regardless of the nature of the material (excellently, well, or poorly adapted).

(b) The relative percentages of convex and of scaphoid (straight concave, and mixed) types in all age periods are dependent primarily upon the age and secondarily on the nature of the material.

(c) The percentages of the convex types increase while the percentages of the scaphoid types (straight, concave, and mixed) decrease in successive age periods from childhood to old age, regardless of the nature of the material (excellently, well, or poorly adapted).

(d) The percentages of the concave and mixed (scaphoid) types decrease more rapidly than the straight scaphoid types from childhood to old age, regardless of the nature of the material (excellently, well, or poorly adapted).

(e) In poorly adapted groups, in similar age periods, there are more scaphoid types and fewer convex types than in well-adapted groups.

(f) Investigations in explanation of the age incidence of scapular types in the various directions herein outlined, including follow-up observations on a relatively large number of individuals during periods of growth, maturity, and senescence, have disclosed no process or circumstance capable of changing one type of scapula into another; hence there remains but one explanation of the age incidence of scapular types: the scaphoid types (straight, concave, and mixed) are more frequently found than the convex types in the unduly disease-susceptible, the plus-potentially sick, the shorter-lived of the race.

The preceding conclusions are based on a study of about 60,000 people. This is supplemented by a recent investigation of 10,000 more individuals ranging in age from 6 to 15 years, and of persons over 60 years of age. The approximate distribution among the group 6 to 15 years is, scaphoid type, 65%, convex type, 35%. Among the group 60 years and over, scaphoid type 35%. convex type 65%. This is a striking indication that on the average the convex type do have an inherently better chance of survival. That the character of itself has any survival value would be questionable. That it should be so definitely linked with other characters conferring on the individual greater fitness to survive in this complicated Twentieth Century is a most interesting fact. A most important research would be to discover whether the association were with general fitness or with some specific ability to resist disease in general, or diseases of a certain type.

Scapula-Type and Health

Dr. Graves also investigated the distribution among various health types. He took as the average healthy group high school and university students and graduates, and members of the A. E. F. Among the less healthy group were tuburcular, insane, feebleminded, and prisoners. "Comparable numbers in similar age groupings showed approximately 1½ to 2¼ times as many convex scapula types in the supposedly healthy as in the known-to-be sick group."

Dr. Graves emphasizes that this is purely a statistical measure of fitness. He found many scaploid individuals of an advanced age, evidently in excellent health and well adapted to their environment. Thus the possession of shoulder blades of the scaphoid type is not by any means to be construed as evidence of inferiority, nor could it be considered to be an indication for sterilization, as the production of mental defective children might be. A determination of the nature of the other survival characters this peculiarity is associated with would seem to be of the utmost importance; it should be undertaken without delay.

EARLY REFERENCES TO THE WHITE INDIANS OF PANAMA

NORMAL AND WHITE-INDIAN CHILDREN

Figure 10

In stature and facial characteristics the albinos resemble the other members of the tribe. The reduced tolerance to light is indicated by the squinting of the two albinos. Photographs by R. O. Marsh. Courtesy of Science Service.

THE press in recent years has carried extensive accounts of native white people in Panama. There is one phase of the situation which has not received the emphasis that it deserves. It seems that albinism has persisted in that region for a long time. For some unaccountable reason the story of finding white people has had a romantic attraction for early American explorers. Rumors were common among the first adventurers in America. Humboldt writing about 1700 speculated on these rumors as follows: "Were they albinos such as have been found heretofore in the Isthmus of Panama?" Just what he is referring to is not certain but a very clear account is recorded by Wafer in 1681 which has bearing on these rumors. Wafer was a buccaneer who, after the manner of pirates of the time, wrote a book when he retired from his adventures. The necessities of his profession forced him into the territory of the white Indians. The easier crossing at Panama was so strongly held, that the buccaneers made use of the crossing from the Gulf of Darien. While crossing, Wafer was wounded and forced to stay behind for some months among the Indians. He had an obvious gift for observation and his notes, reprinted below are the best record even today of the nature of the white Indians. His account opens with description of the normally pigmented Indians:

"Their natural complexion is a copper-colour, or orange-tawney; and their eye-brows are naturally black as jet. They use no art to deepen the colour either of their eye-brows, or the hair of their head; but they daub it with oil to make it shine; for like other Indians they anoint themselves all over, whether for beauty to make the skin smooth and sleek, or to supple it and keep it from parching, or to hinder too much perspiration in this hot country, I know not.

"There is one complexion so singular, among a sort of people of this country, that I never saw nor heard of any like them in any part of the world. The account will seem strange, but any privateers who have gone over the Isthmus must have seen them, and can

Photograph by R. O. Marsh

THREE WHITE INDIANS

Figure 11

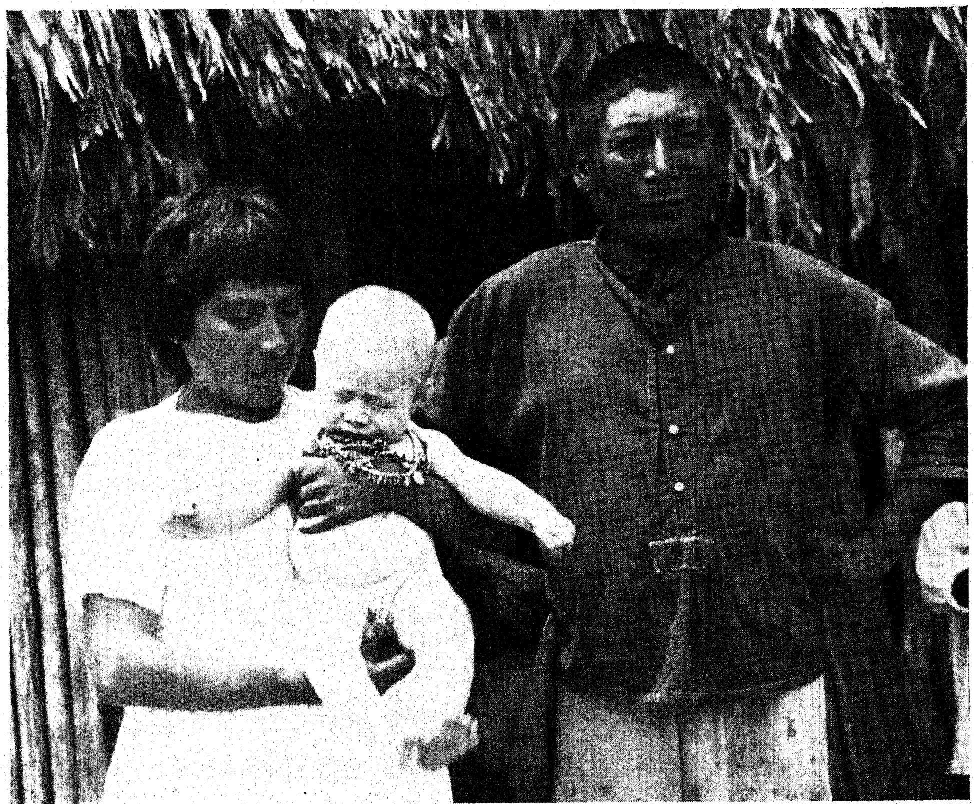
These were the children brought to the United States by Dr. Marsh. The rediscovering of albinos among these Indians raised a storm of controversy. Wafer's detailed account of them in 1681, gives as long a record of human albinism as is in existence today.

attest the main of what I am going to relate; though few have had the opportunity of so particular an information about these people as I have had.

"They are white, and there are of them both sexes; yet there are but few of them in comparison of the copper-coloured, possible but one to two or three hundred. They differ from the other Indians chieflly in respect of colour, though not in that only. Their skins are not of such a white as those of fair people among Europeans, with some tincture of a blush or sanguine complexion; neither yet is their complexion like that of our paler people, but 'tis rather a milk-white, lighter than the colour of any Europeans, and much like that of a white horse.

"For there is this further remarkable in them, that their bodies are beset all over, more or less, with a fine short milk-white down, which adds to the whiteness of their skins: For they are not so thick set with this down, especially on the cheeks and forehead, but that the skin appears distinct from it. The men would probably have white bristles for beards, did they not prevent them by their custom of plucking the young beard up by the roots continually; But for the down all over their bodies, they never try to get rid of it. Their eye-brows are milk-white also, and so is the hair of their heads, and very fine withal, about the length of six or eight inches, and inclining to a curl.

"They are not so big as the other Indians; and what is yet more strange, their eye-lids bend and open in an oblong figure, pointing downward at the corners, and forming an arch or figure of a crescent with the points



EVIDENCE OF MENDELIAN SEGREGATION

Figure 12

Normally pigmented parents and a "white" child among the Indians of Panama. Photograph by R. O. Marsh. Courtesy of Science Service.

downwards. From hence, and from their seeing so clear as they do in a moonshiny night, we us'd to call them Moon-ey'd. For they see not very well in the sun, peering in the clearest day; their eyes being but weak, and running with water if the sun shine towards them; so that in the day-time they care not to go abroad, unless it be a cloudy dark day. Besides they are but a weak people in comparison of the other, and not very fit for hunting or other laborious exercise, nor do they delight in any such. But notwithstanding their being thus sluggish and dull and restive in the day-time, yet when moon-shiny nights come, they are all life and activity, running abroad, and into the woods, skipping about like wild-bucks; and running as fast by moon-light, even in the gloom and shade of the woods, as the other Indians by day, being as nimble as they, though not so strong and lusty.

"The copper-coloured Indians seem not to respect these so much as those of their own complexion, looking on them as somewhat monstrous. They are not a distinct race by themselves, but now and then one is bred of a copper-colour'd father and mother; I have seen a child of less than a year old of this sort. Some would be apt to suspect they might be the off-spring of some European father; But besides that the Europeans come little here, and

have little commerce with the Indian women when they do come, these white people are as different from the Europeans in some respects, as from the copper-coloured Indians in others. And besides, where an European lies with an Indian-woman, the child is always a Mostese, or tawney, as is well known to all who have been in the West Indies; where there are Mostesa's, Mulatto's, etc., of several gradations between the white, and the black copper-colour'd, according as the parents are; even to decompounds, as a Mulatto-Fina, the child of a mulatto-man, and Mostesa-woman, etc.

"But neither is the child of a man and woman of these white Indians, white like the parents, but copper-colour'd as their parents were. For so Lacenta told me, and gave me this as his conjecture how these came to be white. That 'twas through the force of the mother's imagination, looking on the moon at the time of conception; but this I leave others to judge of. He told me withal, that they were but short-lived.

"Both these and the copper-coloured Indians use painting their bodies, even of the sucking children sometimes. They make figures of birds, beasts, men, trees, or the like, up and down in every part of the body, more especially the face; But the figures are not extraordinary like what they represent, and are of different dimensions, as their fancies lead them." *(Quotation from:* A New Voyage and Description of the Isthmus of America. By Lionel Wafer. Pages 133-136. Reprinted from the original edition of 1699; Edited by George Parker Winship, Librarian of the John Carter Brown Library. 1903.)

It appears then that the strain of albinism, if these are albinos, was as prevalent some centuries ago as it is today. If this is true, it is beyond the possibility of speculation to estimate the number of centuries that albinism has persisted in Panama. Certainly it is a long clear record of human albinism.

HARRY V. HARLAN.

U. S. Department of Agriculture.

Book Advertisements and A-G-A Book Service

In this issue appear two advertisements of books covering various phases of the field of interest of members of the American Genetic Association. It is believed that such announcements of important new books and of standard works will be a real addition to the JOURNAL. Furthermore, the income from the sale of advertising space is much needed to enable us to keep up with our new publication schedule, *on time!*

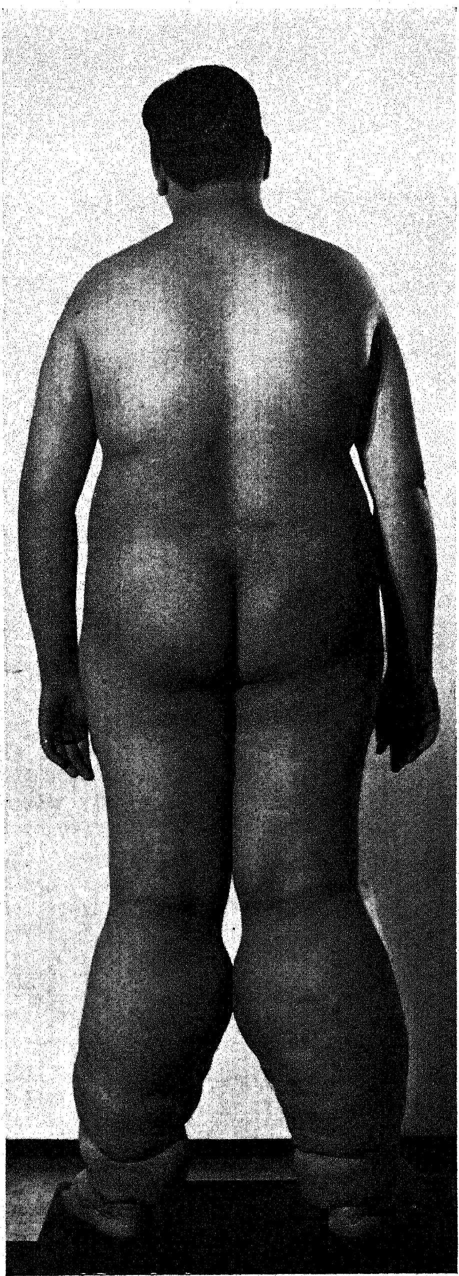
In these times especially, to continue to sell advertising space in the JOURNAL, we must demonstrate to publishers, by books actually sold, that advertisements in the JOURNAL are a good investment. *Under no other conditions will we continue to receive the advertising patronage of publishers.* Thus in the purchase of books advertised in the JOURNAL OF HEREDITY, and even of books reviewed in our JOURNAL, the mention of the fact that the announcement of the book *"was seen in the* JOURNAL OF HEREDITY" is invaluable. Better yet, members are urged to place orders for such books through the *A-G-A* Book Service. The book will cost less, and such purchases are the most convincing possible evidence that the JOURNAL OF HEREDITY is a desirable medium for advertising genetic, eugenic and biological books. Members are urged to bear this in mind in ordering books.

HEREDITARY ODEMA OF THE LIMBS

FORMERLY geneticists were satisfied to describe the end-character produced by a gene, and there was little interest in the way genes go about getting their tremendously complicated final results. Today our ignorance of this problem is still profound, but much pioneer work is being done in genic physiology, and we are learning more and more about how the genes go about putting together living things. In man, of course, genetic physiology has made relatively little advance because it is not feasible to study characters "along the way." A recent report of a histological study of Milroy's disease is interesting in this connection as it enables us to speculate on the way the end-result, a tremendous odema of the lower limbs, is brought about.

Milroy's disease is an hereditary condition, in which the lower extremities become greatly swollen in early adult life (Figure 13). While those affected are incapacitated, general health is not affected, and victims of the condition frequently live to a ripe old age. In a case reported by Dr. Johnson McGuire and Dr. Pearl Zeek (*Jour. Amer. Med. Assoc.*, March 12, 1932, page 870), both the father and the grandfather of the patient under consideration were also affected. An operation was attempted with the purpose of facilitating anastomosis between the deep and the superficial lymphatics of the legs. The pathological examination of the tissue removed in this operation showed that much of the odematic fluid was in pools without definitely defined boundaries suggesting that the tissues had literally been torn apart by the hydrostatic pressure of the body fluids.

Conclusions from a single case are, of course, hazardous, but considering the fact that the odema is limited to those parts of the body where the hy-



A CASE OF MILROY'S DISEASE

Figure 13

This form of odema of the lower limbs is definitely hereditary. It is disabling, but those who have the condition frequently live to a considerable age. (Photograph, courtesy of Dr. Johnson McGuire and Dr. Pearl Zeek.)

drostatic pressure is greatest, one is led to wonder whether the condition may not be caused by a weakness of some of the tissue elements, which are not able to withstand this greater pressure. When we consider the radical physical and physiological rearrangements involved in a change from a horizontal position to an upright, it seems remarkable that so many successful adaptations could be made. Just as the "creeper" gene in fowls is a reversion to a reptilian characteristic, so one wonders whether victims of Milroy's disease are not really suffering from a gene that is adapted to the development of tissue "normal" for a quadruped, but not sufficiently rugged to withstand the doubling of this pressure incident to an upright posture. This possibility might be tested by keeping incipient sufferers from Milroy's disease on all fours, so that the hydrostatic wear and tear on the tissues would be halved, but whether all the other adaptations to the vertical could be "sold" on such an atavism might be an even more serious question.

Beet Breeding in Russia

One element of the Five Year Plan that is producing large results, on paper at least, is the Russian Sugar Trust, whose extensive publications cover all phases of beet sugar production. The Ukraine has long been an important beet-sugar center. Plans for the next few years contemplate expanding the acreage in this region to equal that of the rest of Europe and of America combined. Whether this plan can be realized depends for one thing on the amount of seed available. This has resulted in an intensive development of stations for the study of the genetics of the sugar beet. Reports of this research appear in the *Bulletin of the Sugar Trust,* published in Kiev. Many of the articles have brief, sometimes enigmatic, summaries in English. A recent study by D. Savizky attempts to separate the relatively variable and relatively invariable characters in the beet. The characters showing a marked variability were as follows: the size of the assimilation surface; the weight of the root; the weight of the foliage; the size of the leaf; the productivity of units of green tissue. The characters subject less variation in comparison with the above are: the number of leaves; the absolute length of the petiole; the relative length of the petiole; the absolute length of the leaf blade; the shape of the leaf; the percentage of sugar; the length of life of leaf. Correlation of characters showing a high phenotypical variability are thus of little genetic significance.

The Sugar Trust has recently issued an English pamphlet summarizing the accomplishments to date in beet seed production. The many illustrations give the impression that the industry is rapidly becoming "mechanized," which magic term is used many times in the text. The tables showing increases in yield and in sugar content indicate, strange as it may seem, that this is not interfering with the successful application of biological principles.

The United States, although it produces some two billion pounds of beet sugar per year, raises practically none of its own seed. Before the war the German and Polish seed, our chief sources of supply, came largely from Russia, the seed farms being located in the region of the Kiev. At the present time American seed supplies are obtained from Germany, Russia and France, the seed actually originating in these countries. The Russian eagerness to enlighten the Western world on the success of the five-year plan in beet seed production is doubtless motivated in part by a natural desire to regain this lost market.

EFFECTS PRODUCED BY AGING AND X-RAYING EGGS

of *Drosophila Melanogaster*

J. T. Patterson, Weldon Brewster, and A. M. Winchester
Department of Zoology, University of Texas

IN making a study of the effects of X-rays on the production of mosaics in Drosophila, the senior author found that if the eggs were "aged" in the females before the flies were treated and mated, the number of exceptional or non-disjunctional individuals appearing in the F_1 cultures was significantly increased over that found in cultures derived from eggs that had not been aged before the females had been treated and mated. In a series of experiments in which five different groups of virgin yellow white females were aged one, three, five, seven, and nine days, respectively, and then X-rayed and crossed to eosin-singed males, the percentage of exceptional flies, due to primary non-disjunction, was found to gradually increase up to and including the seventh day, but on the ninth day there was a decided drop in the percentage (Journal of Heredity, Vol. XX, p. 266). In the control series no such increase occurred. It was therefore evident that the observed increase in the experimental series must in some way be related to the aging process plus the effects of irradiation.

In view of these facts, it was decided to make a series of studies on the effect of X-raying eggs that had been aged in the ovaries of the females. The two junior authors have carried on the major part of this investigation; Mr. Brewster studying the effects of aging on the production of eggs and on fertility, together with the effect of X-radiation on the fertility, and Mr. Winchester making an investigation of the effect of aging on the production of primary non-disjunction. The senior author has summarized his data that has a bearing on the question of the effect of aging on the production of breaks in the X-chromosome.

In our separate investigations we have used the same general methods for handling the flies and for giving the treatments. In order to "age" the eggs, the virgin females were kept under crowded conditions (in all except one experiment) in one by four inch vials, which contained the agar-banana food. It was found that this method inhibited the virgin females from depositing many eggs, especially during the first six or seven days of the aging process. Whenever a given group of females had reached the desired stage, half of the flies were X-rayed and mated, and then placed in half-pint bottles containing food rich in yeast. The other half of the group was mated without treatment and used as controls. In most of the experiments about fifteen or twenty pairs of flies were placed in each culture bottle. The cultures were kept at room temperature which varied between seventy and eighty degrees, with an average of about seventy-five degrees.—*J. T. P.*

I. The Effect of Aging on Egg Production and Fertility, and the Effect of X-Radiation (Brewster)

Egg Production by Virgin Females

In order to have a basis for the study of the effect of X-radiation on aged eggs, it was necessary to obtain information concerning the rate at which mature eggs were formed and retained in the ovaries of the virgin females. The first experiment was

designed for this purpose. Ninety wild-type virgin females were collected within six hours after the culture bottles had been freed of all flies, and placed separately in vials, each of which contained a paper spoon holding banana food. The spoons were removed and fresh ones added at the end of each twenty-four hour period. Counts were made each day of the eggs deposited on the food for a period of ten days. Each day the ovaries of nine females were carefully dissected out and the number of fully formed or mature eggs was counted and recorded.

The same procedure was followed in handling the ninety females used as controls, except that these females, immediately after they had been collected from the culture bottles, were mated to wild-type males.

In Table I are given the results obtained in this experiment, both for the experimental and control series. During the first two days the virgin females deposited no eggs, but on the third and fourth days a few eggs were laid. Beginning with the fifth day, the virgins laid an increasingly larger number of eggs up to and including the eighth day. On this day the remaining twenty-seven females deposited a total of one hundred and twenty eggs, for an average of 4.4 eggs per fly. The ninth and tenth days showed a decrease in the number of eggs laid.

As shown in the last column of the table (top half), very few mature eggs were found in the ovaries of the nine females dissected at the end of the first day, the average being 0.7 of one egg per pair of ovaries. After the first day the average number of eggs per pair of ovaries gradually increased throughout the rest of the ten day period, with the exception of the eighth day. The eighth day shows a distinct drop in the average number of eggs, but this can in part be explained by the fact that the virgin females deposited on this day the largest average number of eggs for any of the ten days.

The data for the control series (lower half of table) stand out in sharp contrast to those of the experimental series. The females here were mated immediately after collection, so that even as early as the second day the average number of eggs laid was 39.8 per female. With but slight variations, this average increased each day and reached the highest point on the ninth day. The average number of mature eggs found in the ovaries of the dissected specimens remained practically constant after the first day, the average being 9.8 eggs per pair of ovaries for the last nine days. This average for the corresponding days for the virgin females was 68.6 eggs. The total number of eggs laid by the virgin females during the ten day period was three hundred and seventy-five while the mated females laid a total of 23,371. The total number of mature eggs found in the ovaries of the dissected females was 5,570, and only 821 were obtained from the ovaries of the mated females.

It is evident that virgin females "store" up a comparatively large number of eggs in their ovaries, and deposit very few, while mated females do just the reverse. The mated female lays her eggs practically as fast as they are formed in her gonads. It must be remembered that in this particular experiment the virgin females were not "crowded," and were therefore carrying on their activities under favorable conditions. Had crowding many virgin females in each vial been resorted to, it would have been found that fewer eggs still would have been laid, and a corespondingly larger number would have been retained in the ovaries. It has been found that under the condition of crowding, a female may retain as many as a hundred and twenty mature eggs. There is obviously a limit to the number of mature eggs that can be held in the gonads, so that one of two things will happen, either the fly will deposit the eggs after seven or eight days, or some of the eggs will disintegrate.

The Effect of Aging on Fertility

In order to determine the effect of aging on fertility of the stored eggs, ninety wild-type virgin females were divided into five groups of eighteen each, and aged one, three, five, seven, and nine days, respectively. At the end of each period of aging the virgins were mated and one pair placed in each food vial. The food spoons were changed daily for the first three days, and at the end of each day the eggs deposited on the food were counted and the spoon was transferred to a vial containing moist cotton. The eggs were then incubated at a temperature of 27 degrees centigrade. At the end of thirty-six hours the number of eggs that had hatched was determined by counting the number of undeveloped eggs.

The control series was treated in a similar manner, except that the females were mated immediately after being collected, and the counts of the eggs laid and hatched were not started until the beginning of the corresponding period of aging of the experimental series. For example, the eggs laid by the second group of mated females during the three days following mating were discarded, and the counts then made on the eggs laid during the three succeeding days.

The results obtained in this experiment are shown in Table II. The number of eggs examined for the experimental series was 10,583, and that for the control series was 11,424. An analysis of the data reveals the fact that the percent of eggs that developed was consistently lower on the first day of collection than on either of the two succeeding days. On the second and third days of collection the percentages of fertility were approximately the same as for those of the corresponding days of the controls. The difference in percentage of fertility of the eggs aged one day and the controls is considered of no significance, but a very decided decrease in this percentage is found for the first days for eggs aged three, five, seven, and nine days respectively.

Since, as the control series shows, the average percentage of fertility of females that were not aged was 92.9, it was necessary for comparison to correct the percentage of fertility found in the experimental series. This was done by dividing the percentage of fertility in the experimental series by that for the corresponding days of the control series. On this basis a corrected percentage of 100 would mean that the aging process has had no effect on fertility. The corrected percentages are given in the last column of the upper part of the table. It will be observed that for the first day the eggs from females aged one day, the percentage of fertility drops to 93.3, and for the second day it was normal, while the third day shows a slight decrease. For females aged three days, the percentage decrease amounts to 22 per cent for the first day, with insignificant decreases for the second and third days. For females aged five days, the first day shows a decrease of nineteen per cent, the second day only one per cent, and the third day was normal. Finally, for the two groups of females aged seven and nine days, the first day shows a decrease of twenty-one and twenty per cent, respectively. The second and third days for each group were normal.

From this, one may conclude that aging the female, and hence the contained eggs, significantly decreases the percentage of fertility of the eggs laid during the first day after the close of the aging period, but has little or no effect on fertility thereafter. This decrease may be due to the fact that the older of the retained eggs undergo degeneration, although it is possible that it may be due to a failure of the inseminating mechanism to function properly.

The Effect of X-rays on the Fertility of Eggs That Have Been Aged

For this test the ninety wild-type virgin females were first handled exactly as were those for the preceding

experiment. They were divided into five groups of eighteen each, and aged one, three, five, seven, and nine days, respectively, and then X-rayed with a dosage of 1325 *r* units. After treatment the females were mated to wild-type males and one pair placed in each food vial. Their subsequent treatment was the same as for the last experiment. The control flies of the preceding test served also as controls for this experiment. In correcting the percentages to obtain the effect of X-rays on aged eggs, it was necessary to compute the percentage of eggs that would develop under normal conditions and the percentage that would develop when the eggs were aged for each of the five respective periods in the preceding experiment, and then calculate the additional effect of X-radiation.

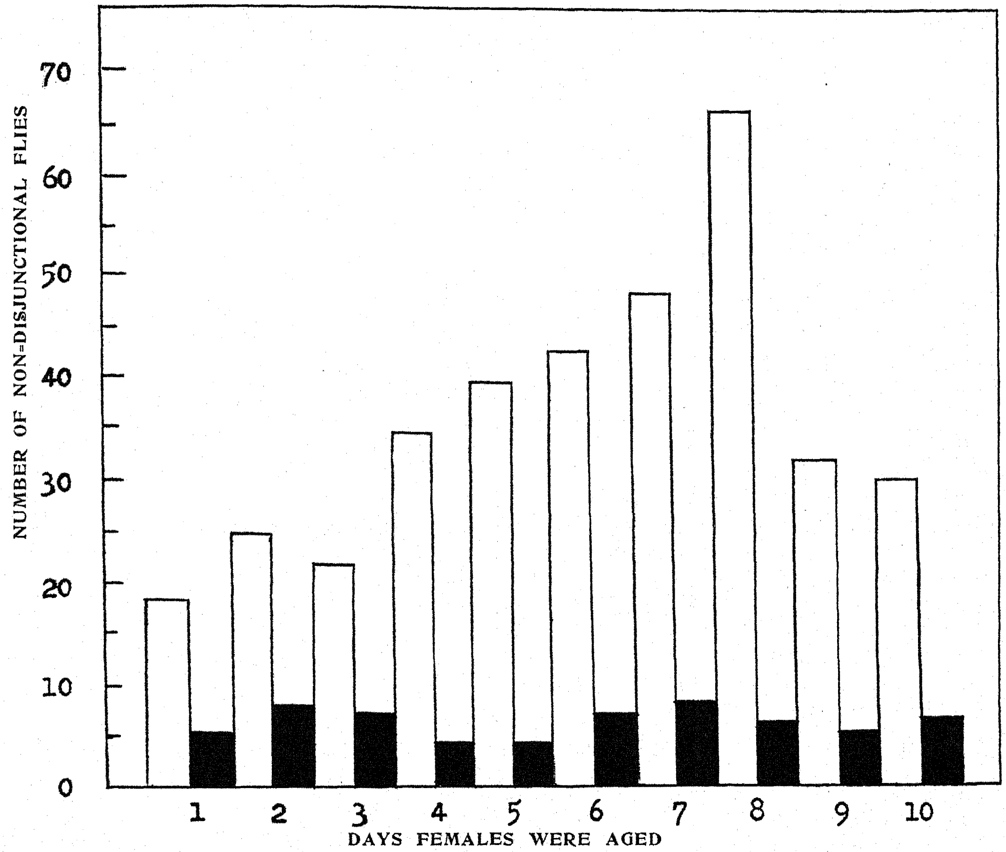
In Table III are given all of the data, and the last column shows the corrected percentages as determined by the method of calculation as just stated. A percentage of 100 here means that X-radiation had no additional effect on fertility above that resulting from the aging process. The data obtained show that the fertility of the eggs deposited on the first day of each of the five periods of aging was distinctly lowered as a result of the treatment. The greatest effect is seen in the group that had been aged seven days, where the percentage drops to 28.3. The eggs laid on the third day of each period shows no decrease in fertility, but the average percentage decrease for those laid on the second day was 5.6. This is probably to be accounted for by the fact that not all of the mature eggs present in the ovaries had been deposited on the first day of laying. It is concluded that the effect of X-radiation plus aging in decreasing the percentage of fertility is manifested primarily on the fully formed eggs. If there is an effect on immature eggs it is not apparent.

II. The Effect of Aging the Egg Before X-Raying on the Production on Primary Non-Disjunction (Winchester)

As mentioned in the introductory section, the senior author had shown, in a limited series, that the age of the virgin females at the time of giving the X-ray treatments was an important factor in the production of primary non-disjunction. It was found that for a constant dosage the amount of non-disjunction was directly proportional to the age of the virgin females at the time of treatment, up to a certain stage, after which there was a decrease. The purpose of this series of experiments was to extend the earlier observations, by the use of a more extensive series of stages.

In these experiments females bearing the sex-linked recessive genes for yellow, white, forked, and the dominant gene for bar eyes were used as the treated parents, while the untreated parents were eosin miniature males. With both of these combinations it is a simple mater to detect the exceptional flies in the F_1 cultures, for the exceptional males will be eosin miniature like their father and the exceptional females will be yellow white forked bar like their mother, while the regular males will be yellow white forked bar and the regular females will be heterozygous eosin bar.

The virgin females were held in vials, under crowded conditions, for various lengths of time. At the end of any period of aging, one half of the females were given a dosage of 795 *r* units, and then mated and placed in half-pint culture bottles, with about fifteen pairs to the bottle. The other half of the group was mated without treatment and used as controls. At the end of each twenty-four hours, for a period of ten days, the flies were transferred to fresh culture bottles, so that at the end of any given ex-



EFFECT OF AGING AND X-RAYS ON NON-DISJUNCTION

Figure 14

Block graph showing the effects of aging the eggs before X-radiation on the production of non-disjunction. The rectangles in solid black represent the control series; those in outline represent the X-rayed series. The ordinates indicate the number of non-disjunctional flies.

periment there were ten sets of bottles, each set containing eggs that had been laid within a period of twenty-four hours. After the flies had all emerged in a set of bottles the first 1,000 were examined and the number of exceptional flies determined.

During the course of the work, ten different groups of flies were handled in this way. The groups were aged one, two, three, four, five, six, seven, eight, nine, and ten days, respectively. It was found that the flies in the X-rayed series in each group laid very few eggs on the first day after mating, making it impossible to obtain 1,000 offspring for that day. In order to be certain that the exceptional flies were due to primary non-disjunction, many of these males were tested and in no case was such a male found to be fertile.

The results obtained from the experiments outlined above are shown in Tables IV and V. There was a total of 91,262 flies examined in the X-rayed series, and 93,997 in the control series. The total number of exceptional flies secured from the X-

rayed series was 432, and that obtained from the control series was 68. As previously stated, it was impossible to obtain 1,000 flies for each experiment from eggs laid during the first day. However, there was found to be a very high per cent of exceptions from those that were secured. Taking them as a whole, we find that for the ten-day periods of aging only 1,262 flies emerged from eggs laid on the first day after treatment, but of these 56 were exceptions, which is 4.43 per cent of the total. This is more than three times the total per cent for the second day, which was the next highest with 1.27 per cent. In the controls (Table V) there was a total of 3,997 flies developed from eggs laid the first day, of which five were exceptions. This gave a per cent of .11 of non-disjunction flies, as compared to the average of .07 per cent from the other nine days.

In both tables the per cents of non-disjunction flies given in the first horizontal lines for the first day were calculated on the bases of 1,000 flies for each period of aging, but since the probable error of these numbers would be high, we have omitted them in the totals and base the calculations that follow on the data for the last nine days. Returning now the number of exceptional flies, we find that the X-rayed series gave a per cent of .418 as against .07 for the controls. The difference is $.348 \pm .011$, showing definitely that X-radiation greatly increases primary non-disjunction. The rate of the controls is in accordance with the number found by Mavor and others, who placed it at .06 per cent.

The totals given at the bottom of Table IV show that the number of non-disjunction flies in the treated series increases from nineteen, from eggs derived from females aged one day, to a maximum of sixty-six from eggs of females aged eight days. The numbers drop to thirty-four and thirty-two for eggs from females aged nine and ten days, respectively. This decrease can be explained by Brewster's observations that after eight days of aging the virgin females may deposit some of the mature eggs and others undergo disintegration within the ovaries. Such losses of mature eggs would tend to counteract the effect produced by storing the eggs with a proportionate decrease in the effect of X-radiation.

The corresponding numbers for the controls, as seen in the totals listed at the bottom of Table V, show no such increase for the different period of aging. If we take the numbers for the first eight days and express them in statistical terms, we find, in the X-rayed series, a coefficient of correlation of .966 between the number of days the females were aged and the number of non-disjunction offspring produced. This is a very high positive correlation. In the case of the controls, for the corresponding days, the coefficient of correlation is exactly .00, which shows that the aging itself has no effect on the production of non-disjunction. Figure 14 gives a graphical representation of these facts.

As we have just shown, the number of exceptional flies increases through the eighth day of aging. However, the data show that with reference to the day on which the eggs were laid, there is a decrease down through the sixth day, after which the number of exceptional flies per day remains practically constant, but still significantly higher than for the corresponding days of the controls. The per cents of non-disjunction from the first to the tenth days were 4.43, 1.27, .81, .52, .45, .24, .13, .11, .10, and .12, respectively. This is in agreement with Mavor's work, in which he showed that the greater number of non-disjunction flies come from eggs laid during the first six days after treating and mating. This shows that in the production of non-disjunction, the older the eggs the greater is their susceptibility to X-radiation.

Another point of interest is the ratio of primary non-disjunction females to primary non-disjunction males. Mavor places this ratio at 1 : 6. We find that in both the X-rayed and control series this ratio is 1 : 7.

III. The Effect of Aging on Breakage of the X-Chromosome

An extensive series of experiments on the effects of breaking the X-chromosome on the production of variant flies has been carried out by the senior author. In the light of the results given above it becomes a matter of interest to determine what effect, if any, aging the eggs before treatment may have on the formation of these variants.

The results obtained indicate that the percentage of variant flies due to breakage is also increased by aging the mothers before X-raying. For these experiments there were used several special stocks which made it possible to detect variations due to breakage of one of the X-chromosomes of the XX-zygote. The variant flies fall into four groups: (1) Aberrant females, caused by a loss of a portion of one of the X-chromosomes; (2) gray-yellow females or half and half mosaics, produced by breaking off a small fragment attached to the right- or fiber-bearing end of the X-chromosome (Theta X); (3) aberrant males, caused by the loss of the major part of one of the X-chromosomes of an XX-zygote; (4) sex-mosaics, or gynandromorphs, in which the male parts have received one X-chromosome and part of a broken X-chromosome.

The upper part of Table VI shows the results obtained from treating the eggs in females that had been aged for various lengths of time. The percentages there shown were corrected for the controls. The average dosage used in giving the treatments was 795 *r* units. The lower half of the table gives the data on aged mothers that were not treated.

If we compare, in the experimental series, the increase in breaks that occurred in the X-chromosome for each day of aging, after the second, with the number found for the second day, we find that there is a distinct difference. The per cent of breaks in eggs from females aged three days before treatment has increased from .144 to .566 over the second day, the difference being $.00422 \pm .00108$. This is three and nine-tenths times greater than the probable error. If in turn, each succeeding day (from the fourth to the seventh) is compared in a similar way with the second, the differences are found to be 4.01, 4.13, 3.78, and 3.72 greater than their respective probable errors. The data for the three days, 8th, 9th, and 11th, are placed together, and here we find that the per cent of breaks is lower than for any of the preceding five days, although the difference from the second day is still slightly greater than three times the probable error. The decrease in the percentage of breaks found after the seventh day is undoubtedly due to the same causes cited above in the experiments on non-disjunction, namely to the deposition of some of the mature eggs and to the disintegration of others.

In Table VII are shown the results obtained when virgin females were aged and then crossed to treated males. As may be observed, there is no consistent increase in the number of breaks due to the aging of the untreated mothers. The per cent for the first day is almost as high as for any of the other days, and is actually higher than that for the sixth day. Furthermore, the difference from the first day for the succeeding days is lower, in each instance, than the probable error. It is evident then that aging the eggs in females that are mated to treated males will have no additional effect on the rate of breakage in the X-chromosome. It is only when aged females are X-rayed that

Table I. Daily Egg Production of Virgin Females

Number of days	Number of females	Number of eggs deposited	Average number of eggs	Average number of mature eggs in ovaries
Experimental Series				
1	90	0	0	0.7
2	81	0	0	34.7
3	72	4	.05	56.2
4	63	3	.04	53.8
5	54	17	.31	76.5
6	45	62	1.40	76.7
7	36	84	2.30	79.6
8	27	120	4.40	66.0
9	18	41	2.20	85.5
10	9	34	3.70	88.5
Control Series				
1	90	71	.78	2.6
2	81	3226	39.80	10.4
3	72	4246	58.90	10.3
4	63	3233	51.30	11.7
5	54	3241	60.00	10.2
6	45	3011	66.90	10.0
7	36	2240	66.20	7.8
8	27	2041	75.50	8.6
9	18	1414	78.50	10.6
10	9	658	73.10	8.6

Table II. The Effects of Aging on Fertility

Age of females in days	Number of days eggs collected	Daily egg production	Average daily egg production	Number of larvae	per cent hatched	corrected percentage
Experimental Series						
1	1	332	18.4	287	86.4	93.30
	2	856	47.5	778	90.5	100.00
	3	969	53.8	885	91.1	98.80
3	1	270	15.0	190	70.3	78.00
	2	727	40.3	668	91.8	99.50
	3	774	43.0	708	91.4	98.80
5	1	396	22.0	294	74.2	81.20
	2	854	47.4	790	92.3	99.00
	3	962	53.4	915	94.7	100.00
7	1	593	32.7	440	74.2	79.30
	2	863	48.0	811	93.3	100.00
	3	786	43.8	742	94.0	100.00
9	1	635	35.2	476	74.9	80.10
	2	769	42.7	722	93.8	100.00
	3	795	44.1	723	92.2	100.00
Control Series						
1	1	410	22.7	380	92.6	
	2	873	48.5	789	90.3	
	3	919	51.0	861	93.6	
3	1	477	26.6	410	90.1	
	2	851	47.2	788	92.7	
	3	844	46.8	791	93.7	
5	1	545	30.2	498	91.3	
	2	784	43.5	740	94.3	
	3	834	46.3	788	94.4	
7	1	694	38.5	649	93.5	
	2	771	42.8	720	93.3	
	3	743	41.2	694	93.4	
9	1	752	41.7	703	93.5	
	2	811	45.0	762	93.8	
	3	816	45.3	765	93.7	

Table III. The Effects of X-Rays on the Fertility of Eggs that had been Aged

Number of days aged	Number of days eggs collected	Daily egg production	Number of larvae	Per cent hatched	Corrected percentage
1	1	351	232	66.0	76.3
	2	595	521	87.6	96.7
	3	751	703	93.6	100.0
3	1	613	260	42.4	60.7
	2	783	689	87.9	95.7
	3	762	706	92.6	100.0
5	1	640	215	33.5	45.1
	2	800	696	87.0	94.0
	3	850	770	90.5	100.0
7	1	637	153	21.0	28.3
	2	772	671	86.9	94.0
	3	855	803	94.5	100.0
9	1	565	211	37.3	49.8
	2	787	678	86.1	91.6
	3	911	835	91.6	100.0

Table IV. X-Rayed Series

Days eggs were laid	Days females were aged before treatment. Below shows percents of N.D.per 1000.										Reg. Males	Reg. Females	N.D Males	N.D Females
	1	2	3	4	5	6	7	8	9	10				
1st	0.8	3.7	3.1	10.4	7.7	11.9	11.3	11.4	7.9	2.4	575	631	51	5
2nd	0.5	0.7	0.6	1.5	1.4	1.6	2.0	2.6	0.8	1.0	4503	5370	109	18
3rd	0.3	0.6	0.6	0.7	1.3	0.6	0.9	1.3	0.8	1.0	4569	5350	74	7
4th	0.4	0.4	0.3	0.5	0.5	0.6	0.8	1.0	0.4	0.3	4635	5313	44	8
5th	0.1	0.3	0.2	0.5	0.4	0.7	0.9	0.7	0.4	0.3	4580	5375	41	4
6th	0.1	0.2	0.2	0.2	0.1	0.2	0.3	0.6	0.4	0.1	4662	5314	23	1
7th	0.2	0.0	0.1	0.1	0.1	0.3	0.1	0.2	0.2	0.1	4681	5305	11	3
8th	0.1	0.1	0.0	0.1	0.2	0.2	0.0	0.0	0.2	0.2	4611	5378	9	2
9th	0.1	0.2	0.1	0.0	0.1	0.1	0.1	0.1	0.1	0.1	4659	5331	10	0
10th	0.1	0.1	0.2	0.1	0.1	0.2	0.1	0.1	0.1	0.1	4627	5361	9	3
TOTALS	19	26	23	37	42	45	52	66	34	32	41,527	48,097	330	46

Table V. Control Series (See Table IV)

Days eggs were laid	Days females were aged before mating. Below shows percents of N.D.per 1000.										Males	Females	N.D. Males	N.D Females
	1	2	3	4	5	6	7	8	9	10				
1st	0.0	0.0	0.0	0.0	0.3	1.0	0.2	0.0	0.1	0.0	1881	2111	5	0
2nd	0.0	0.1	0.0	0.1	0.1	0.1	0.2	0.1	0.0	0.1	4649	5343	6	2
3rd	0.1	0.1	0.1	0.0	0.1	0.1	0.0	0.1	0.0	0.0	4623	5371	5	1
4th	0.1	0.0	0.1	0.0	0.0	0.1	0.0	0.0	0.1	0.2	4741	5253	5	1
5th	0.1	0.2	0.1	0.1	0.0	0.0	0.1	0.1	0.1	0.1	4709	5282	8	1
6th	0.0	0.0	0.1	0.1	0.1	0.1	0.1	0.0	0.1	0.0	4628	5366	5	1
7th	0.3	0.1	0.0	0.0	0.0	0.1	0.1	0.2	0.0	0.1	4661	5330	8	1
8th	0.0	0.1	0.1	0.0	0.0	0.0	0.1	0.0	0.0	0.1	4668	5328	4	0
9th	0.0	0.1	0.1	0.1	0.0	0.1	0.2	0.0	0.1	0.0	4687	5306	6	1
10th	0.0	0.2	0.1	0.0	0.1	0.1	0.1	0.1	0.0	0.1	4565	5427	8	0
Totals	6	9	7	4	4	7	9	6	4	7	41,931	48,006	55	8

Table VI. Treated Females

X-ray Series

Age of females in days	Number of treated X-chromosomes	Number of broken X-chromosomes	Percent of breaks	Differences from second day
2	2899	9	.144	
3	7029	46	.566	.00422 ± .00108
4	8538	57	.582	.00438 ± .00109
5	4639	28	.603	.00459 ± .00111
6	2070	13	.628	.00484 ± .00128
7	546	7	1.282	.01138 ± .00301
8,9,11	2985	11	.464	.00320 ± .00102

Control Series

Age of females in days	Number of X-chromosomes	Number of broken X-chromosomes	Percent of breaks
1	763	0	.000
2	4206	7	.166
3	4525	4	.088
4	1170	1	.085
5	380	0	.000
6	316	0	.000

Table VII. Treated Males

X-ray Series

Age of females in days	Number of treated X-chromosomes	Number of broken X-chromosomes	Percent of breaks	Differences from first day
1	637	4	.592	
2	5040	42	.599	.00007 ± .00258
3	1895	16	.763	.00171 ± .00274
4	707	5	.647	.00055 ± .00306
6	375	2	.533	.00059 ± .00337
7	531	4	.753	.00161 ± .00326

Table VII--Control Series

Age of females in days	Number of X-chromosomes	Number of broken X-chromosomes	Percent of breaks
1	2800	1	.035
2	4269	10	.234
3	1234	1	.081
4	1457	1	.060
7	663	0	.000

an increase in the number of variant flies due to breakage is found in the cultures. It has been observed that the increase in the number of variants is confined to the first three or four days after the flies begin to emerge, showing that the effect of X-radiation is more pronounced on the more mature eggs.

General Conclusions

1. Virgin females if held in crowded quarters retain their mature eggs for the first seven or eight days, but thereafter will deposit some of these eggs, while other eggs tend to disintegrate in the ovaries. The retention of mature eggs in the ovary greatly retards the development of the younger eggs.

2. Mature eggs retained by virgin females, when deposited after mating occurs, produce fewer larvae than those subsequently laid by the same females. The failure of some of these eggs to develop may be due to the fact that they are laid too rapidly for the inseminating mechanism to function properly.

3. We find that X-radiation increases the percentage of infertility, of primary non-disjunction, and of breakage in the X-chromosome, when applied to mature eggs that have been "aged" by retention in the ovaries. The conclusion is that the nearer the eggs are to maturity, the more susceptible they will be to X-radiation, with regard to the production of these phenomena. The advantage of using the aging method is so that larger numbers of eggs, having a greater susceptibility to the effects of X-radiation, can be exposed at one time. This susceptibility is probably related to the condition of the chromatin in maturing eggs.

Statistics of Divorce

STATISTICAL ANALYSIS OF AMERICAN DIVORCE, *by* ALFRED CAHEN. Columbia University Press. New York. Pp. 149. Price, $2.25. 1932.

PROFESSOR J. O. HERTZLER suggested several years ago that social efficiency requires a periodical audit of all institutions. The American family, sanctioned as it is by religion, law, and custom, has in recent years been indicted, in part, a number of times by various writers. The present study by Mr. Cahen, while by no means a complete study of the family, is a notable contribution to the body of exact knowledge concerning the reaction of the modern family to changing social conditions.

Family ties have been growing weaker ever since the Civil War. During the past 63 years the divorce rate has increased five fold. If the steady increase of the divorce rate, which "compounded annually at about a 3 per cent rate of increment" since 1867, is projected into the future, more than half the American marriages will end in divorce by 1965. In but ten states was the 1929 rate below the highest pre-1925 rate. Professor Willcox's prediction in 1891 has practically been realized, for at present 18 per cent of all marriages end in divorce, of which one-half occur in the first seven years. The average length of American marriages until ended by death or divorce is 20 years. Were divorce non-existent the average duration would be 23 years.

Concerning the immediate causes of divorce there is little exact information. About one-third may be "classified as due to grave causes" (adultery, drunkenness, and long time desertions); "the remaining two-thirds consist of the mutual incompatibility type." Since only about one-third of the divorced persons remarry and since many of these "probably had no idea of remarriage at the time of the divorce suit, there is no evidence that increasing desire for remarriage has been a substantial cause of the five-fold increase."

Laws have almost no effect upon the divorce rate. Only about 3 per cent of the annual 200,000 divorce quota are secured by non-residents who obtain divorces in Nevada, Paris, Mexico, etc., where the laws are most lenient. Dr. Cahen shows that "the divorce laws of today are not substantially different from those of sixty-three years ago" and that consequently legal changes have exercised little effect upon the divorce rate. But a "negligible" effect is observed in the 18 states which have altered their divorce laws since 1922. Dr. Cahen opposes the adoption of a nationally uniform divorce law. The expert knowledge requisite for such a law is lacking. Its adoption would penalize progressive states and destroy experimentation on part of the states. Actually 43 states have "substantial uniformity of laws."

The Causes of Divorce

Concerning the underlying causes of divorce there is much disagreement. Dr. Cahen's statistical analysis shows that degeneracy and poverty are but minor causes and that "evidence on religious decline and woman's freedom as causes of divorce is too uncertain to permit generalization." Urbanization, however, is "distinctly associated with divorce." Relatively more women work in cities and the birth rate is lower there. Thus the urban wife is economically more independent and the binding influence of children is lacking. While but 8 per cent of married couples are eventually divorced, 71 per cent of the childless marriages end in this manner. It follows from these figures that the absolute anti-social effects of divorce upon children have been exaggerated inasmuch as children of divorced parents form but a small percentage of the total number. Urban growth is also synonymous with growing economic production and accelerated speed of living in industrial cities, such circumstances tending to cause increased friction in family circles."

Summarizing, the increase in the divorce rate "is the product of changing social conditions. The equilibrium of the family institution has been shaken by the radical transformations in its economic and social environment." While intangible factors are not measurable it is apparent that social approval of divorce is increasing. The principal measurable factors which have affected divorces have been increased economic production, childlessness, entrance of woman into industry, etc., and urban growth. The business cycle has almost no effect. "City life involves a complexity of interacting factors affecting the unity of the home, that are reflected later and to a lesser degree in the country" where the divorce rate is only half as high. Functions formerly performed by the family are largely performed by other agencies today, especially in the cities. Divorce, in brief is a cost of progress. And since progress will probably continue the divorce rate will increase.

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University of Arizona.

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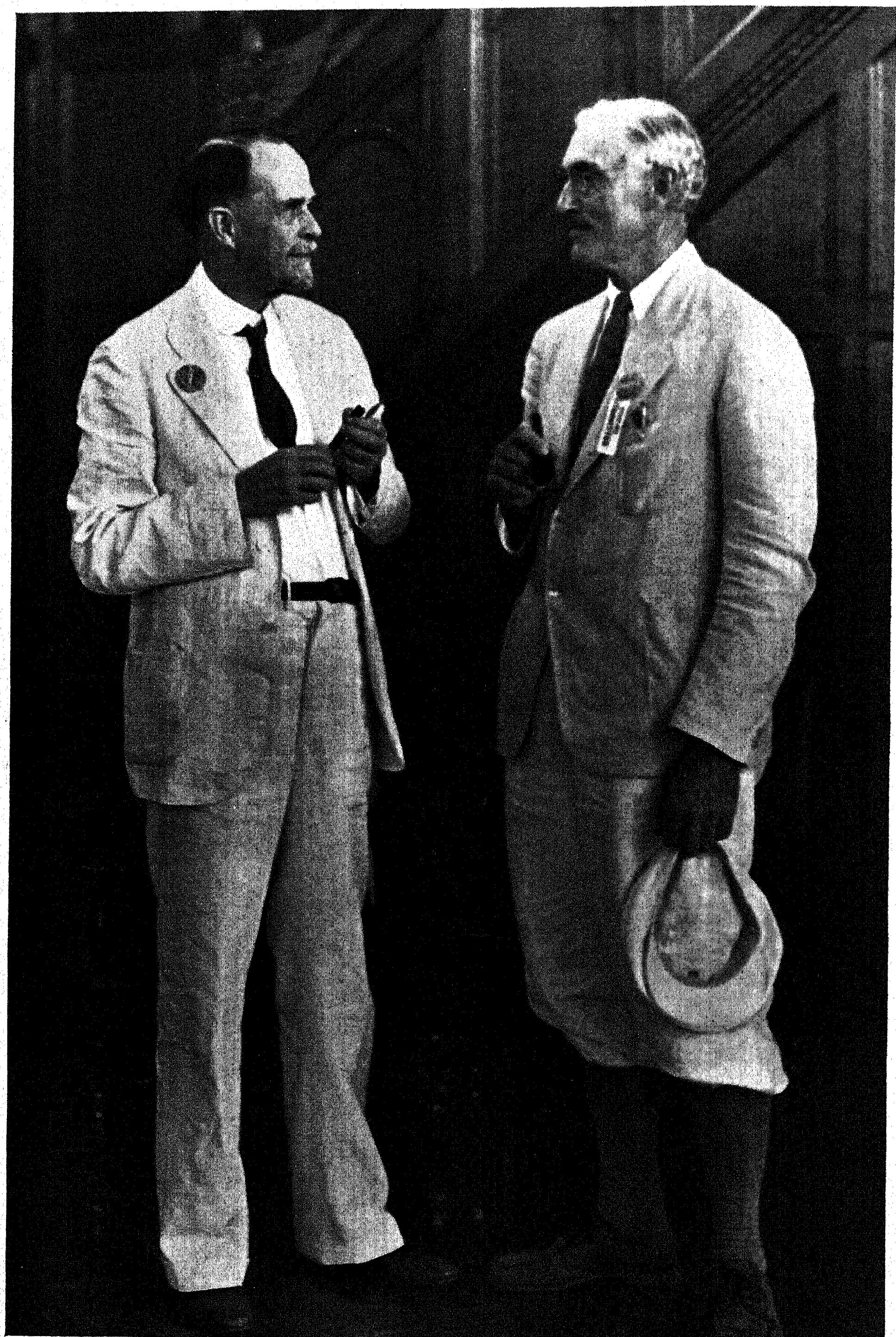


Photograph by Trevor Teele

AT ITHACA

Frontispiece

Thomas H. Morgan, President of the Sixth International Congress of Genetics, and R. A. Emerson, Chairman of the Local Committee, the organization at Ithaca whose arrangements contributed so greatly to the success of the Congress. The photograph was made in the Memorial Room of Willard Straight Hall, the headquarters of the Congress at Ithaca. Dr. Emerson was nominated as the American member of the Permanent International Committee, appointed to arrange for the next International Congress of Genetics.

THE RISE OF GENETICS

Excerpts from the Address of the President of the Sixth International Congress of Genetics, at Ithaca*

T. H. Morgan

THE new developments in science that occur from time to time can generally be traced either to the invention of a new method, or to the discovery of a new fact that has far-reaching consequences, or to the elaboration of a new theoretical principle that suggests new lines of investigation. In the latter case, it is the prerogative of science, in comparison with the speculative procedure of philosophy and metaphysics, to cherish those theories that can be given an experimental verification and to disregard the rest, not because they are wrong, but because they are useless.

In the case of genetics the situation was in some respects different from any of these procedures; for, it began with the discovery of a discovery that had been made 35 years before. We can date the beginning of genetics, then, from the resurrection of Mendel's paper in 1900. Its rehabilitation was not, however, due to a literary find, but to a need resulting from similar experiments by de Vries, Correns and Tschermak that unveiled a series of phenomena identical with the facts of Mendel's earlier work.

The significant fact is that when the time was ripe to appreciate its fundamental significance, Mendel's forgotten paper was discovered with the result that the activities of hundreds of biologists, as the program of this present Congress bears witness, had the direction of their scientific careers entirely redirected, or begun along new lines. The discoveries that rapidly followed, showing that the same laws applied widely to the other plants and to animals also, brought about realization that a great step forward in biology had been made.

* * * *

So far I have spoken only of plants. What part, may be asked, has the study of animals played in the pre-Mendelian history of genetics, i. e., down to 1865?

The question of sex in plants that took botanists a hundred years to decipher, was not so difficult for zoologists. If we may accept the traditional story, it was not unknown in the Garden of Eden. Aristotle had a good deal to say about it. The credit of finding a sex-determining mechanism can properly be claimed by zoologists, but this happened only in the opening years of the present century.

Hybridizing was also familiar to zoologists, but in pre-Mendelian times occupied only a relatively small part of their interest. What was known has been recorded by Darwin in his *Animals and Plants under Domestication.* This scattered and loose information was incorporated after 1859 in the discussions of the theory of evolution.

The chief contribution of zoologists to present-day genetics was along different lines. In the latter half of the last century there was great activity in the field of cellular morphology. The important facts concerning chromosome-division, and the extraordinary changes that take place at the time of maturation of the germ-cells

*Delivered by Dr. Morgan on Thursday evening, August 25, at Bailey Auditorium, Cornell University, Ithaca, New York. The full address will appear in the Proceedings of the Congress, and in *Science*.

and at fertilization were first made out by zoologists.

* * * *

Thus at the end of the last century some extraordinary advances had been made in unravelling the changes that take place in the maturation of the germ-cells. These advances led to the recognition of a mechanism that was to place the theoretical elements of Mendel's hypothesis on a firm foundation of fact. But this, however, was not apparent until 1903.

Genetics at the Beginning of the Century

We come now to the fateful year 1900, when three lines of fundamental significance for genetics were ready to be brought together. I refer, of course, to the mutation theory of de Vries, to the re-discovery of Mendel's paper, and to the application of the discoveries in cytology to the new theories.

The intimate connection between the Mutation Theory, as first propounded, and the origin of the characters that follow Mendel's laws, was not immediately evident, since de Vries laid emphasis on the many character-changes that result from each progressive mutational step.

* * * *

It was the emphasis that de Vries laid on mutational changes in the *germinal* material as sharply discontinuous, irrespective of the effect on the character, that has had important and far-reaching consequences for genetic work and theory.

The groundwork for discontinuous *phenotypic* variation had in 1894 been laid by Bateson's contribution on discontinuous variation. While we recognize that some of the examples Bateson collected are not inherited but phenotypic (which confused the picture), nevertheless his insistance on the importance of discontinuity prepared the way for the acceptance of the more fundamental distinction that de Vries has made.

But I wish to emphasize that the revolution in our ideas that took place at this time was not so much due to the insistance on discontinuity of somatic structures, but discontinuity in the hereditary elements. An example will serve to illustrate the difference. When a gene changes, its effects on new characters, taken individually, are generally very different. Some of them may be sharply marked off from the original character. The character showing the greatest effect is the one generally picked out for genetic work. But at the same time there are changes in other organs that are less conspicuous—some of the characters are so little affected or so variable that, taken by themselves, they would give a picture of continuity rather than of discontinuity. They would often pass unnoticed were not attention drawn to them by the discovery of the major change.

For the theory of evolution some of these inconspicuous changes may be more significant than the more obvious discontinuous change. In fact, if evolutionary advances are more often through invisable physiological mutational changes rather than morphological ones, we can better understand the paradoxical situation in which taxonomists find themselves, to wit, that the sharp structural differences that are used for diagnostic separation of species relate to characters that seem often to be unimportant for the well-being of the individual. The new point of view is a complete inversion of much of the thinking in which the evolutionary theory indulged in the past.

As I have said, the rapid expansion of genetics after 1900 has been intimately connected with the applications of the chromosome theory to the experimental work in genetics. The integrity of the chromosomes and their continuity from one cell-generation to the next, the constancy in number of the chromosomes in each species, and the absence of mix-

ing of the materials of the conjugating chromosomes at the time of meiosis, have furnished the basis on which genetics rests.

I think we can not over-emphasize the significance of this relation between the theoretical side of genetics and the factual side as observed in the known behavior of the material basis of heredity. To put the matter bluntly, the recognition that there is a mechanism to which genetic theory must conform, if it is to be productive, serves to keep us on the right track, and acts as a check to irresponsible speculation, however attractive it may seem in print.

* * * *

Expansion Since 1900

I come now to the expansion of the Mendelian theory that has taken place in the last 30 years. If I refrain from giving the names of the numerous contributors to this advance, it is because many of the discoverers are before me in person; or, if not, will get reports of the Congress. Future congresses will probably be better able to evaluate individually the merits of those who have made the significant contributions in this generation.

It must have been evident to many geneticists after 1903 that if the chromosomes are the bearers of Mendel's elements, there would be only as many independent "characters" as there are chromosomes, provided the then current idea of the integrity of the chromosomes were true. This would place limitations on Mendel's second law—the law of independent assortment.

* * * *

Linkage also turned out to have its limitations, and these very limitations made it possible to determine the localization of the genes in the chromosomes. I refer, of course, to crossing over. Since localization of the genes is today the basis of much of the quantitative work in genetics, I may be allowed to elaborate it somewhat.

The outstanding genetic fact is that these interchanges take place only between homologous chromosomes, i. e., between members of the same pair.

The second important genetic fact is that when the interchange takes place, large blocks of the chromosomes are exchanged. This can be proven only in cases where more than two loci are involved, and best when a considerable number of well-spaced genes have been located, Until recently the evidence that large blocks of genes are involved in crossing over was known only genetically. No certain cytological proof was known. Today, however, the proof has been found. No doubt this cytological evidence will be presented and discussed at this Congress.

It has also been determined on genetic evidence that more than one interchange may take place between a pair of chromosomes which can be checked only in cases where there are enough intermediate loci between two pairs to serve as markers.

A moment ago I said that crossing over has furnished the basis for the theory of localization. May I give an illustration, in the hope of removing a criticism of the localization technique that is based, I believe, on a misunderstanding? It has been said, for example, that the changes made from time to time in the genetic map of the Drosophila chromosomes, discredit the method by which the localization is determined. It might as well be said that the method by which the atomic weights in chemistry were gradually improved discredited the procedure of the chemist.

* * * *

This brings us to one of the most recent fields of modern genetics: the study of the redistribution of the linkage group by translocation. Treatment with x-rays has been found to be a prolific source for material of this kind, but it should not be forgotten that translocation had been

discovered and utilized for genetic interpretation several years before x-rays were used. Even today, with much evidence before us, the way in which x-rays bring about this result puzzles us. In a crude way we might picture the electron shooting holes in the chromosomes, thus breaking them apart. But when the relative sizes of the electron and the chromosome are considered, it is difficult to see how such a disruption would result from a single shot.

Even more surprising is the fact that the broken end of a piece may reunite with the end of some other chromosome and, acquiring thereby an attachment fibre, form a new linkage group. Of course it does not follow that such a reunion occurs whenever a chromosome is broken. It is only those cases where reunion does occur that are recovered and studied by geneticists. When no such union is brought about the piece, lacking an attachment point, will be lost, and the zygote containing it will probably die.

* * * *

Polyploidy

In even a passing review of present day genetics, the numerous problems connected with the increase in number of the chromosomes, or polyploidy in technical language, can not be ignored. But how can one hope even to summarize the work that is pouring in with the arrival of every new number of the genetic journals? The importance of polyploidy for the evolution doctrine is perhaps clear, but needs cautious handling in the light of the past history of phylogenetic interpretation of the facts of comparative anatomy.

* * * *

There are several known ways in which we can bring about a doubling of the number of chromosomes in a cell. The usual way is to suppress the cytoplasmic division of the cell at the time when the chromosomes divide. When this is done the chromosomes do not reunite, but the descendants of that cell will forever possess twice the original number of chromosomes. Theoretically the process might go on forever, unless there are upper limits of a physiological nature preventing an indefinite increase. Doubling diploids gives tetraploids. These crossed to diploids give triploids. Double tetraploids (or octoploids) crossed to tetraploids give hexaploids, and so on.

This work furnishes an opportunity for the solution of certain genetic problems of theoretical interest, for, without this knowledge, some of the known genetic ratios would have been difficult to interpret. With this knowledge they are found to conform to recognized genetic principles.

* * * *

Of great importance for the genetic interpretation of polyploidy in terms of chromosomes is the identification of chromosomes that carry specific genes. Only a few years ago this was known in only one animal, but the number of cases is steadily increasing. Until information of this kind becomes more general there will be, as at present, a good deal of guessing as to the relation of chromosome groups having different numbers of chromosomes.

Influence of the Genes on the Cytoplasm

If another branch of zoology that was actively cultivated at the end of the last century had realized its ambitions it might have been possible today to bridge the gap between gene and character, but despite its high-sounding name of Entwicklungsmechanik nothing that was really quantitative or mechanistic was forthcoming. Instead, philosophical platitudes were invoked rather than experimentally determined factors. Then, too, experimental embryology ran for a while after false gods that landed it finally in a maze of metaphysical subtleties. It is unfor-

tunate, therefore, that from this source we can not add, to the three contributory lines of research which led to the rise of genetics, a fourth and greatly needed contribution to bridge an unfortunate gap. I say this with much regret, for, during that time and even now I have not lost interest in the fascinating field of embryological experimentation. It is true that a great deal of factual evidence came to light, and it is true that many misleading ideas were set aside, but the upshot was negative so far as the formulation of any of the factors of development, whether mechanistic or otherwise, are concerned. This may be because the work was pioneer and largely qualitative. Perhaps my disappointment at the outcome of the work has led me to an overstatement of its failures. Something did emerge that the future may show to be of fundamental importance for genetics. I mean the experimental demonstration that the immediate factors in the differentiation of the embryo are, at the time of their activity, already in the cytoplasm of the cell. Second only in interest was the discovery that within certain limitations the already determined specificity may be reversed, or rather, shall I say, the initial steps already taken are reversible by factors extraneous to the individual cells.

* * * *

The second inference is no less significant. I need not labor the point at this late date that the characters of the individual are the product, both of its genetic make-up and its environment, both internal and external. The earlier, premature idea, that for each character there is a specific gene —the so-called unit-character, was never a cardinal doctrine of genetics, although some of the earlier popularizers of the new theory were certainly guilty of giving this impression. The opposite extreme statement, namely, that every character is the product of all the genes, may also have its limitations, but is undoubtedly more nearly in accord with our conception of the relation of genes and characters. A more accurate statement would be that the gene acts as a differential turning the balance in a given direction, affecting certain characters more conspicuously than others. But the environment may also act as a differential, intensifying or diminishing, as the case may be, the action of the genes.

The best illustration of this double relation is seen in the determination of sex. When an unpaired chromosome is present, in one or in the other sex, its genes determine, as a rule, whether a male or a female develops from each egg. Under environmental conditions which, as we say, are normal, the differential acts almost perfectly, but under other unusual conditions and in a few special cases its power may be partially overcome, and even a reversal may take place. These unusual environmental conditions may be external agents, such as temperature or light. They may also be internal factors, such as hormones. Even "age" itself may bring about a reversal of sex in certain types. These statements are commonplaces today. The only differences of opinion concern the emphasis that one theorist places on the environment, and another on the genetic composition.

In passing, a word may be said about the genes as sex factors, or differentials. All through the 32 years of the present century there have been attempts to isolate (in a genetic sense) the sex-determining factors. At first, when the chromosome mechanism was discovered, the idea prevailed that one X, let us say, made a male, and two X's a female. This was a rough quantitative theory of sex. The sex-chromosome itself was then taken as the differential. Very soon after this the idea that the sex-chromosome was the carrier of a gene for sex prevailed, and a long search was started to locate such a gene or genes in this chromosome. More recent work on translocations has shown the probable futility of such an interpretation. The

tendency at present is rather to look upon all the genes, or at least many of them, as sex-determining in exactly the same sense, as all or many of the genes have an effect on the development of each character. It may well be, however, that certain genes in the sex-chromosome (as in other chromosomes) are more influential than others in turning the balance one way or the other, but even so, it does not at the present moment,—in the light of recent evidence,—seem probable that a single gene for sex-determination is to be found in the X-chromosome any more than, in the contrary sense, there is a single gene for sex in any special autosome. Here again, some one or a few genes may be more influential than others, but this is also true to varying degrees for the genes of any other character.

* * * *

Evolution

Sooner or later every geneticist is asked what bearing this work has on the theory of evolution. In the early years of the century when genetics was new, some of us tried to sidestep the question, partly on the grounds that genetics was not ready to discuss the bearing of the new work on evolution, but mainly because it seemed unfortunate to compromise the precise results of the new procedure with the evolution doctrine which, because it dealt with an historical problem, was largely speculative. After 32 years of activity, caution may still be the wiser course to pursue; yet, on the other hand, we are now prepared, I think, to make a more definite commitment. It is, of course, obvious that only those characteristics that are inherited can take part in the process of evolution. The only characters that we know to be inherited are those that arise first as mutants, *i. e.,* discontinuously, or, as we say, by a change in a gene. Here genetics has made a very important contribution to evolution, especially when it is recalled that it has brought to the subject an exact scientific method of procedure. If we compare our present status in this respect with the discussions of the old school of evolutionists concerning variability, there can be no question but that genetics has made a distinct advance.

In the second place, the objection has been not infrequently made that geneticists are dealing only with aberrant or abnormal characters — hence their results, however accurate, can have nothing to do with the kind of progressive changes that have made evolution of new types possible. Such objections have come largely from those who ignore what geneticists have done and are doing. The same objections have also come from those whose minds are closed to new evidence or who can not distinguish between the value of tested and verifiable theories and vague views or juvenile impressions with a teleological background or bias.

Without elaborating, I wish to point out briefly that there is today abundant evidence showing that the differences, distinguishing the characteristics of one wild-type or variety from others, follow the same laws of heredity as do the so-called aberrant types studied by geneticists.

Even this evidence may not satisfy the members of the old school because, they may still say, all these characters that follow Mendel's laws, even those found in wild species, are still not the kind that have contributed to evolution. They may claim that these characters are in a class by themselves, and not amenable to Mendelian laws. If they take this attitude, we can only reply that here we part company, since *ex cathedra* statements are not arguments, and an appeal to mysticism is outside of science.

There remains still the question of the causal origin of mutations. Here also some progress had been made, but the subject is admittedly by no means on the same footing as is our knowledge of the laws of inheritance. It behooves us, then, to be careful, for our

progress in this respect has been slow and to some extent erratic. I mean by this that we have not yet found a method of producing specific results—*i. e.,* a method by which particular genes can be changed in a particular way.

Even here, however, something has been done. In the work with x-rays and heat the same mutants appear that are already known, and that have come up without treatment. In addition, new mutants appear, as they do also without treatment. If it can be shown on a large scale that the same ratio for known mutations holds for x-ray and for spontaneous mutations, we may nave found an opening for the further study of the causes of certain types of mutation.

The Future

I have been challenged recently to state on this occasion what seemed to be the most important problems for genetics in the immediate future. I have decided to try, although I realize only too well that my own selection may only serve to show to future generations how blind we are (or I have been, at least) to the significant events of our own time.

First, then, the physical and physiological process involved in the growth of genes and their duplication (or as we say their "division") are obviously phenomena on which the whole process of reproduction rests. The ability of the new genes to retain the property of duplication is the background of all genetic theory. Whether the solution will come from a frontal attack by cytologists, geneticists and chemists, or by flank movements, is difficult to predict, although I think the latter more promising.

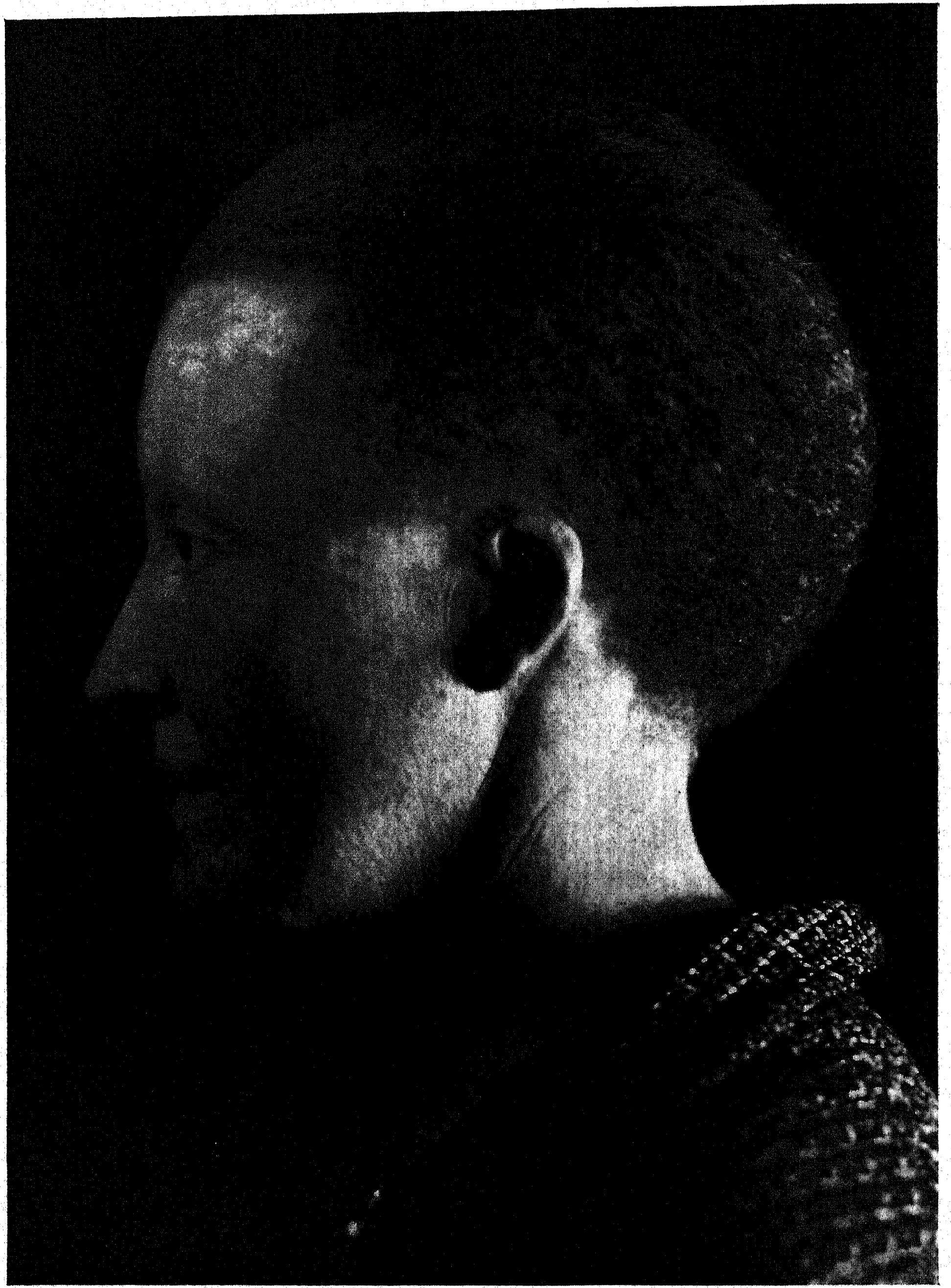
Second: An interpretation in physical terms of the changes that take place during and after the conjugation of the chromosomes. This includes several separate but interdependent phenomena: the elongation of the threads; their union in pairs; crossing over, and the separation of the four strands. Here is a problem on the biological level, as we say, whose solution may be anticipated only by a combined attack of geneticists and cytologists.

Third: The relation of genes to characters. This is the explicit realization of the implicit power of the genes, and includes the physiological action of the gene on the rest of the cell. This is the gap in our knowledge to which I have referred already at some length.

Fourth: The nature of the mutation process—perhaps I may say the chemico-physical changes involved when a gene changes to a new one. Emergent evolution, if you like, but as a scientific problem, not one of metaphysics.

Fifth: The application of genetics to horticulture and to animal husbandry, especially in two essential respects; more intensive work on the physiological rather than the morphological aspects of heredity; and the incorporation of genes from wild varieties and species into strains of domesticated types.

Should you ask me how these discoveries are to be made I would become vague and appeal to generalities. I would then say—by industry, trusting to luck for new openings; by the intelligent use of working hypotheses (by intelligence I mean a readiness to reject any such hypotheses unless critical evidence can be found for their support); by a search for favorable material, which is often more important than plodding along the well-trodden path hoping that something a little different may be found; and lastly, by not holding genetic congresses too often!



"SELF-BOBBING" HAIR

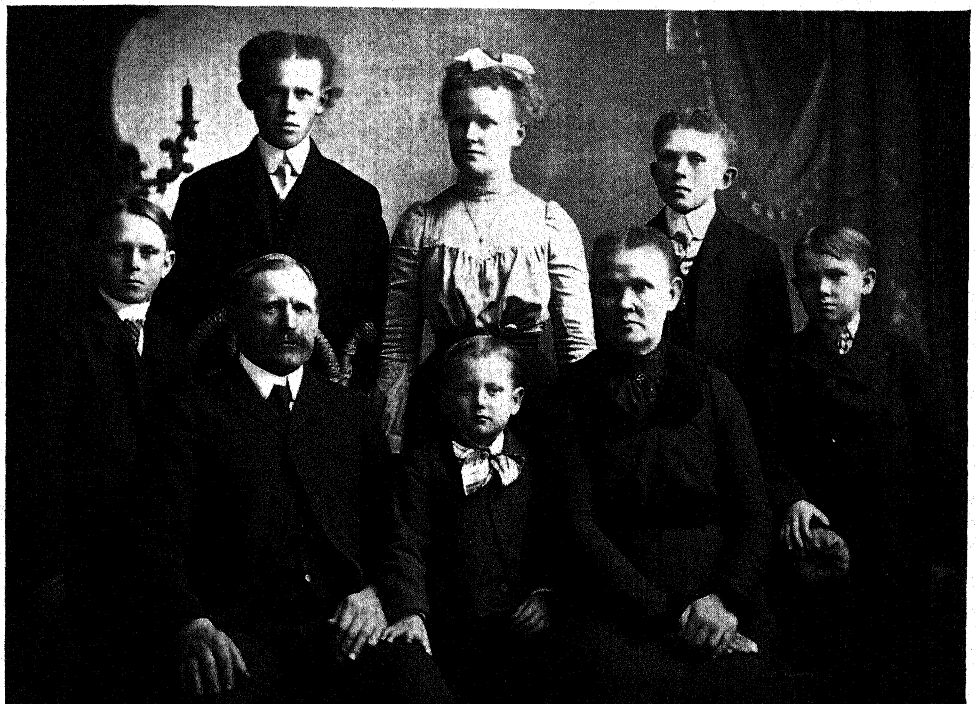
Figure 1

Woolly-haired woman with short, uncut hair. No. 2, sixth family line 2, IV generation. Before the days of bobbed hair the female members of this family were embarrassed by the fact that their hair bobs itself spontaneously by breaking off when it reaches a length of from one to three inches.

WOOLLY HAIR A DOMINANT MUTANT CHARACTER IN MAN

Otto L. Mohr

Anatomical Institute, The University, Oslo, Norway

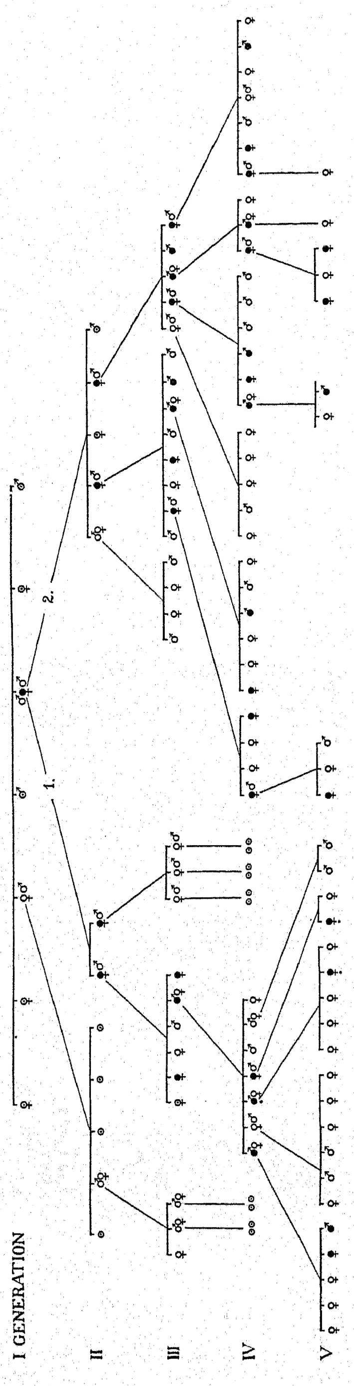


SEGREGATION FOR WOOLLY HAIR

Figure 2

Heterozygous woolly-haired woman (hair uncut) and her unrelated straight-haired husband. Fourth family, line 2, IV generation. The Mendelian segregation is clearly demonstrated among the children, of whom three are woolly-haired and three straight-haired. This character has appeared in a Nordic stock in which the possibility of negro admixture is so extremely remote as to be negligible.

AFTER a popular lecture on heredity for teachers, one of the lady attendants came to show me her very peculiar "woolly" type of hair, which was inherited within her family. She provided me with a rich material of pedigree data, and later also with numerous photographs of affected and normal members of the family. Through her assistance I also was put in touch with one of her relatives, Rector A. D. Danielsen, Hornnes High School. Rector Danielson, who is himself typically woolly-haired, is particularly interested in genealogy and has collected a remarkably complete and detailed family record which he kindly placed at my disposal. I am likewise indebted to him for a large number of photographs of family members. He was fully aware of the dominant inheritance of this type of hair in his family. He had, in fact, published a pedigree of his own family as an illustra-



PEDIGREE CHART OF INHERITANCE OF WOOLLY HAIR

Figure 3

Inheritance of woolly hair in two lines of a family for five generations.

tion of dominant inheritance in man, in connection with a popular lecture on this subject.

The character is so striking and the data on its hereditary type so clear that it deserves to be put on record. In fact, a photograph like the one reproduced as Figure 2 may in itself justify the following short account of the case. The author wishes to express his sincere indebtedness to the above-mentioned family members for their interested cooperation.

The general description of the character is based upon personal examination of hair samples from different affected family members, on the study of photographs from 23 woolly-haired individuals, of which 9 belong to line 1, 14 to line 2 (see pedigree, Figure 3) and on information supplied by the members of the family.

Description of the Woolly Character

The woolly character is very constant in appearance, as may be seen from the photographs reproduced in this paper, which cover three generations. The capillus is very strikingly curled or frizzled, not unlike negro hair, a fact which makes it very natural that family members frequently have discussed whether an intermixture of negro blood in the remote ancestry might possibly account for their peculiar hair type. The hairs of the head are densely implanted.

A very peculiar feature is the following: In spite of a very good hair growth (frequent cutting in male individuals) the hair never gets long. This is due to spontaneous breaking of all the hairs when they attain a certain length (see below). Before bobbed hair became a fashion among the ladies, woolly-haired female members of the family were much bothered because they could not arrange their abnormally short, strange-looking hair in a way that freed them from annoying inquisitiveness of others. Also, even today, they frequently realize that they are not like other people. The dense curling is also apt to make the hairs en-

tangled so that combing is rendered difficult. The abnormal shortness of the capillus is interesting in so far as it is analogous to the permanent shortness of the hair in the ulotrichous races (Negro, Negrito, Melanesian, Bushman, Hottentot, Papuan).

Only three individuals belonging to line 1, IV and V generation (in the pedigree denoted by a period (.) under the individual symbol) are described as being strikingly curled, but perhaps not quite as woolly or frizzled as the other affected family members. I have not, so far, been able by personal examination to decide whether we are here dealing with a modified type of the woolly character.

It is a common feature that the hair is of a very light color, in childhood, even apparently unpigmented, and that it gradually turns somewhat darker with age. Occasionally, however, a dark hair color may be present in the wooly-haired children, so there is no absolute correlation between the shape of the hairs and their color.

The following more detailed description of a woolly-haired female individual (Line 2, V generation, age 30 years; Figure 1) may be regarded as representative in all principal features:

The hairs of the head are densely implanted and intensely crisped or frizzled. The curling is of a spiral type. The dense implantation and the very pronounced crisping makes the capillus feel compact and resistent to the hand, much like sheep's wool.

The length of the hairs on the vertex varies from 5 cm. to 10-12 cm. Only in rare cases do they attain a length of 14 cm. Judging from photographs of woolly-haired females the capillus may in some individuals reach a somewhat greater length (see Figure 5). But the hair never gets long. On the whole, the length seems to correspond well to the values which are regarded as typical of the short-haired ulotrichous races. Thus, according to Martin[7] "the uncut hair of the ulotrichous races remains short, the length varying from about 8 cm. to about 25 cm." According to Landauer,[6] Friedenthal found the average length of the capillus in negros to be about 15 cm. in both sexes.

A WOOLLY-HAIRED CHILD
Figure 4

Woolly-haired girl, No. 1, fifth family, line 2, IV generation. One of the reasons for believing that this is a mutation rather than due to hybridization is that the characteristic remains constant for generations, rather than fading out as is the case in continued backcrosses from a negro-white hybrid to the straight haired race.

In the above-mentioned female family member the degree of curling comes nearest to No. 28 in Fischer-Saller's standard series of hair samples. The hair color is light, "faded," corresponding approximately to Fischer-Saller C. or to Eugen Fischer No. 23 with a shade approaching No. 25.

The supercilia are rather dense, of the same color as the capillus, short, thin, and curled. The cilia are ordinary in number and implantation and of the same color as the supercilia. They are not particularly thin, but show a slight indication of curling.

On the shoulders are scattered pale, lanugo-like hairs in slightly brownish pigmented hair follicles. On the dorsal surface of the under-arm sporadic hairs of an analogous type are found, on the ventral surface of the under-arm

UNUSUALLY LONG HAIR
Figure 5
Woolly-haired woman, mother of woman in Figure 1. The uncut hair somewhat longer than that of her daughter, No. 5, third family, line 2, III generation. The factors involved in variations in length at which the hair breaks off are not known.

and on the dorsum of the basal finger joints very few analogous hairs are present. The axillary hairs and the pubes are thin, relatively scarce, short and densely curled, slightly darker in color than the capillus.

Microscopical examination of hairs from the vertex of woolly-haired individuals seemed at first to indicate that the diameter of the woolly hairs was about equal to that of corresponding hairs from straight-haired control individuals. But it was observed that the woolly hairs showed with rather irregular intervals narrower parts where they seemed to be thinner. And on closer examination it turned out that these seemingly narrower parts in reality are due to a relatively sudden twist along the longitudinal axis, a fact which indicated that the woolly hairs are flattened in shape and, in this respect, different from the more roundish hairs of the control individuals. In some samples of hairs from wooly family members the flattened hairs are more continuously spirally twisted in a spring-like way, when examined under the microscope.

Transverse sections were now secured by aid of the celloidin method devised by Fiala[3]. As will be seen from the micrographs of hairs from the vertex of two woolly-haired individuals and one smooth-haired control individual (Figure 8, a, b, c) the former are characterized by a flattened shape, giving the cross-sections a flattened oval, or, in some cases—presumably near a twist—a kidney-shaped form.

For a discussion of the relation between flattening of the hair shaft and curling we may refer to Danforth[2]. Though it seems clear that the form of the cross-sections may vary considerably within a race or even to some extent in the same person, there seems on the whole to be a marked correlation between straight hair type and roundish form on the one hand, and between frizzly hair type and flattened oval hair form on the other. Thus, in Fritsch's tables of cross-sections from different races[4] the cross-sections from the curled hairs of the ulotrichous races (by Fritsch denoted as Nigritier) are all characterized by a flattened, or in some hairs, kidney-shaped cross-section, in contrast to the cross-sections from the rest of his material, which were more or less roundish in shape. The cross-sections of hair from the woolly-haired individuals here investigated are very much like some of Fritsch's samples of cross-sections from ulotrichous individuals (in Fritsch's material represented by Nubian, Bushman, Andamanese, Papuan, Zulu, Akka).

It was a striking feature that traces of a medulla were present only in very exceptional cases in woolly-haired individuals, and in some samples of wooly hairs every indication of a medulla was lacking. But the material is as yet too

WOOLLY-HAIRED FATHER AND SON
Figure 6

No. 1, fourth family, line 2, IV generation, and No. 2, second family, line 2, V generation.

limited to permit conclusions as to whether this may be regarded as a typical relation. The irregularly transverse border lines of the imbricate cuticular epithelial cells (the "scales") are perhaps somewhat more marked in the woolly hairs than in the hairs from smooth-haired individuals, but the difference is not striking. The free margin of the ceratinized scales is irregularly crenate.

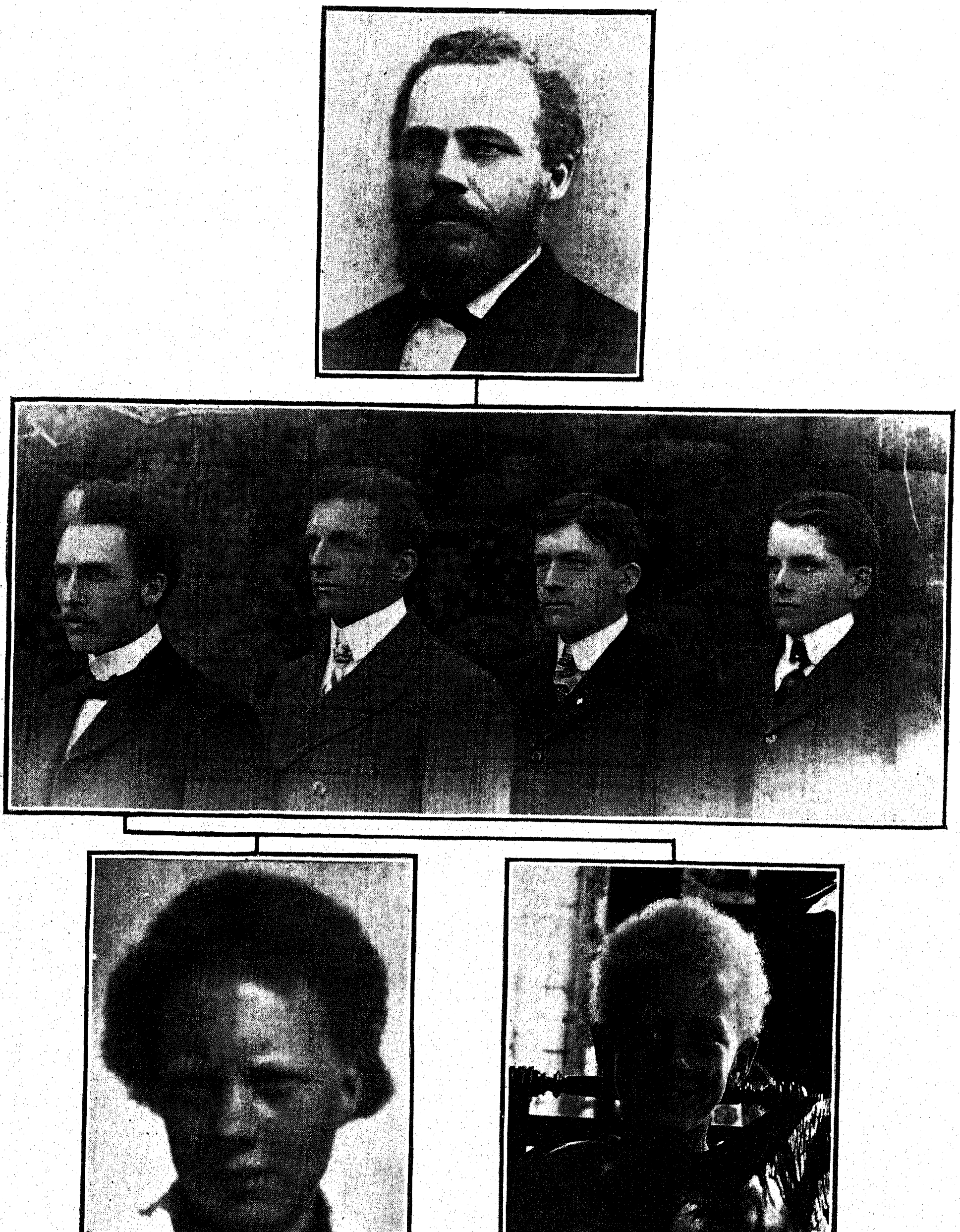
The free end of the woolly hairs is not tapered. Suddenly the hair splits up in a bundle of separate fibres, each of which breaks off transversely or in a slightly oblique direction. The protrusion of the separate fibres from the solid part of the hair shaft is of a somewhat different degree. Not infrequently the central fibres are slightly longer than the peripheral ones, so that the free end of the hair may appear bluntly rounded at low power of magnification. When the wooly hairs are treated with concentrated sulphuric acid, the end fibres diverge more markedly, giving the end of the hair a more pencil-like appearence. The fibres themselves resist the action of sulphuric acid and retain their shape practically unaltered, even when the acid is heated to the boiling point and the scales entirely macerated.

Hereditary Type of the Woolly Hair

As seen from the pedigree (Figure 3), the woolly hair character studied behaves as a clear cut Mendelian dominant.

As regards the reliability of the information on which the pedigree is based it should be mentioned that with very few exceptions (see below) we know the full name, the birth and eventually the death year as well as the hair type of all the individuals belonging to the family lines 1 and 2. If there is any doubt as to the general hair type, e. g., in individuals which died in infancy, this is denoted by a (.) in the individual ♀ or ♂ symbol. Individuals of unknown sex are denoted by ⊙; families where number and sex of the individuals is unknown are denoted by ⊙⊙. In line 2 the information on the members of the V generation is probably incomplete, since quite a few members of the line 2 have emigrated to the United States of America where their descendants now live.—The ♀ ♂ symbols indicate normal-haired, i. e., non-woolly individuals, while the woolly-haired family members are given black symbols. I have as yet had no opportunity of making the hair type of those individuals who have married into the family the object of a more detailed study. Whether different hair types in these individuals may possibly influence to some extent the woolly hair type among their children is therefore an open question.

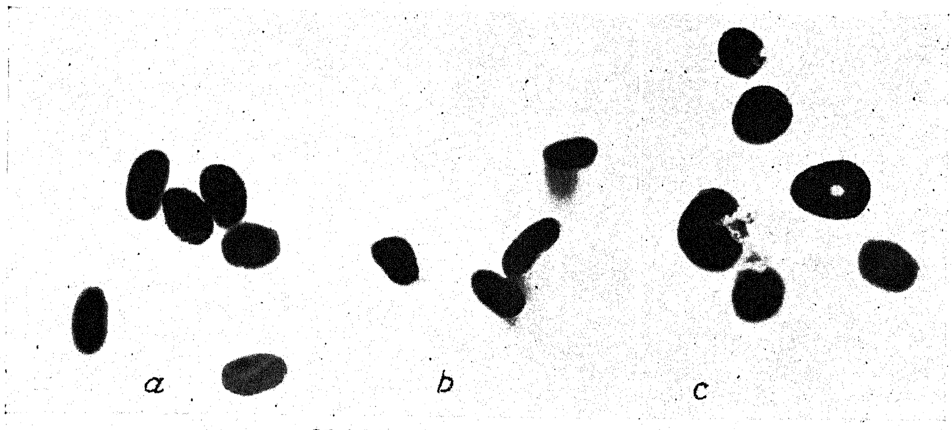
As seen from the pedigree the woolly character can be traced back to a female ancestor who married twice and from whom two different lines, 1 and 2, are derived. This individual,



THREE GENERATIONS OF WOOLLY HAIR

Figure 7

Photo-chart, showing inheritance of woolly-hair characteristics for three generations. The individuals shown are in generations III, IV and V in the chart (Figure 3) (III-8; IV, 3, 5, 7 and 8; V-4 and V-5). The inheritance is typically that of a dominant Mendelian gene, which appears little modified in successive generations, and showing clear-cut segregation of normal brothers and sisters of woolly-haired individuals.



CROSS-SECTION OF HAIRS

Figure 8

A and *B* are cross sections of the hairs of woolly-haired individuals in generations III and IV; *C* shows cross sections of hair from a straight-haired individual. Note that the woolly hairs are flattened or kidney-shaped in outline. A striking feature was entire absence of the medulla in a large majority of the woolly hairs. These characteristics are found in the hair of the short-haired Negro races.

I. H. M. ♀ (1786-1825) was born in V. Askerőya, Dybvaag, Vestagder, Norway. Nothing is now known of her hair type, but the double genetical test clearly demonstrates that she must have been heterozygous for the woolly gene.

Her parents were K. O. ♀ (1747-1813), and M. J. ♂ (1745-1814). The parents of K. O. ♀ were G. T. ♀ from Aas in Vegardsheia, and O. S. ♂ Fosstveit in Holt. The parents of M. J. ♂ were A. M. ♀, V. Askerőya, Dybvaag, and J. B. ♀, Sanden in Dybvaag. The mother of A. M. ♀, T. A. ♀, was from Stordal, Dybvaag, her father, M. I. ♂, from V. Askerőya, Dybvaag. The mother of J. M. ♂, A. M. ♀, was from Risőr, Austagder, his father, B. P. ♂, from Sanden, Dybvaag. All these ancestors of I. H. M. ♀ have typically Norwegian names, a fact which, in connection with their residence, indicates that her family descended from Norwegian farmers without any traceable foreign intermixture.

I. H. M. ♀ was the fifth in a family of seven. Only one of her brothers and sisters, A. M. ♀ (1781-1852), married, and she gave birth to five children, all non-woolly. No woolly-haired individuals have occurred among their descendants (see pedigree, Figure 3). I. H. M. ♀ herself married in 1807 M. T. ♂ (1768-1810) and gave birth to two daughters, both woolly-haired. From the first of these, H. M. ♀ (1807-1880) the woolly gene is transmitted to the descendants belonging to line 1, while the other had three non-woolly daughters whose children were all smooth-haired. In her second marriage, in 1812, with T. A. ♂, I. H. M. ♀ gave birth to five children, of whom two daughters had typically woolly hair. Through these daughters the gene is transmitted to the following generations of line 2 (see Figure 3).

The pedigree presented needs no further comment. No case of intermarriage has occurred within the family lines studied. The expectation for a dominant character is accordingly in marriage of woolly × normal-haired: woolly and normal-haired individuals in equal numbers. Twenty such marriages occur in our material. In a total of 83 children 38 were woolly-haired and 42 non-woolly, while for 3 the hair type is unknown, a result that is in perfect accordance with expectation.

For a survey of the not very comprehensive data on the inheritance of the

curled hair type in man, the reader may be referred to Landauer[6] and Fischer.[4] Though there are still points which need further elucidation, there seems to be consistent evidence to the effect that the curly hair type generally behaves as a dominant trait. This statement seems to hold true both for crosses of ulotrichous races × smooth-haired Arians and for some curly hair types among Europeans. In the ulotrichous races apparently more than one factor is involved. Crosses involving Mongols, Indians, etc., which are characterized by a special type of straight hair, are here disregarded.

Concluding Remarks

The evidence presented above demonstrates that the woolly hair type described represents an unusually "good" dominant character in man. A photograph like the one reproduced as frontispiece illustrates the Mendelian segregation with "text-book" clearness.

In one respect the case is particularly interesting: The character complex frizzly hair type with flattened oval hair form and shortness of the hair is just the one which is typical of the ulotrichous races. In our case this complex is inherited as a clear cut dominant. Apparently in race crosses the analogous complex is also inherited as a dominant trait.

Any intermixture of negro blood may, in our material, be safely excluded. Not only is it apparent from the pedigree data covering seven generations that the family is of Norwegian farmers descent, but the clear "Nordic" type of the family members (see Figure 2) also excludes any assumption of distant race crossing. Even nowadays, with the highly developed means of communication, a negro is practically never seen in Norway. And the occurrence of a negro × white cross or a hybrid × white crossing of this order more than seven generations back is for social and other reasons so improbable that it may safely be left out of account. Such an assumption is also ruled out by the genetic evidence involving the wooly character itself, since this character is remarkably constant in its somatical manifestation through succeeding generations, while the same is not true of the analogous hair characters of the ulotrichous races which appear modified in succeeding hybrid generations.

We arrive, accordingly, at the conclusion that a hair character which in main features parallels those present in the ulotrichous races, must have occurred in a European stock as the result of an independent mutation.

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Computing Correlations

THE C-D MACHINE CORRELATION CHART, *designed by* Edward E. Cureton and Jack W. Dunlap. Pad of twenty-five charts with directions, $1.00. The Macmillan Company, 1932.

THIS is a double entry frequency table with formulas and directions for computing the correlation coefficient and associated constants. It employs the scheme of using the sum and dif-

ference as auxiliary variables, thus furnishing a check on the calculations. The novel feature is the blue and white checker-board design for decreasing the chance of error in summing the diagonal frequencies. The general effect is pleasing to the eye. The printing is neat and legible. There is an error in the lower left hand corner of the chart which would lead a novice to enter the first Y-interval one line too low.

The directions for computation could be improved by both additions and omissions. Of the former, two are especially important. First, directions for plotting the paired observations and entering the resulting frequencies should be included; an experienced clerk to whom the chart and directions were handed was unable to start the scoring until this point was explained. Second, a workout example would add greatly to the clarity of the explanations. As to the omissions, they might well extend to most of the details for setting up the computation on Monroe and Marchant machines. I say this both because operators usually know how to handle the machines if the objective is clearly stated and illustrated; and also because key driven machines, with which many computing rooms are equipped, are just as well adapted to this work as the crank driven machines.

The availability of calculating machines raises the question as to the desirability of such a chart for computing correlation. It is well known that machines can be readily employed to handle the original data without the intermediation of the chart. The accompanying table* displays the results of a time study recently made in our laboratory to determine the most efficient method for handling correlation calculations.

These are conservative estimates of time. While no attempt was made to attain extreme speed, every effort was put forth to make the estimates comparable. All operations were repeated for verification unless they included self-checking devises.

This table indicates that correlation charts will not ordinarily be used if machines are available.

George W. Snedecor.
Iowa State College.

*NUMBER OF MINUTES REQUIRED TO COMPUTE CORRELATION COEFFICIENTS

	Number of pairs of observations					
Method of computation	50	100	200	300	500	1000
Correlation chart	41	56	87	118	180	335
Original data with calculating machine	22	38	70	103	167	330
Punched card tabulating machine	22	32	51	69	102	191

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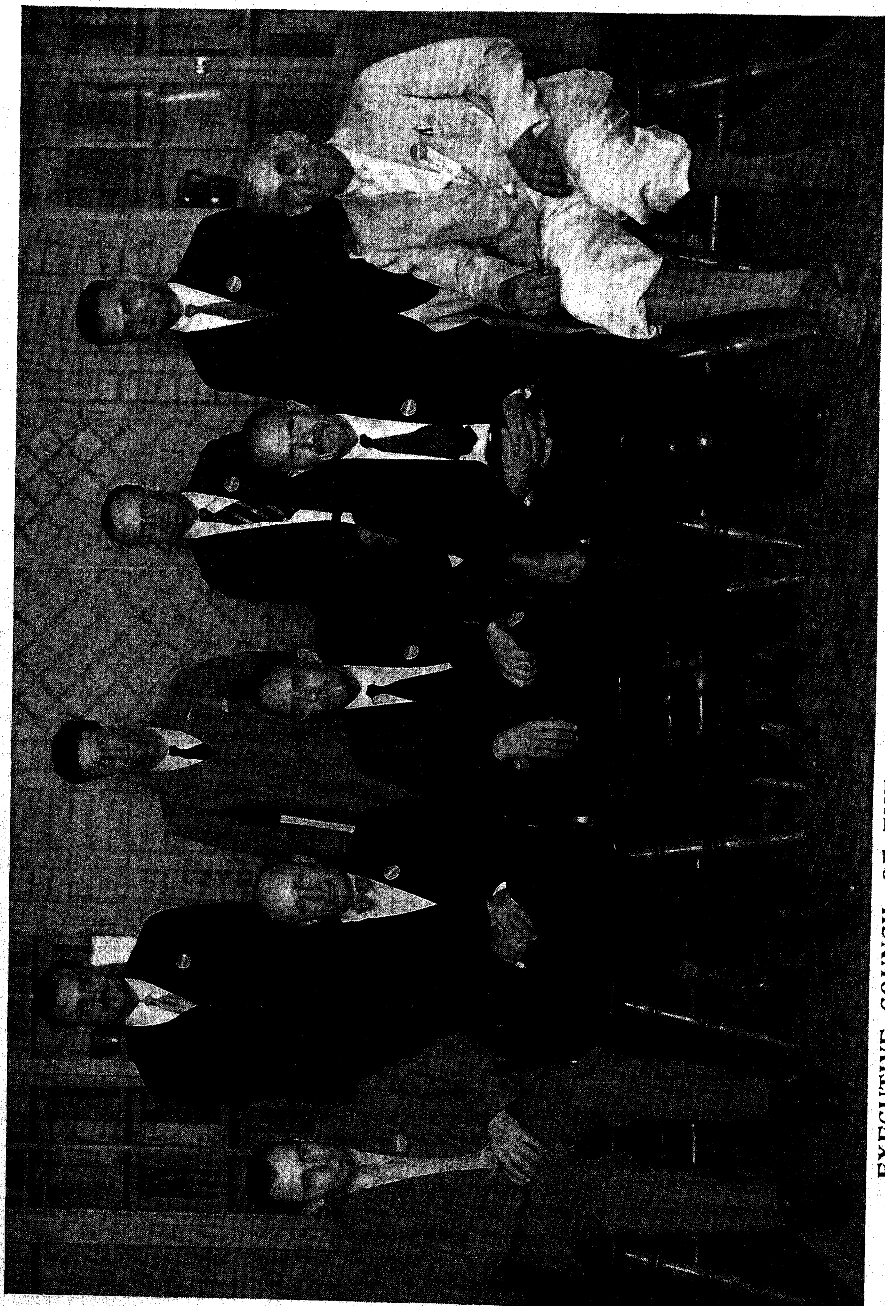


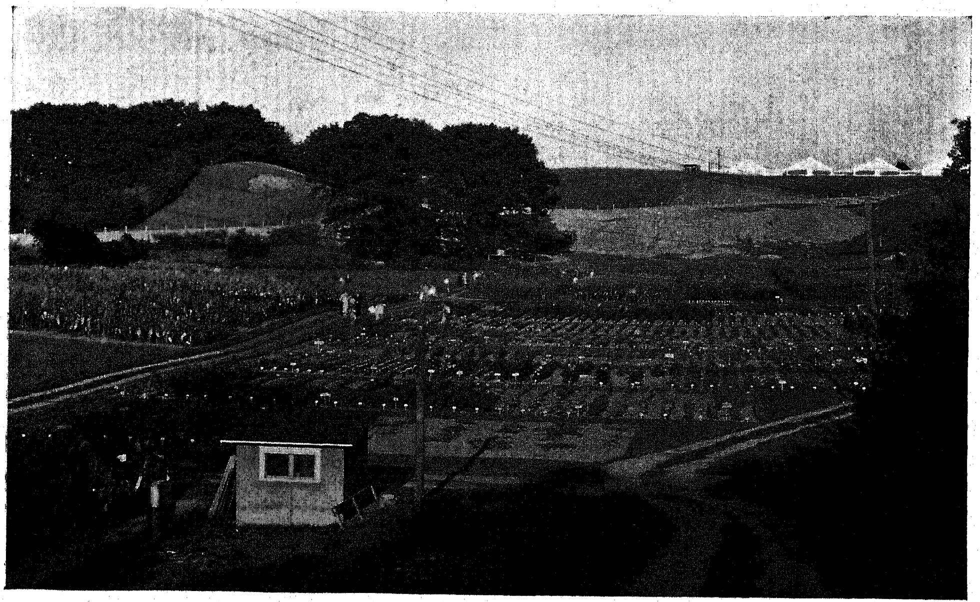
EXECUTIVE COUNCIL OF THE SIXTH INTERNATIONAL GENETICS CONGRESS

Figure 9

C. C. LITTLE *Secretary General* — L. C. DUNN *Transportation Committee* — D. F. JONES *Publications Committee* — M. DEMEREC *Exhibits Committee*

R. C. COOK *Treasurer* — E. M. EAST *Program Committee* — T. H. MORGAN *President* — C. B. DAVENPORT *Finance Committee* — R. A. EMERSON *Local Committee*

THE GENETICS CONGRESS

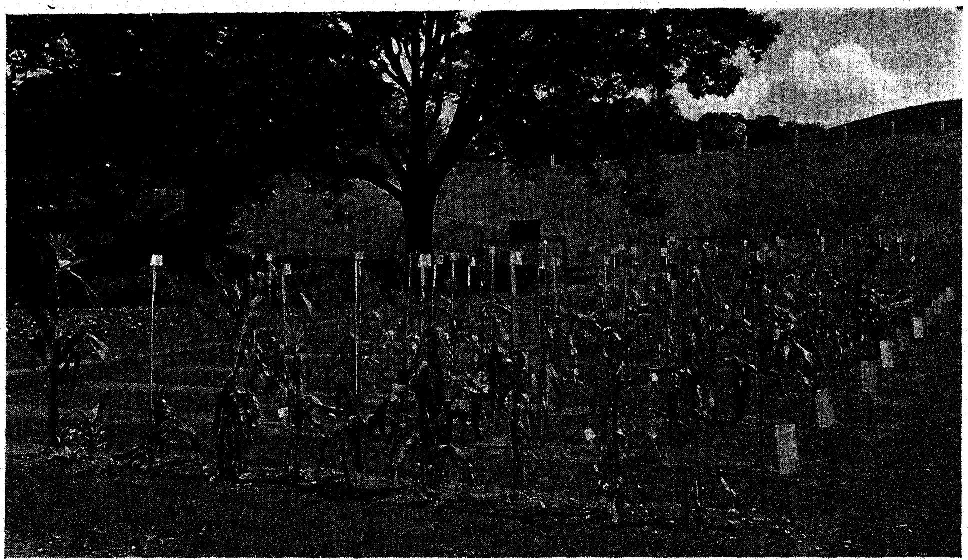


THE LIVE PLANT GARDEN
Figure 10

General view of the live plant exhibit, showing material used in genetic research. This garden was one of the outstanding features of the Congress. Its preparation involved more than two years' work, as trial plantings of many of the forms had to be made last year at Ithaca, to give information on the correct time to plant to have the exhibits in best condition during the last week in August. Without this preliminary work many of the exhibits would have been a failure because of too-early or too-late maturity of the plants. The exhibit committee's foresight resulted in a really remarkable proportion of the exhibits being in excellent condition at the time of the Congress.

RARELY does the date of an international scientific gathering coincide with the announcement of the discovery of important new facts or principles. When such announcements are made at a scientific meeting, as was the discovery of insulin in 1923, the significance of the discovery may not be realized at the time. In that instance the scientific world woke up only some weeks later to the fact that a very important advance in medicine and physiology had been announced. Thus to say that no revolutionary advances in genetic concepts were given to the world at Ithaca may prove to be an actual misstatement of fact. In any event it by no means implies that the Sixth International Genetics Congress was not a scientific and social success of the highest order.

To give a really coherent account of the important points of such a Congress is much like a small boy trying to give the folks at home an adequate idea of the wonders of a three-ring circus. So much was going on of great interest in so many places at once, that any single individual could get at best only a very sketchy view of the whole. The writer has asked at least a score of people what they considered to be the outstanding feature of the Congress. Very few were so clear on this point and they were able to give an immediate and unequivocal reply. Most of these nominations for first place were different, but it is safe to say that the ex-



Photograph by Troy Studios

A LIVING CHROMOSOME MAP

Figure 11

The live plant exhibit had so many interesting features that it is hard to single out the outstanding exhibit. Possibly this living chromosome map of maize deserves that honor. In it were arranged living examples of the end-effects of the genes at present known and mapped (or at least located on a given chromosome) in maize. The labels at the ends of the rows listed the genes in each row.

hibits of living plants used in Genetic research and the other very extensive exhibits were accorded a prominent place in the minds of those who attended the Congress. Often this attitude was stated defensively, for it was evidently felt that the distinguished speakers and the very stimulating discussions should be conceded first place. Those who considered that the exhibits stood first felt that most of the outstanding points in the discussion could be read about and pondered over later, but nowhere else could one have seen such an array of the actual material on which Genetic research is based. The arrangement of the program so that ample time was allowed for the exhibitors to be present, and to demonstrate the media in which their researches were conducted, greatly added to the value of the exhibits.

In a science dealing with abstract principles such as mathematics or (recent) physics, one might question whether an International Congress could possibly yield a return in any way commensurate with the great effort and expense involved. In a science like genetics, dealing in the last analysis with living organisms, a congress that brings together the actual basic material of the science fills a need that perhaps cannot be met in any other way. Speculation is one of the most important and dangerous tools of science. Unsound speculation is perhaps held in check by the presence of such overwhelming living evidence of how much more there is to be known!

The publication and program committees of the Congress deserve special praise for the arrangement of the meetings. Abstracts of all papers to be delivered at the special section meetings of the Congress were placed in the hands of members when they registered. This volume of over four hundred pages represents about half of the published proceedings of the Congress.

Its preparation and publication in time for the opening of the Congress was a noteworthy accomplishment. The program of the Congress, compiled by the Program Committee and the Local Committee, was a model of clarity and conciseness. With these two volumes as a guide the members were in excellent position to make the most effective use of their time, and to attend the meetings and discussions of most interest to them. So much ground was covered simultaneoulsy at the afternoon section meetings that only the conventionally depicted ultra-specialist could have been altogether satisfied that he was present at all the meetings he wanted to attend.

Advance Abstracts

A criticism has been offered that it would have been much more convenient to have had the abstracts one or two weeks in advance of the Congress. The obvious advantage of such an arrangement commends it as an ideal; one suspects that Dr. D. F. Jones, on whose shoulders fell the preparation of the Abstract volume, would question whether it could be put into practice. We understand that the final proofs were released to the printer just eight days before the Congress convened. The actual preparation of a book of this size in so short a time and its delivery to Ithaca was a remarkable feat. Whether the work of preparation could have been further accelerated, or started earlier, to make earlier publication possible, is a practical problem rather difficult to answer.

Another feature of this method of pre-publication of abstracts, which has interesting implications, is that many papers were submitted for presentation at the Congress, but because of the unavoidable absence of the authors these papers were not presented. A technical question might be raised whether such abstracts were legitimately a part of the Congress Proceedings. From a practical point of view this is not serious, as those actually taking part in the Congress will be listed in the final volume of the Proceedings and the inclusion of papers submitted but not read makes the Proceedings of the Congress a more complete record of genetic thought and progress in 1932. A number of the newspaper announcements of the Congress were based on these abstracts, and as a result not a few events were chronicled that in a strict sense never really happened.

The frontiers of genetics are expanding so fast that the principle of publication of abstracts may, for practical reasons, have to be permanently adopted. The writer has heard regrets expressed that all papers are not to be published in full in the proceedings. This is an ideal of perfection but mechanical and financial difficulties make such an undertaking almost out of the question. It was requested that the abstracts submitted represent about three per cent of the total length of the papers. One suspects that this percentage was somewhat exceeded, but nevertheless the publication of the complete papers would fill at least four 500-page volumes. Certainly such an explosion of genetic literature would represent rather an alarming aftermath of the Congress, could it be financed.

Social Activities

Certain schools of sociological thought teach that all men are equal units and one is just like another. To people who actually believe this, the personal equation should be a matter of no concern. In Genetics, where all our experience teaches us that each individual is a unique and unduplicatable phenomenon, the kind of people who are engaged in genetic research is of utmost interest and importance. After attending a gathering of this kind, meeting many interesting and stimulating people, all different; and, by the Grace of What Gods there are, getting actually acquainted with a few rare and unique personalities, one wonders more than ever how anyone could be such a fool as actually to maintain the each-person-

GROUP PHOTOGRAPH OF MEMBERS OF THE SIXTH INTERNATIONAL GENETICS CONGRESS AT ITHACA

Figure 12

Of the 544 members registered at Ithaca, 389 are shown in this photograph, which was made Friday morning, August 25th, on the West side of the Armory at Cornell University. Below is a key giving the names of those in the photograph. While the registered membership at Ithaca was only 544, the total attendance was considerably greater. At the picnic at Taughanock Falls the attendance was about 800. The total paid membership of the Congress was 813. In spite of the unfavorable economic conditions and the adverse exchange existing at the present time, there were 101 members present from other countries.

Dr. Morgan has his hand on one of the numerous dogs which are a feature of the campus at Cornell. Some of these, in their un-self-conscious canine way added a truly informal touch to some of the functions. Photograph by Troy Studio.

1 F. A. E. Crew
2 F. B. Hutt
3 Katharine S. Brehme
4 R. G. Jaap
5 Edward J. Wenstrup
6 Sara F. Passmore
7 J. L. S. Simpson
8 Florence L. Barrows
9 Helen Besley
10 Helen Houghtaling
11 Solomon Horowitz
12 G. L. Slate
13 W. H. Alderman
14 John T. Bregger
15 David H. Thompson
16 A. P. French
17 Glen Salisbury
18 E. E. Heizer
19 Kenneth L. Turk
20 Stuart N. Smith
21 Jack Shultz
22 L. J. Stadler
23 A. C. Fraser
24 T. H. Morgan
25 R. A. Emerson
26 F. P. Bussell
27 C. C. Hurst
28 J. W. Gowen
29 E. H. Gay
30 E. W. Lindstrom
31 Anastasia J. Romanoff
32 N. Dobrovolskaia-Zavadskaia
33 Mrs. F. W. Herriott
34 George Haines
35 Th. Dobzhansky
36 L. C. Dunn
37 Lua A. Minns
38 Lillian Phelps
39 R. J. Kamenoff
40 Marcus M. Rhoades
41 Barbara McClintock
42 Virginia H. Rhoades
43 Harriet B. Creighton
44 Luther Smith
45 L. H. Newman
46 G. H. Cutler
47 W. W. Worzella
48 W. R. B. Robertson
49 F. A. Hays
50 Mrs. F. A. Hays
51 B. Rosinski
52 Zenas H. Ellis
53 Myron Gordon
54 J. B. S. Haldane
55 L. R. Waldron
56 E. F. Gaines
57 Margaret Gaines
58 Mrs. J. Rheinheimer
59 J. Rheinheimer
60 George H. Shull
61 G. P. Frets
62 N. R. Speiden
63 C. F. Feng
64 E. S. McFadden
65 Matthew Fowlds
66 Curt Stern
67 Mildred Hoge Richards
68 C. D. Darlington
69 O. L. Mohr
70 Mrs. O. L. Mohr
71 Mrs. Barbara Davis
72 Mary Crawford
73 H. Timofeeff-Ressovsky
74 Mrs. Gertrude Lindegren
75 Carl C. Lindegren
76 F. S. Howlett
77 W. P. Spencer
78 Noel L. Bennion
79 S. J. Holmes
80 C. L. Huskins
81 L. C. Glass
82 S. M. Saenko
83 Mrs. S. Belfield
84 Roy E. Gibson
85 A. Richards
86 H. H. Newman
87 C. R. Stockard
88 A. H. Estabrook
89 Lois Lampe
90 W. D. Merrell
91 E. B. Babcock
92 L. H. Snyder
93 Mrs. L. H. Snyder
94 Corrado Gini
95. A. Vandel
96 A. Ghigi
97 R. Goldschmidt
98 L. G. Kulkarni
99 C. Stuart Christian
100 L. Gordon Miles
101 E. Chroboczek
102 P. C. Ma
103 John H. Schaffner
104 Mrs. John H. Schaffner
105 Yun-Kuei Yang
106 C. H. Chung
107 E. A. Lods
108 D. W. Robertson
109 W. J. Sando
110 W. H. Leonard
111 T. R. Stanton
112 G. F. Sprague
113 A. A. Bryan
114 Florence Stuck
115 W. G. Einsele
116 Mrs. T. H. Morgan
117 E. M. Vicari
118 Catherine V. Beers
119 Mary B. Stark
120 G. O. Hall
121 W. A. Maw
122 Maurice Proulx
123 J. L. Lush
124 D. C. Warren
125 Charles W. Upp
126 H. B. Goodrich
127 J. W. Mavor
128 Raymond T. Moyer
129 C. C. Kwan
130 C. K. Parris
131 F. D. Richey
132 R. A. Fisher
133 Alexander Weinstein
134 Daniel Raffel
135 Mrs. A. Vandel
136 H. R. Hunt
137 Kurt Hubert
138 A. E. Brandt
139 P. W. Gregory
140 G. L. Stebbins, Jr.
141 A. P. Saunders
142 A. B. Stout
143 C. G. Bowers
144 J. T. Buchholz
145 G. W. Woolley
146 H. O. Hetzer
147 M. T. Macklin
148 N. I. Vavilov
149 E. F. Grossman
150 D. D. Whitney
151 S. I. Kornhauser
152 W. T. M. Forbes
153 J. H. Gerould
154 N. Timofeeff-Ressovsky
155 F. R. Immer
156 L. R. Powers
157 Jane Spier
158 Marie Hearne
159 J. S. Bangson
160 J. P. Kelly
161 C. D. Gordon
162 Arthur B. Chapman
163 John H. Quisenberry
164 Walker M. Dawson
165 William H. Eyster
166 W. T. Macoun
167 R. Summerby
168 H. Nachtsheim
169 Harry Federley
170 Kristine Bonnevie
171 Mrs. Hansen
172 C. H. Mahoney
173 Mrs. Helen D. Hill
174 J. Ben Hill
175 J. Sanders
176 E. J. Gumbel
177 Rene Vandendries
178 A. Zulueta
179 Jacques Rousseau
180 L. J. Cole
181 E. N. Wentworth
182 F. B. Morrison
183 C. J. Lynch
184 Wilhelmina F. Dunning
185 A. Schmid
186 E. W. Sinnott
187 A. F. Blakeslee
188 R. Ruggles Gates
189 H. H. Strandskov
190 Edward C. Colin
191 M. N. Weismann
192 Nathan Kaliss
193 A. C. Scott
194 Haig Dermen
195 G. D. Durham
196 Reginald H. Painter
197 B. Nebel
198 Lillian Hollingshead Hill
199
200 R. J. Garber
201 Max M. Hoover
202 J. A. B. Nolla
203 V. W. Jackson
204 John T. Crofts
205 W. H. Longley
206 A. Franklin Shull
207 J. T. Patterson
208 D. E. Lancefield
209 F. A. Varrelman
210 Herbert S. Warren
211 Virgene Warbritton
212 G. Pincus
213 C. B. Davenport
214 R. S. McEwen
215 E. C. MacDowell
216 E. M. East
217 Fred N. Briggs
218 J. L. Collins
219 E. Eleanor Carothers
220 P. W. Whiting
221 Roger de Vilmorin
222 J. W. MacArthur
223 R. K. Nabours
224 John J. Bittner
225 Wm. H. Gates
226 Mrs. Wm. H. Gates
227. Ruth Marshall
228 O. Winge
229 A. M. Banta
230 S. C. Harland
231 O. E. White
232 R. A. Brink
233 D. N. Shoemaker
234 Paul A. Warren
235 A. F. Swanson
236 E. P. Hume
237 O. D. Smith
238 T. Hawryluk
239 G. A. Lebedeff
240 C. W. Metz
241 Ralph E. Cleland
242 Karl Sax
243 B. O. Dodge
244 B. L. Warwick
245 Mrs. Adeline Van Lone
246 E. E. Van Lone
247 J. E. Bowstead
248 D. G. Steele
249 H. R. Albrecht
250 D. C. Cooper
251 Ruth H. Lindsay
252 P. H. Senn
253 N. P. Neal
254 M. C. Parker
255 Henri Prat
256 L. C. Strong
257 J. W. Lesley
258 H. U. Good
259 E. H. Hart
260 H. D. King
261 L. F. Whitney
262 E. G. Ritzman
263 Paul Popenoe
264 Mrs. W. E. Castle
265 Mrs. F. S. Tulloss
266 Beatrice Johnson-Little
267 Robert Cook
268 Hugh C. McPhee
269 Elmer Roberts
270 B. P. Kaufmann
271 Burch H. Schneider
272 L. R. Watson
273 J. R. Livermore
274 R. B. Hinman
275 W. Neely
276 Arturo Roque
277 Carlos A. Krug
278 Tage Kemp
279 J. Clausen
280 L. C. Thomas
281 W. F. Hanna
282 L. E. Kirk
283 O. McConkey
284 G. P. McRostie
285 T. R. Wood
286 Mary J. Brown
287 Mary Eleanor Davis
288 Donald W. Davis
289 Martha H. Scott
290 Donald F. Jones
291 M. Demerec
292 R. G. Wiggins
293 A. M. Brunson
294 S. G. Smith
295 H. D. Goodale
296 H. E. Warfel
297 Mrs. B. P. Kaufmann
298 Ernest C. Driver
299 A. E. Clarke
300 Alan Deakin
301 A. H. Hersh
302 T. W. Whitaker
303 Ladley Husted
304 Wilbur M. Luce
305 Merle T. Jenkins
306 W. R. Singleton
307 T. L. Smith
308 H. M. Showalter
309 S. H. Yarnell
310 H. L. Ibsen
311 Albert Lorz
312 Alfred Marshak
313 S. O. Burhoe
314 P. C. Mangelsdorf
315 Edgar Anderson
316 Kenneth Kopf
317 W. J. Duchemin
318 W. E. Castle
319 C. C. Little
320 N. F. Waters
321 C. V. Green
322 Horace Feldman
323 J. M. Murray
324 J. B. Park
325 A. E. Waller
326 Alan Boyden
327 H. H. Plough
328 A. H. Sturtevant
329 C. R. Plunkett
330 E. W. Erlanson
331 H. A. Senn
332 Eva M. Tully
333 S. H. Emerson
334 Ruth E. Lenderking
335 Emerson G. Knowles
336 Philippe L'Heritier
337 Charles Zeleny
338 M. R. Irwin
339 J. I. Kendall
340 G. D. Snell
341 H. Bentley Glass
342 E. M. Perry
343 M. M. Perry
344 Mrs. B. O. Dodge
345 Hally J. Sax
346 Wm. H. Brittingham
347 S. K. Ru
348 Otto S. Margolis
349 A. L. Baron
350 George P. Child
351 L. H. Hamilton
352 M. H. Harnly
353 R. O. Earl
354 E. Grace White
355 M. A. Hayden
356 L. W. Taylor
357 N. A. MacRae
358 T. J. Arnason
359 W. H. McGibbon
360 Howard B. Frost
361 Floyd Ingersoll
362 W. V. Lambert
363 C. R. Burnham
364 F. A. Coffman
365 A. J. G. Maw
366 F. B. Meacham
367 J. B. Cotner
368 C. H. Bostian
369 R. A. Derick
370 A. G. Whiteside
371 J. R. G. Sutherland
372 Edward W. Shrigley
373 H. J. Fitzpatrick
374 W. G. McGregor
375 J. M. Armstrong
376 G. H. Stringfield
377 W. K. Smith
378 P. B. Sawin
379 F. H. Clark
380 S. C. Reed
381 Everett B. Clark
382 R. Cumming Robb
383 F. A. Krantz
385 Elliot
386 T. M. Currence
387 A. N. Wilcox
388 G. H. Rieman
389 Herbert P. Riley

a-robot-unit view. The real value of such a Congress is, after all, in what it teaches us of the uniqueness and importance of the individual.

Thus the social activities of the Congress constituted one of its most notable features. For these Ithaca is marvelously adapted. It is isolated enough to prevent too many diversions, and yet the country round about is so varied that the members of the Congress were continually stimulated by the new scenes and situations. Had it not been for the Local Committee, these might have gone unappreciated, but so fine a job was done in organizing the social side of the Congress that many other things than the scientific meetings will long be remembered by those who went to Ithaca the last week of August, 1932.

What did the Congress tell us of future progress in genetics—after Dr. Morgan had had his guess before the gathering convened? Oceans of words were spilled in formal and informal gatherings to discuss the vital question: "What is the Gene?" but that important entity is still illusive. Perhaps in 1937 the answer may be forthcoming. The other angle is the relation between genetics and cytology which is becoming so close and so fruitful. The notable work of maize geneticists and cytologists in linking gene-behavior and observed chromosome behavior is an outstanding example of this fruitful trend. Another development is tracing through the long history of the gene from fertilization to the adult organism, discovering how it controls development. In this a beginning only has been made as in the recent work on the lethal effects of the "creeper" gene in fowls. Of this more will perhaps be heard in later Genetics Congresses, which if not held too often, as Dr. Morgan wisely counselled, should neither be held too long apart! —R. C. C.

THE EUGENICS CONGRESS

THE Third International Congress of Eugenics opened with an excursion to Cold Spring Harbor on Sunday, August 21. There members of the Congress were guests of the Carnegie Institution of Washington. The Station for Experimental Evolution and the Eugenics Record Office were thrown open to the inspection of the members, and lunch was served by friends of the Institution resident in the vicinity.

The general session on Monday morning was devoted to a discussion of problems of immigration, of ways to encourage the reproduction of the fit, and to discourage the reproduction of the sub-normal and defective. These problems are, of course, the basic problems of Eugenics. Their practical solution is by no means simple. No easy way has been found to limit the families of those in the lower mental brackets. This is the paradox that threatens to keep birth-control dysgenic, in spite of the hopes of its votaries for the discovery of a technique that any half-wit *can,* and *will,* use. In spite of considerable discussion this three-headed eugenic cat still remains unbelled.

The afternoon was devoted to section meetings at which were discussed the eugenic implications of race differences, variation, mate selection, fecundity, and the socially inadequate. Much interesting information on various phases of these problems was presented, but one was left with little in the way of a conviction as to "What to do!" The noise of street-cars, trucks and elevated trains was overwhelming, and this feeling may have been due to the general hubbub, rather than because the moral was not plain. One wondered somewhat about the wisdom of staging a discussion that hinges in the last analysis on *people,* in the midst of so many *things* that connected thought was almost beyond the realm of possibility.

In the evening a dinner was held at

the Museum, and following this the General Assembly of the Congress. R. A. Fisher transmitted a note of greeting from Leonard Darwin, son of Charles Darwin, and for many years president of the British Eugenics Education Society. This was followed by Dr. Davenport's presidential address, and by addresses by the Honorary President of the Congress, Henry Fairfield Osborn, and by Dr. Corrado Gini of Rome. Following this there was a reception and inspection of the remarkable Eugenics exhibit which is one of the outstanding features of the Congress.

On Tuesday the General Morning Session was devoted to a discussion of Eugenics in relation to Education, Marriage, and War. At this meeting Dr. Madge T. Macklin made what many consider to have been the most important practical suggestion advanced at the Congress. She urged that the medical curriculum be revised to the extent that medical men will have an adequate grounding in the basic principles of genetics. She considers it especially important that in the final year of the medical course, after the students have learned of the symptoms of disease, they be given a course in the genetics of human diseases.

Popenoe's account of the Family Relations Institute at Los Angeles left it plain that to have any value whatsoever, a eugenic outlook must be inculcated early in life. To try to do this in the last days or weeks before marriage, is years too late.

The afternoon sections were devoted to physiology and sanitation; society in relation to eugenics; and to general discussions of the problems of eugenics.

The extensive exhibit of material bearing on the subject of eugenics and human heredity is one of the outstanding contributions of the Congress. This will be open to the public throughout September, and will be viewed by many people. These exhibits represent a tremendous amount of painstaking work. Anyone who takes the time to go through the exhibit in detail cannot but be impressed with the great amount of very fruitful work that is being done in working out the details of human heredity.

Other "Stigmata of Degeneracy"

Editor, THE JOURNAL OF HEREDITY:

IN the August, 1932, number of THE JOURNAL OF HEREDITY, I note the editorial comment at the bottom of page 317. "Stigmata of Degeneracy," as applied to straight and concave scapulae would be softened by the addition of the word "physical" as I was in the habit of doing for many years before becoming an *Emeritus* Professor of Surgery. People who present stigmata of physical degeneracy are to be found among our most brilliant mental groups—the orchids who blossom gloriously while the root is dying.

I have not read the article by Dr. W. W. Graves, of St. Louis, in the *London Eugenics Review* for October, 1931, but it seems far from being news to surgeons that people with "gun stock scapulae" in professional parlance, are among those who die comparatively young and who are "unfit" so far as the term may be applied to physical survival.

The gun stock scapula is indeed an index to human unfitness in physical connotation of the latter term and it seldom occurs singly as a "mark" which is perhaps a less offensive word than "stigma"; the latter having sinister significance in some of its applications. In people with the concave scapula in particular we may find as many as three or four other marks of physical decline in a single individual, perhaps even more than that in a highly responsive girl who is the "life of the party" and at the head of her class at school.

In the course of my lectures in surgery I have called the attention of the class to this point very many times and we have set out to look for as many "stigmata" as might be found associated in some one individual. For example—a high-arched palate with or without other defects of the superior maxillary bones, facial asymmetry, defective mandible, irregularities in contour of the helix of the ear, Mosher's short sternum, a kidney or kidneys pulled out of Gerota's capsule by a dragging colon, acute-angled rib insertions, preputial adhesions in girls especially, coccygeal dimple, undeveloped mammary glands, male or female hirsute pubic arrangement in the wrong person—the latter point sometimes allowing us to make an estimate as to the individual's probable behavior as a social unit. Dysfunction of the endocrine glands is a common accompaniment of tangible defects of physical structure, confirming the old idea that structure and function belong together.

Your question as to the wisdom of the cover-title under which Dr. Graves' article appears had not occurred to me at all because "Stigmata of Degeneracy" would be conventional in medical writings but I am glad that the idea of "misuse of terms" has been brought forward in this connection. We may properly agree that the word "physical" or "mental" as the case may be, should qualify the word "stigmata," and "decline" is a more gracious word than "degeneracy" for the heading of an article. Unfit people are extremely sensitive over that sort of classification in a case of physical defect in particular, and as a physican I have many times tried to give comfort with the statement that mental superiority is one of Nature's compensations for physical deficiency. I have never know moral deficiency to cause any pangs of grief.

Robert T. Morris, M. D.
140 E. 54th St., New York City.

It appears that Dr. Graves has gone a good deal farther than most surgeons in tracing the development of the scapulae from early embryonic stages, and in adducing evidence to show that scapula-shape is a constant characteristic, throughout life — apparently rather strongly inherited. Apparently my strictures regarding the use of the term "Degenerate" were not quite clear. Graves has shown that statistically certain shapes of scapulae are associated with prospects of survival. By no means *all* of Graves' "scapoid types" fail to reach three score years and ten. That is, some people with concave shoulder blades do survive and apparently show no other evidences of "degeneracy." The question of whether to use the unpleasant words "stigmata" and "degeneracy" in a specific case or even where there is an absolute correlation is distinctly another matter from what I had in mind—the casual use of such an opprobrious term when to use it is both unscientific and very questionable taste. A note by Dr. Graves in the succeeding number of the *Eugenics Review,* and a personal conversation with him, makes it plain that he altogether concurs in the view which I took.—Editor.

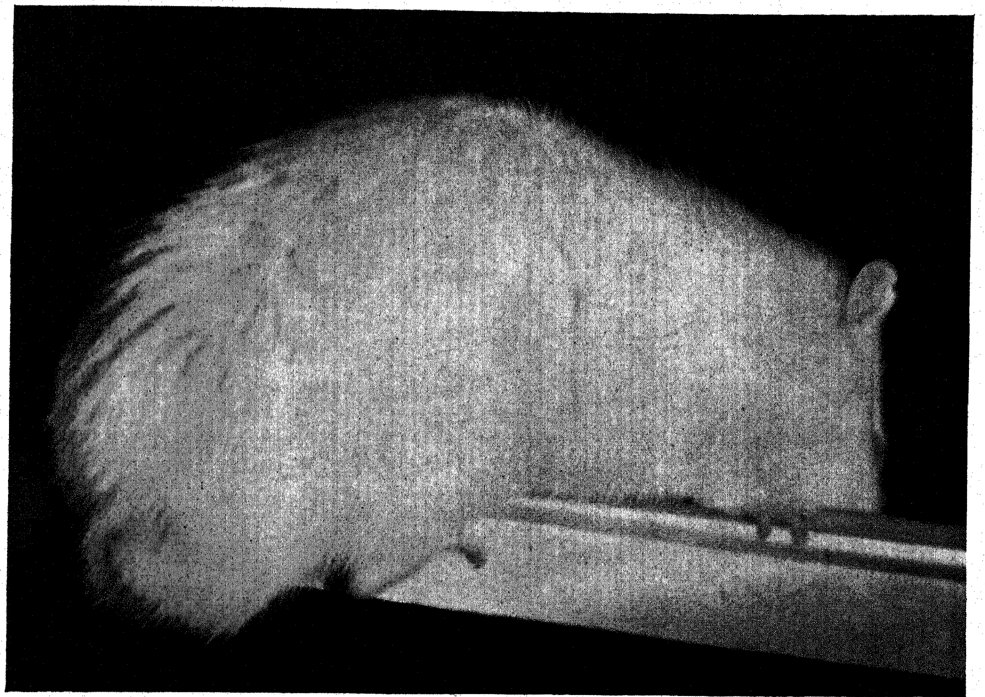
Errors in Bud Mutation Article

Attention has been called to two errors in the article on Bud Mutation in the Apple in the May Journal of Heredity. On page 217, legend to Figure 9, cuts *D* and *F* are interchanged. *F* shows the Dorrence Mutation in Connecticut and *D* the Dickey Mutation in Oregon. The second error is in *Literature Cited,* No. 9. Bregger's paper was presented at the 1930 meeting of the Wash. Hort. Assoc., but published in 1931.

CONGENITAL TAILLESSNESS IN THE RAT*

F. B. Hutt and O. Mydland

University of Minnesota, St. Paul, Minnesota



TAILLESS IN APPEARANCE BUT GENOTYPICALLY TAILED

Figure 13

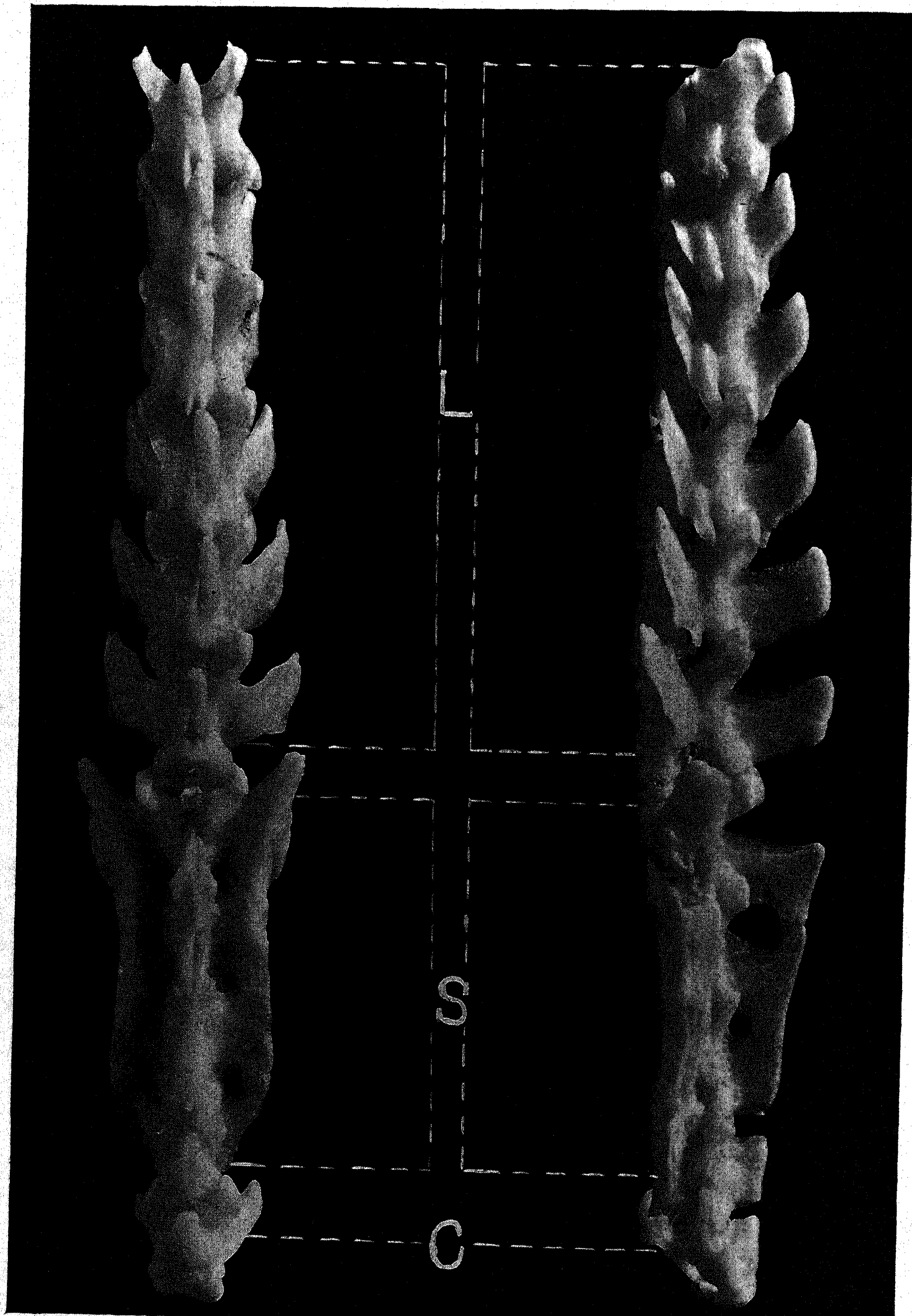
The 229 descendants of this rat all had conventional tails. This fairly conclusively proves that the loss of this rat's caudal appendage was due to accidents of development rather than to any change in the animal's genetic constitution.

IN January, 1931, through the kindness of Mr. Ivan G. Grettum, the writers of this note secured a tailless male albino rat. (*Rattus norvegicus* Erxleben.) This animal had appeared in the rat colony of the Champion Animal Food Company of Minneapolis.

Neither at birth nor when full grown (Figure 13) did the rat have the slightest external evidence of any tail. By the absence of this appendage the animal's scrotum was made much more conspicuous than in tailed rats.

Tailless albino rats were described by Conrow,[1,2] but up to the time when this specimen was secured no breeding trials had been reported, so that it was impossible to say whether this condition arose from genetic causes or from some accident during development. The latter possibility seemed the more likely in view of the rarity of the tailless condition at the Wistar Institute as reported by Miss Conrow. On the other hand, Lang[12] has reported the investigations of Nägeli which made it evident that in the closely related house mouse, *Mus musculus*, the tailless or short-tailed condition is hereditary. In the house mouse this abnormality is apparently due to a

*Paper No. 1072 of the Journal Series of the Minnesota Agricultural Experiment Station.



VERTEBRAE OF TAILLESS RAT

Figure 14

Posterior end of the vertebral column of the tailless rat; dorsal view on the left, lateral view on the right. *L*—6 lumbar vertebrae; *S*—4 sacred vertebrae fused to form the sacrum; *C*—single caudal vertebra. Twice natural size.

dominant gene which is lethal in the homozygous state.

The special interest attached to the comparative genetics of related species made it desirable that the tailless rat should be submitted to a thorough genetic investigation. Through the kindness of Dr. L. S. Palmer the necessary facilities were provided in the nutrition laboratories of the Division of Agricultural Biochemistry at this institution.

Unfortunately the mother of the tailless rat had been destroyed, so that a back-cross to her was impossible. However, in breeding tests with unrelated stock, the following results were obtained:

F_1:	49, all with normal tails
F_2:	157, all with normal tails
Back-cross to tailless:	23, all with normal tails

These findings are in complete agreement with those of King[8] which appeared when the investigation herein reported was nearly completed. In her tests no tailless rats appeared in F_1 or F_2 from matings of three tailless females and two tailless males to normal sibs, while in a crucial test of tailless male $\times$ tailless female, only tailed rats appeared in 25 F_1 and 132 F_2 animals.

It is evident that the defect is not a genetic character. The experiments of Conrow[2] showed that no post-natal accident could amputate the tail so closely to the body that the vertebral column would be terminated within the pelvic girdle as was the case in her tailless rats and in the writers' specimen. Congenital taillessness in these animals was therefore caused by some accident during gestation, presumably an arrest of development at a time when the primordium of the caudal vertebræ was at a critical period in its development.

That this occurrence is rare is shown by the small number of cases observed in the Wister Institute colony, given by Miss King[8] as follows:

Period	*Number of rats raised*	*Number tailless*
1907 to 1917	over 71,000	11
1918 to 1930	over 100,000	5

Prof. H. M. Evans has advised us, in response to an inquiry, that only two tailless animals have been encountered among about 60,000 rats raised during fifteen years in the colony of the Department of Anatomy in the University of California. Congenital absence of the tail is evidently a rare occurrence in the rat.

The posterior end of the vertebral column of the tailless rat reported in this paper is shown in Figure 14. The pelvic girdle was quite normal and was removed before photographing to permit a better view of the sacrum. Normally this species has 6 lumbar, 4 sacral and about 30 caudal vertebræ. This specimen had all the lumbar vertebræ and a normal sacrum of 4 vertebræ. In addition there was present a single caudal vertebra which had well formed transverse processes and prezygapophyses, but was badly misshapen at its distal end.

Of five tailless rats described by Miss Conrow, only one had any caudal vertebræ and in that case, as in the writers' rat, the single caudal vertebra was deformed. Her other four cases lacked from one to all four sacral vertebræ and one even lacked the sixth lumbar vertebra.

Discussion

The vicissitudes besetting the vertebrate tail are of various kinds. The appendage may be shortened after birth by accidents or enemies, and many a puppy has been deprived of the pleasure of chasing his tail because that organ has been trimmed down to meet the Procrustean dictates of the show bench and of the dog fancier.

Similarly, albeit less harshly, the tail may be removed or amputated by an accident in development prior to birth. Such non-genetic congenital taillessness is rare in the rat, but that it may be more common in other animals is indicated by Dunn's report[5] of 10 such cases among 10,170 fowls. Two instances of congenital taillessness in cattle have been reported to the senior

author, and there are occasional references to the same condition in horses, sheep and swine.

If animals which should have tails can be deprived of them through accidents during development, there is compensatory justice in the occasional persistence of embryonic tails at birth among animals in which tails are decidedly superfluous. The list of such occurrences in man numbers over eighty-five. Their distribution reveals that the event may even occur in Tennessee and shortly after the Scopes trial!

Finally, hereditary modifications of tail-length are common. The genetic nature of the tailless or "bob-tailed" condition in the Manx cat has been established by several investigators. Less well known is the evidence of Godron[6] and of Klodnitzky and Spett[9] for a dominant gene or genes shortening the tail in the dog. Experiments of Nägeli (reported by Lang[12]), Dubosq[4] and Dobrovolksaia-Zavadskaia[3] make it clear that there is a hereditary type of taillessness in the house mouse and that it is dominant and lethal in the homozygous state, but the exact genetic basis for the condition has not yet been definitely determined. Landauer and Dunn[10] and Landauer[11] have found that in fowls there are two types of hereditary rumplessness as well as the non-genetic congenital rumplessness referred to above.

In addition to these specific cases, most geneticists would agree that genes are responsible for the differences in tail length which are characteristics of certain species. Within the Rodentia one finds that the number of caudal vertebræ may vary from 30 in the rat down to the rudimentary tail of the guinea pig. Similar differences are found in the Primates. Even in closely related species, and within a species, there may be great hereditary differences in length of tail. For example, in sheep the number of caudal vertebræ varies in so-called tailless, short-tailed and long-tailed varieties from three to twenty-four. Genetic factors affecting the caudal vertebræ are obviously among those which have played a conspicuous rôle in the evolution of species.

Since several of the mammals have achieved apparent taillessness by the acquisition of genes which inhibit development of the tail beyond the embryonic stages, it is not impossible that hereditary taillessness may yet be found in the rat. During the course of this investigation several lady members of a class in nutrition expressed the hope that a race of tailless rats might be produced. Their interest arose from the feeling that if the rat could modify his long snaky tail, replacing it with either a short one like that of the rabbit or none at all, like the guinea pig, their aversion to handling the animals would be overcome, and, as a result, they would enjoy more fully certain phases of a laboratory course in nutrition.

However desirable from this standpoint a gene for taillessness might be, there is reason to believe that such a mutation could not be utilized to add to the prestige of the laboratory rat. Investigations with short-tailed mice show that homozygosity for short tail is lethal and that there is apparently a reduction of viability in the heterozygote. Evidence of reduced viability in short-tailed cats is presented by Hind[7] and by Schwangart and Grau[13], while the report of Klodnitzky and Spett[9] suggests that some short-tailed dogs are similarly afflicted. If a reduction in vigor is inevitably associated with a reduction in the tail, that character would be undesirable in most laboratories, even if the rat could attain it.

Because hereditary taillessness effects vitality, it does not follow that accidental congenital taillessness must have the same effect. However, Miss King[8] reports that the tailless rats studied at the Wistar Institute were somewhat below the average in size, vigor and fertility. These defects were not noted in our specimen. The average

size of 10 litters from him was 7.2, which is slightly above the average for the albino rat. It is possible that females may be more adversely affected than males. Several years ago there appeared in the rat colony of the Division of Agricultural Biochemistry here, the only tailless rat observed in that department among a number estimated to be over ten thousand. The pelvic girdle of this animal, a female, was so deformed that she could not give birth to her litter. That such a condition is most unusual is shown by the fact that Miss King got several litters from four tailless females.

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Books Received

BOOKS are acknowledged in this column as received, and such acknoweldgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the Journal *will be reviewed in later numbers.*

THE SOUTH-AMERICAN SWAMP BEAVER (NUTRIA), Care, Feeding, Breeding, and General Management. *By* Ad. R. Walther, Ph.D. and D. V. T., Professor of Agricultural High School, Hohenheim, Germany. Second Edition. 108 Illustrations. Pp. 128. English Version by C. R. Partik, Lantier, Quebec. Verlag F. C. Mayer, Munich, Germany. 1931.

A South American fur bearer, new to the breeders in the United States, makes his bow by way of Germany.

PSYCHOLOGY AND PSYCHIATRY IN PEDIATRICS: THE PROBLEM. Report of the Subcommittee on Psychology and Psychiatry, *by* Bronson Crothers, M. D. Chairman, White House Conference on Chilc Health and Protection. Pp. 146. Price, $1.50 The Century Co., New York. 1932.

Another publication of the White House Environmental Conference.

SCIENCE TODAY AND TOMORROW, Compiled from a Series of Lectures Delivered at Morley College. Pp. 196. Price 8/6. Williams & Norgate, Ltd., London. 1932.

Psychology, anthropology, biology, botany—essayed by authorities, with varying degrees of general interest and lucidity.

THE METHODS OF STATISTICS, An Introduction Mainly for Workers in the Biological Sciences. *By* L. H. C. Tippett, M. Sc., Statistician to the British Cotton Industry Research Association. Pp. 222. Price, 15/. Williams & Norgate, Ltd., London. 1931.

Mr. Tippett is not a biologist, but he has given biologists much to struggle over.

EVOLUTION YESTERDAY AND TODAY. *by* Horatio Hackett Newman, Ph. D., Professor of Zoology, University of Chicago. Pp. 171. Sixteen Chapters. Price, $1.00. Century of Progress Series. Williams & Wilkins Co., Baltimore. 1932.

Dr. Newman discusses other matters than twins.

RECENT ADVANCES IN CYTOLOGY, *by* C. D. Darlington, D. Sc., Ph. D., Cytologist, John Innes Horticultural Institution. Pp. 8 Plates, 109 Text Illustrations, 65 Tables. Price, $4.00. Philadelphia, P. Blakiston's Son & Co., Inc. 1932.

The graft-hybrid between genetics and cytology has already shown thrilling evidences of heterosis. Darlington brings up to date the ground already covered and discusses the implications of this work in a manner that probably will not be universally accepted, but, as Haldane says in the Introduction, can hardly be ignored.

SCIENCE AND HUMAN EXPERIENCE, *by* Herbert Dingle, D. Sc., Asst. Professor of Astrophysics, Imperial College of Science and Technology; Hon. Secretary, Royal Astronomical Society. The Macmillan Company. Price, $1.75.

Physics, the exact science, seems to be leaving biology far behind when it comes to speculative and essentially unverifiable hypotheses. Dingle considers this an advance. To a biologist it smacks of metaphysics.

PLANTS, WHAT THEY ARE AND WHAT THEY DO, *by* A. C. Seward, Sc. D., D. Sc., LL. D. Master of Downing College and Professor of Botany, Cambridge. Pp. 141. Twelve Chapters. Price, $1.50. Cambridge, England; At the University Press. New York; The Macmillan Company. 1932.

The problem of utilizing solar energy was solved by the "invention" of chlorophyll, these many ages ago. We still depend on plants for the energy that keeps us animals going. So we, as a species, *ought* to be interested in what Dr. Seward has to say.

HANDBUCH DER BLUTGRUPPENKUNDE, *by* Dr. Paul Steffan. 669 Pp. Nine Chapters. Price, 48 marks. Lwd. Mk. 50. J. F. Lehmanns Verlag—Munchen. 1932.

Our knowledge of the blood groups will soon fill an encyclopedia, it seems.

JOBS FOR THE COLLEGE GRADUATE IN SCIENCE, *by* Edward J. V. K. Menge, Ph. D., Sc. D., Director of Department of Zoology, Marquette University. Pp. 175. 4 Parts; Appendix A and B. The Bruce Publishing Co., New York. 1932.

If Dr. Menge *knew* where jobs were to be had this book ought to have a tremendous sale. He lists many of the possibilities in Federal and State governments, and elsewhere, but right now this doesn't mean very much. In the case of the Federal Government, at least, vacancies cannot be filled without specific approval of the President, which some graudates in science may find hard to obtain.

PRIMITIVE SECRET SOCIETIES, A Study in Early Politics and Religion, *by* Hutton Webster, Ph. D., Professor of Social Anthropology in the University of Nebraska. Pp. 243. 11 Chapters. Price, $3.00. (Second Edition, Revised). New York, The Macmillan Company. 1932.

Politics and religion are two subjects eugenists must learn a good deal more about before the "factors under social control" are much applied with an eye to racial welfare. Perhaps this book might serve as a primer.

HUXLEY, *by* Clarence Ayres. Pp. 254. 9 Chapters. Price, $3.00. New York, W. W. Norto & Co. 1932.

Evolution's Saul of Tarsus held up to the mirror of biography.

NEW JERSEY'S AGRICULTURAL EXPERIMENT STATION, 1880-1930. *by* Carl Raymond Woodward *and* Ingrid Nelson Waller, with an Introduction by Jacob G. Lipman. Pp. 645. 25 Chapters. New Jersey Agricultural Experiment Station, New Brunswick. 1932.

The daddy of all of the State Experiment Stations celebrates its jubilee.

MENTAL AND PHYSICAL TRAITS OF IDENTICAL TWINS REARED APART

Case VI. Twins Ada and Ida

H. H. Newman,
University of Chicago

SEPARATED THIRTEEN YEARS*

Figure 15

Twins, Ida (left) and Ada (right) when they were eighteen years old. Separated at three years, they remained apart until they were sixteen. Ada had been married a year when this picture was taken, and Ida did not marry until fifteen years later. Their close resemblance and other criteria of identity leave no question that they are identical twins.

THIS case is of special interest in that the women are the oldest twins reared apart that have been reported. They were separated at about three years of age and are now fifty-eight years old. The period of complete separation was that from three years to sixteen years. Since sixteen years of age they have seen a good deal of each other, sometimes living together for

months at a time. Ada married at seventeen years of age and Ida married sixteen years later, at thirty-three. Ada has reared five children, Ida four. Ada was separated from her husband after ten years of unsuccessful effort to reform him. Since then she has worked hard to support her children and deserves a great deal of credit for her success in rearing them all to be good citizens. Ida is still living with her husband, who seems to be a man of good character. Both women are devoted mothers. They say: "Our children love us anyway, so there is much to be thankful for."

The early life of these twins was very similar, extremely hard and cruel in both cases. Their father, at the time when the twins were still infants, developed inflamatory rheumatism and became partially blind. He drank a great deal and left his family permanently when the twins were two or three. Misfortunes made it necessary for the twins to be taken care of by relatives of the mother, Ada and Ida living with different families. Neither family wanted the children and the result was they were both neglected and badly mistreated. Both women tell a most tragic story of hardship and abuse, which we must omit from this account because the details are too personal. The main point for our purposes is the fact that their social environment throughout childhood was about the same and about as bad as it could be. Under these circumstances it is very significant that both women have overcome their handicaps equally well and have grown up to be highly respectable persons with rigid moral codes, good average intelligence, and good manners and speech. They both have deep-seated prejudices and both are somewhat bitter about life in general, but this is not surprising considering their wretched childhood.

The educational record of both twins is about the same. Neither one received more than third grade schooling and even this was interrupted and unsatisfactory. They have learned to read largely through their own efforts, and both read somewhat slowly. This slowness caused the various mental tests to drag out and cover much more time than is usual in these cases. The tests were also more tiring to the twins, especially to Ida, than in any other cases studied.

The social and educational environments being so nearly the same for these twins, what may be offered as environmental differences? So far as I can see, the only differences of any consequence are two: *a*, the earlier marriage and unhappy married life of Ada as compared with the late marriage and placid married life of Ida; *b*, Ada has lived far more in cities than Ida, who until recently has lived mostly on farms. This latter difference will be discussed later in connection with Ida's goiter.

Proof of Monozygotic Origin

When these children were born the doctor couldn't tell them apart, so he marked one with pink and one with blue beads. Later their mother had to sew pink and blue buttons and ribbons on their garments in order to distinguish them. When they were old enough to speak and heard people ask which was which, Ida would say, "I know I am Ida because I have pink buttons on my bonnet." When Ida visited Ada after the latter had several children, these children could not tell their Aunt from their mother. At the present time, apart from the fact that Ida has a prominent goiter and Ada has none, the women are still extremely similar in features, voices, manners and disposition. Table I gives their physical characteristics. The only pronounced difference is one of weight, Ida being over 19 pounds heavier and showing somewhat pronounced myxedema as the result of hypothyroidism. They would still be recognized by a close observer as identical twins.

The palm and finger print data are also conclusive evidence for monozy-

THE TWINS AT FORTY-FIVE
Figure 16

In spite of being separated the twins have had a very similar environment. The most marked differences have been in age of marriage and in marital happiness. Recent examination of the twins at the age of 59 shows that they have become less similar in recent years. Probably this is due to the fact that Ada has developed a goiter, while Ida has not. A lack of iodine in Ada's diet is believed to have been the cause of this.

gosity. The finger-print formulae are as follows:

Ada,
Left: U.U.U.U.U.—Right: U.U.U.W^U.U.
Ida,
Left: U.R.U.W^U.U.—Right: U.U.U.U.U.

Apart from the fact that Ida has a radial loop on the left index finger, a common peculiarity of index fingers, there is strong cross resemblance between the twins. Ada's left is more like Ida's right than like her own right. Also Ida's left is more like Ada's right than like her own right. This is a case of mirror-imaging common in identical twins.

The quantitative values of the patterns are as follows:

Ada, Left=36; Right=40; total=76
Ida, Left=34; Right=36; total=70

This is not so close a correspondence as is found in the most similar identical twins, but is about the average found in fifty pairs. Note again that Ada's left is identical with Ida's right in ridge value.

The pattern and main line formulae for the palms are as follows:

Ada, L: 11.9.7.5′-t-A^u/A^c.O.O.L.M.
Ada, R: 11.9.7.5″-t-A^u/A^c.V.D.L.M.
Ida, L: 11.9.7.5′-t-A^u/A^c.O.D.L.M.
Ida, R: 11.11(10).9(8).5″-t-A^u/A^c.V.d.L.O.

The palm patterns are remarkable in that three out of four are very similar, namely the right and left of Ada and the left of Ida. The odd pattern is the right of Ida. The two left hands are more like each other than like either is to own left hand, a criterion of monozygosity. A notable similarity consists of the presence of a vestigial thenar pattern on both right hands and its absence on both left hands. Thenar patterns are characteristically left-hand patterns, rare on right hands. The presence of such a pattern on both right hands is a noteworthy correspondence. The four palm patterns are reproduced in Figures 17 and 18).

These evidences leave no question as to the monozygotic origin of these twins.

I.—Intelligence Tests

1. *Stanford-Binet Mental Test.*—Both women finished the test in one and a half hours. Ada was the quick-

PALM PRINTS OF ADA

Figure 17

PALM PRINTS OF IDA

Figure 18

The two right hands and the two left hands are much more alike than the respective rights and lefts of either pair. This is strong evidence of origin from a single egg cell. The dermatoglyphic formulae are given in the text.

er in her responses, but used up time in irrelevant comments. Nearly every test suggested to her a Bible text, for she is strongly evangelistic in her religious attitude. Both twins were persistent and were anxious to do as well as possible. The scores were as follows: Mental age, Ada 16 years, 3 months; Ida, 15 years. There is a difference of 15 months, which is nearly twice as great as the average difference of fifty pairs of identical twins reared together. Ada had an I.Q. of 101.5, Ida of 93.7, a difference of 7.8 points, which is about one and a half times as great as the average difference of fifty pairs of identical twins reared together.

2. *The Thurstone Psychological Examination* had to be omitted, because this test is designed for freshmen in college and therefore could not be handled by persons with no more than third grade schooling.

3. *Otis Self-administering Test of Mental Ability.*—In this test the scores were as follows:

	Score	*Mental Age*
Ada	26	10 yrs., 4 mos.
Ida	28	10 yrs., 8 mos.
Difference	2	4 mos.

Contrary to the results of the Stanford-Binet test, Ida is slightly superior to Ada, but the difference is small and probably without any great significance.

4. *International Test.*—This test was completed by Ada, but Ida became exhausted and could not complete it at the time the women had to leave. It would not have been a good test in any case, since Ida, on account of her hypothyroid condition, was too tired toward the end of three days' testing to give a fair account of herself in a mental test.

5. *Stanford Achievement Test.*—The test given was the Advanced Examination, Form Y, for grades 4 to 9. The total scores were as follows:

	Score	*Age Equivalent*	*Grade Equivalent*
Ada	80.5	12 yrs., 7 mos.	6.8
Ida	83.6	12 yrs., 11 mos.	7.2
Difference	3.1	4 mos.	0.4

Once more Ida is slightly superior, but the difference is not great enough to have much significance.

Summary of Intelligence Tests.—Ada was rather definitely superior to Ida on the Stanford-Binet test, but Ida was slightly superior to Ada on the two other tests given. On the whole, it seems fair to rate these two women equal in intelligence.

II.—Temperament—Emotion Tests

6. *Downey Will-Temperament Test.*—The results of this test are shown graphically in Figure 19, the "Will Profile." The scores on the first five traits are very low for both sisters:

Ada .... 1,4,0,2,2=9÷5=1.8
Ida .... 1,1,1,1,2=6÷5=1.2

This means that they are notably slow, inflexible, and far from rapid-fire or mobile in disposition. Ada is, on the whole, quicker and surer in judgments than Ida.

In the next three traits the scores are relatively high:

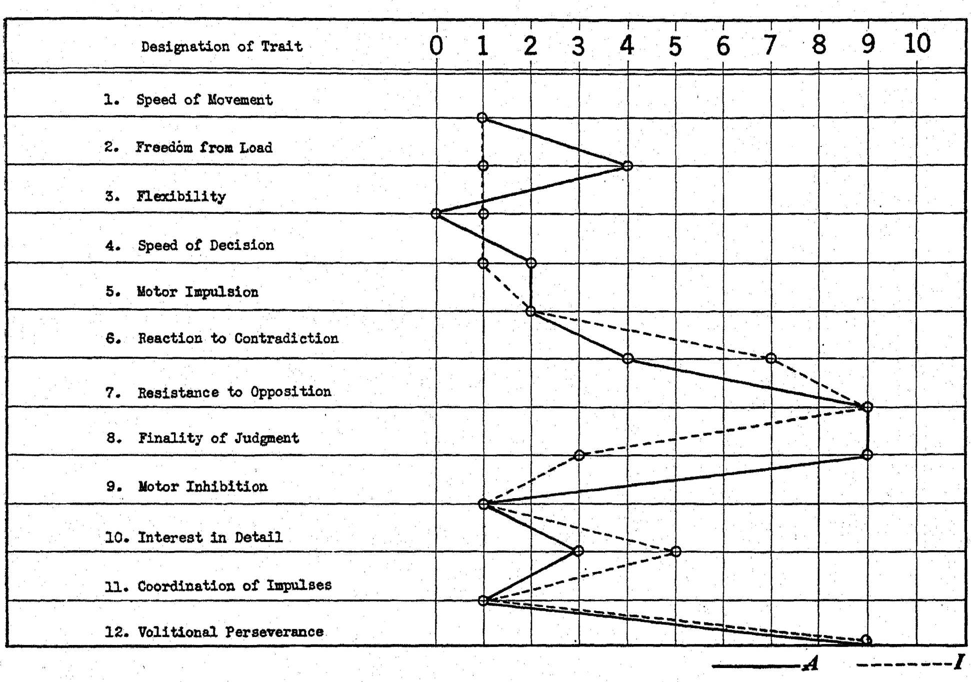
Ada .... 4,9,9=22÷3=7.4
Ida .... 7,9,3=19÷3=6.33

This seems to imply that both are moderately aggressive and forceful persons. This finding is in accord with the fact that, in spite of the most adverse sort of early bad environmental influences, both have grown to be moral and upright women.

In the last four traits they are very similar, high in the same and low in the same traits, with a fair average in the total, as follows:

Ada .... 1,3,1,9=14÷4=3.5
Ida .... 1,5,1,9=16÷4=4

This low total shows that they are not at all controlled, deliberate, careful persons. Ada's total is 45, Ida's 41, both being just above 40, a point below which one might be rated as definitely deficient in will-temperament qualities. The greatest differences are in reaction to contradiction, where Ada scored 4 and Ida 7, and in finality of judgment, where Ada scored 9 and Ida 3. The examiners made the following com-



THE WILL TEMPERAMENT "PROFILE"
Figure 19

Graphic representation of results of Will-Temperament Tests. The picture is one of rather striking similarities. From an educational point of view it is interesting that in spite of an early environment not only adverse, but bad, both twins have grown up to be moral and upright women.

ment, which bears out my own impression:

"Ada is much quicker than Ida in making decisions, and tends markedly to finality in her judgments. Ada appears to be harder driving, less gentle, less flexible than Ida. Ada's low score in reaction to contradiction is probably not significant, for it is part of her code not to engage in 'strife' of an sort."

7. Kent-Rosanoff Association Test.—Ada gave 99 common reactions out of 100, Ida gave 95. Ida gave five reactions so uncommon that they were not recorded in the frequency tables of the author of the test. The mean frequency of all responses of Ada, based on those of "1,000 normal subjects" was 134.5, and that of Ida was 101. This is a fairly marked difference, but is based largely upon five very unusual and far-fetched associations that impress one as an effort to appear unusual. Dr. F. N. Freeman does not consider that this test shows any really significant differences in temperament in these twins.

8. *Pressy X-O Test of Emotions.*—The following table gives the data in compact form:

	Ada	*Ida*
Test I (Dislikes)	36	44
Total deviations	9	8
Test II (Associations)	37	24
Total deviations	5	6
Test III (Things wrong)	76	82
Total deviations	18	11
Test IV (Worries)	62	29
Total deviations	16	11
Total of all deviations	48	36
Total score	211	179

The standard group scores, as made by students, has a median of 230. Neither one is far from normal in this test.

Ada is different from Ida to a significant extent in only one test (Test IV—Worries) where she marked 58 items as compared with only 29 for Ida.

A somewhat more marked difference between these twins appears in the following analysis of Test IV, according to subjects:

	Ada	*Ida*
Suspicious (paranoid)	9	2
"Jumpy" (neurotic)	11	4
Self-conscious (shut-in personality)	13	6
Melancholic	12	9
Hyperchondriacal	13	8
Total of Test IV	58	29

This test seems to indicate a rather marked difference, Ida being much more nearly normal in her emotional reactions than Ada, the latter exhibiting an unexpectedly high number of "abnormal" reactions for one apparently so calm and self-possessed. This is the most marked of the temperament-emotional differences seen in these twins. Dr. Freeman, however, does not consider the differences as very significant, but to me they seem to indicate a moderate degree of divergence and will be tabulated as such when all cases are summarized.

8. *Woodworth-Mathews Test.—Unfavorable Answers.*—In this test the twins showed extraordinary similarity. Both of them gave 17 unfavorable answers of which 10 were identical for both.

Summary of Temperament—Emotional Tests.—One cannot but be more impressed with the similarities of temperament in these two women than with their differences. Yet some of the differences are decidedly marked and cannot be ignored. One must class the differences between them as slight but real.

III. Physical Differences

The outstanding difference between these twins, as in the preceding case (Case V), is one of physical condition. This physical difference is due almost entirely on one feature. Ida has a pronounced goiter and marked hypothyroidism with associated myxedema. Ada says that about thirty years ago she began to develop a goiter but was cured by a homeopathic physician, who gave her some medicine for it. The mother of the twins had a bad case of goiter in later life. Ida shows the typical symptoms of hypothyroidism such as puffiness about the face, general myxedema, increased weight, a tendency to tire easily. She cannot walk more than a block or two without feeling exhausted, and she was made ill by having to climb one or two flights of stairs. Ada has none of these symptoms, though she has had a good deal of illness of other kinds. She is a much stronger, more active, and more vigorous woman than Ida.

The only clue as to why one of these twins should have such a pronounced hypothyroid condition and the other should be entirely free of this condition comes from the data on their modes of life. Ida has nearly always lived in the country close to the Great Lakes, where there is a very high incidence of goiter in country districts, associated with a deficiency of iodine in the soil and the foods. People on the farms get very little food that is brought from other parts of the country and probably use little, if any, iodized salt. Ada has spent most of her adult life in Chicago and other cities and ever since she heard of iodized salt has used it for the sake of the children. It was probably this practice, together with the use of more varied foods, including sea foods, that may be credited with preventing goiter in her case rather than the homeopathic treatment, which she credits with her cure.

Summary of Case VI

These twins have been brought up largely apart from an early age, but their childhood environments were almost as similar as if they had been brought up together. There were differences of some consequence in their married lives, Ada having been married

so much younger than Ida and having many more marital troubles. Both have for a long time been almost fanatically religious, Ada being a Seventh Day Adventist and Ida a Holiness Methodist. Both of them are militantly opposed to the use of tobacco and alcohol. On the whole, I find no significant difference in them intellectually. Both appear decidedly more intelligent than one would expect of people with so little education and such an unfortunate early social background.

They are moderately different in will-temperament qualities when the tests are judged as a whole and when their behavior is taken into account. Ada is a more dominant, assertive and militant sort of person, Ida much more gentle and retiring. Ada took the lead in all arrangements and considers herself the more capable of the two.

The only marked difference between these twins is associated with the presence of a goiter in Ida and its absence in Ada. This one thing makes a vast difference in the physical condition of the two, as has already been brought out. The difference in temperament may also be partly due to this physical difference. The two cases reported in this and the August number of the JOURNAL are distinctly similar in that the only marked difference is in physical condition, with only slight differences in other respects.

Arrangements have been made with three more cases which will be reported as soon as possible. These three new cases will make ten, if Muller's case be included. When ten cases are secured we may feel justified in attempting to correlate the data and attempt to draw some conclusions therefrom. The earlier cases described are all to be found in previous issues of this JOURNAL.

TABLE I—Physical Characteristics of Ada and Ida

Character	*Ada*	*Ida*
Height	167.7 cm.	167 cm.
Weight	208.1 lbs.	227.2 lbs.
Circumference of fore-arm	R, 27.3 cm: L, 27.3 cm.	R, 27.7 cm: L, 27.7 cm.
Length of Head	19.5 cm.	19.5 cm.
Width of Head	13.5 cm.	13.5 cm.
Cephalic index	69.2	69.2
Eye color	deep bluish gray	same
Hair color	iron gray	same
Hair texture	straight and rather coarse	same
Hair growth on forehead	low on forehead in three peaks	same
Hair whorl	clockwise, left of center	clockwise, right of center
Ear shape	long, but shapely	same
Features	regular and rather individual	same except for puffiness about eyes
Goiter	none	very pronounced
Teeth	now has false teeth	has most of her original teeth

Where Ignorance Is Grief

A THOUSAND MARRIAGES, by Robert Latou Dickinson and Laura Beam. Pp. xxv+482. $5. Williams and Wilkins, Baltimore, 1931.

Failure of American civilization to give young people an educational preparation for marriage is the most striking conclusion from this study of case histories from the practice of a prominent gynecologist. High intellect, college degrees, national prominence in a career, are shown to be no bar to ignorance of the elements of physiology and psychology which any 14-year-old boy or girl ought to learn as a matter of course in junior high school. On the other hand, better attitudes and educational preparation, freedom from fears and shocks, and better adjustment go together.

PAUL POPENOE.

WEBBS BETWEEN SECOND AND THIRD TOES

Figure 20

During embryonic development the toes are normally webbed, with an especially marked web between the second and third toes. This webbing is retained in certain families throughout development. This peculiarity is inherited, usually as a mendelian dominant.

INHERITANCE OF "WEBBED TOES"

Two Unusual Pedigrees Showing Inherited Zygodactyly in Man*

W. E. Heston

Iowa State College

THE term zygodactyly, first used by Weidenreich[1] is applied to the anomaly commonly called "webbed toes." The condition involves only the fusion of the skin, and usually occurs between the second and third toes. No variation from the normal bone structure occurs, and usually no variation can be seen in the tendon structure, although in some cases, the tendons of the two toes are slightly fused. According to Schultz,[2] the normal bone structure separates the term zygodactyly from the term syndactylism, which includes the fusion of the bony elements of the foot and hand, sometimes giving the effect of a partial or complete lack of toes or fingers.

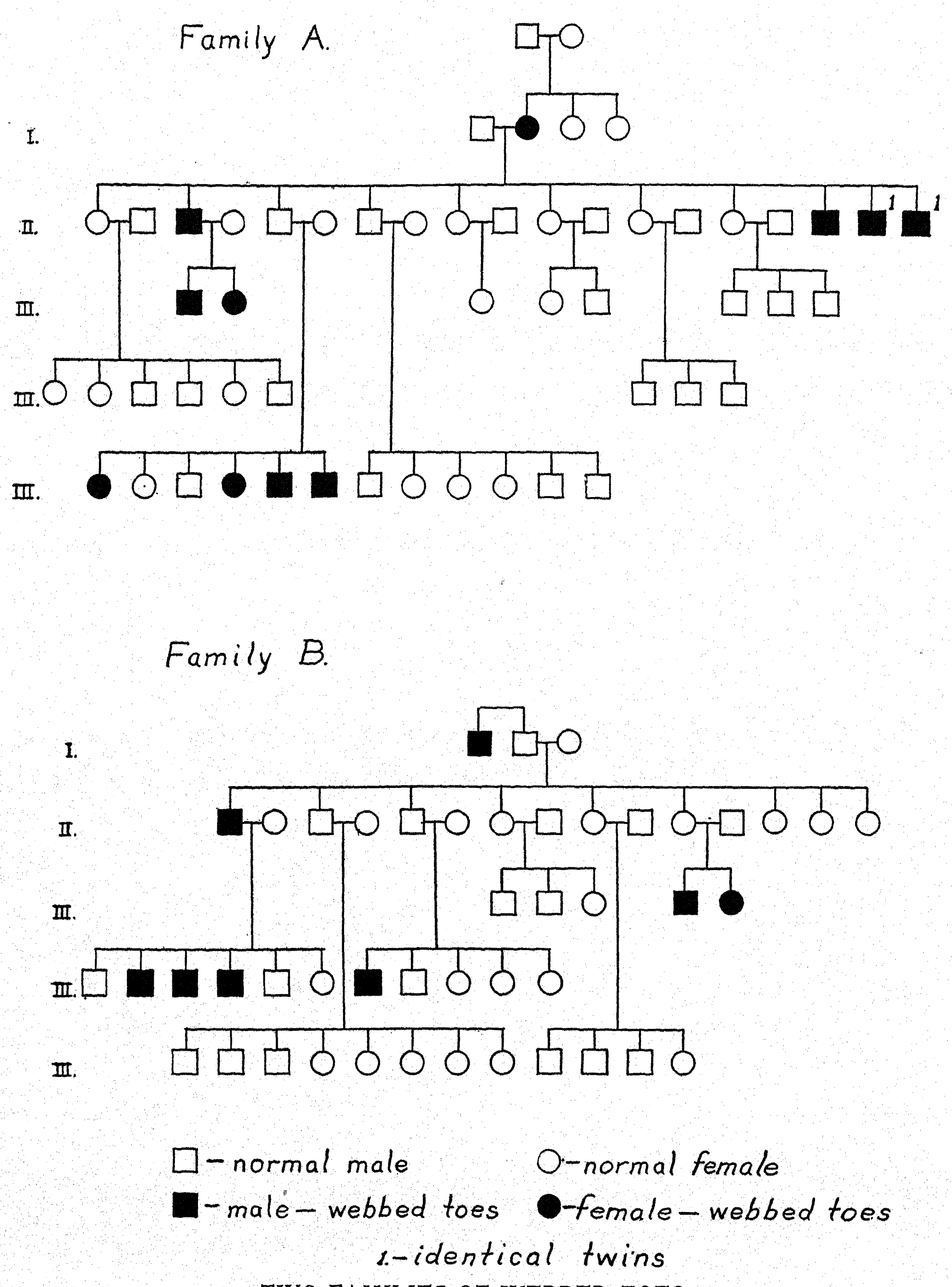
Schultz[2] and Minot[3] report that the condition develops as follows: When the fetus is from 40 to 50 days old the toes normally appear webbed, this webbing being more extended between the second and third toes than between the others. As the toes develop, the webbing becomes less noticeable, until at the age of 60 days there are only slight remnants at the base of the toes. If the web-formation grows outward with the toes, zygodactyly occurs.

This condition is not restricted to man. Schultz[2] states that it occurs frequently among the Siamangs, apes from Sumatra and the Malay Peninsula. In man, however, it is very rare, Schumeier[4] having observed only eight cases of zygodactyly among a total of 20,000 men examined.

Several authors have reported family pedigrees which demonstrate that zygodactyly is an inherited character, but in no case is the manner of inheritance entirely clear. Hurlin[5] published a case in which zygodactyly appeared to be inherited as a typical Mendelian dominant trait. Following this, Schofield[6] presented a pedigree in which zygodactyly was apparently inherited as a secondary sexual character as it appeared only in males and was transmitted only by males. In this case, the web persisted to a greater extent on the right foot than on the left. Wright[7] published a pedigree in which the inheritance appeared to depend on a single dominant gene not influenced by sex. Wright reasoned that the webbing may be determined by different factors in different families. Castle[8] states that aside from the fact that the character is more often reported in males than in females, there is no reason to believe that the character is either sex-linked or sex-limited. He adds that it is a simpler hypothesis to suppose that the character depends on the transmission of a dominant gene located in one of the autosomes than to refer it to a gene in the *X* or *Y* chromosome.

In Figure 21, the writer is presenting two unrelated family pedigrees showing the inheritance of zygodactyly. In Family *A*, no cases of zygodactyly are known back of the mother in generation I. As can be seen in the chart, her father, mother and two sisters were normal. One sister was the mother of seven children and the other the mother of one child. All of these children were likewise normal. In addition, there were three normal half-sisters who produced families of one, two, and eleven children, all children being normal.

*Paper No. 47 from the Department of Genetics, Iowa State College, Ames, Iowa.
Acknowledgment is due Dr. W. V. Lambert for his suggestions, criticisms and supervision of the work.



TWO FAMILIES OF WEBBED TOES

Figure 21

Pedigree charts showing inheritance of webbing between the second and third toes in two families. In both families the character sometimes skips a generation, suggesting that modifying factors play an important part in the development of the webbing.

Four of the eleven children of the affected mother in generation I were zygodactylous, it being especially interesting that two of these four children were identical twins. It should also be noted that a normal son in generation II married to a normal woman transmitted the zygodactyly to four of his six children. Similar cases are to be observed in Family *B*.

In all individuals of these two families having webbed-toes the fusion is between toes two and three. There is, however, a great variation in the extent of the webbing, this ranging from hardly noticeable webbing to the condition shown in Figure 20, the most complete form observed.

The method of inheritance of zygodactyly in these two families is not clear. Most of the evidence would indicate that the character is dominant in nature. It is unlikely that the normal members of the two families transmitting zygodactyly could have married heterozygous individuals; for, as pointed out in Schumeier's investigation, the condition occurs so infrequently. There is no evidence to indicate that the character is sex-linked, or sex-limited, for it is inherited and transmitted by males and females alike. Probably the condition is governed by a single dominant autosomal gene, but if so its expression must be influenced by other genes, as it is not always manifested phenotypically. There are four cases in these two families in which a non-zygodactylous individual transmits the condition to some of his or her children. Evidently in each of these cases the individual in question married a person who contributed the modifying genes causing the condition to be expressed in some of the children.

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Variation in Susceptibility to Infantile Paralysis

WRITING in the *Journal of the American Medical Association* for June 25, John R. Paul, Robert Salinger, and J. B. Trask, report on the simultaneous occurrence of minor illnesses among children in families and in communities where infantile paralysis is epidemic. In 222 families in which a case of infantile paralysis occurred, 35 per cent of the other children had minor illnesses at about the same time. In a group of control families without a case of infantile paralysis, about nine per cent of the children were found to be suffering from a comparable form of "summer grippe." The writers believe that at least part of the higher illness-rate in families having a diagnosed case of infantile paralysis is due to an abortive form of this disease. These possible "abortive cases" of infantile paralysis vary from a few hours of minor indisposition to illnesses of several days' duration with varying arrays of symptoms typical of infantile paralysis. Nasal washings from two cases of such minor illness produced, on inoculation of monkeys, typical infantile paralysis. Negative results were obtained in ten other cases.

It thus appears that infantile paralysis may be a much more widespread disease than has been realized and that it is a disease to which a large proportion of the population are resistant or immune. The fact that in most families where one child has the disease, the others do not, would suggest rather strongly segregation of a mendelian gene. With so erratic and fortunately rare an affliction, it would be difficult, however, to arrive at definite conclusions. In this field also, a study of identical and of fraternal twins might be illuminating.

The possibility that genetic factors are involved is further strengthened by the conclusion of other workers, that a definite type of child tends to be susceptible. These studies are extremely suggestive and of much interest from the point of view of human genetics, as well as from the point of view of the prevention of further epidemics. Draper, who has pioneered in this field, has recently summarized his extensive studies as follows:

"One of the most striking features of the infantile paralysis people is the Mongoloid trend in the palpebral fissure and the not infrequent presence of an epicanthal fold. The latter character, so rarely found in the Caucasian race, appears more often than among the controls. But far more common, indeed almost universally so, is the peculiar smooth downward curving slant of the inner half of the arc of the upper eyelid. This feature is present whether or not the inner canthus is at a definitely lower level than the outer. The latter arrangement produces the well defined slant, downward and inward, of the whole eye slit. This feature may often be present in the poliomyelitis people but not by any means in all cases.—DRAPER, GEORGE: The Nature of the Human Factor in Infantile Paralysis, *Am. J. M. Sc.* 184:115 (July) 1932.

The fact that a significantly large proportion, but not all individuals susceptible to infantile paralysis are of a mongoloid type suggests a genetic linkage between these characteristics, rather than two expressions of the same character. While Draper's results are extremely suggestive, they could hardly be accepted as evidence of more than a need for further intensive study.

R. C.

The

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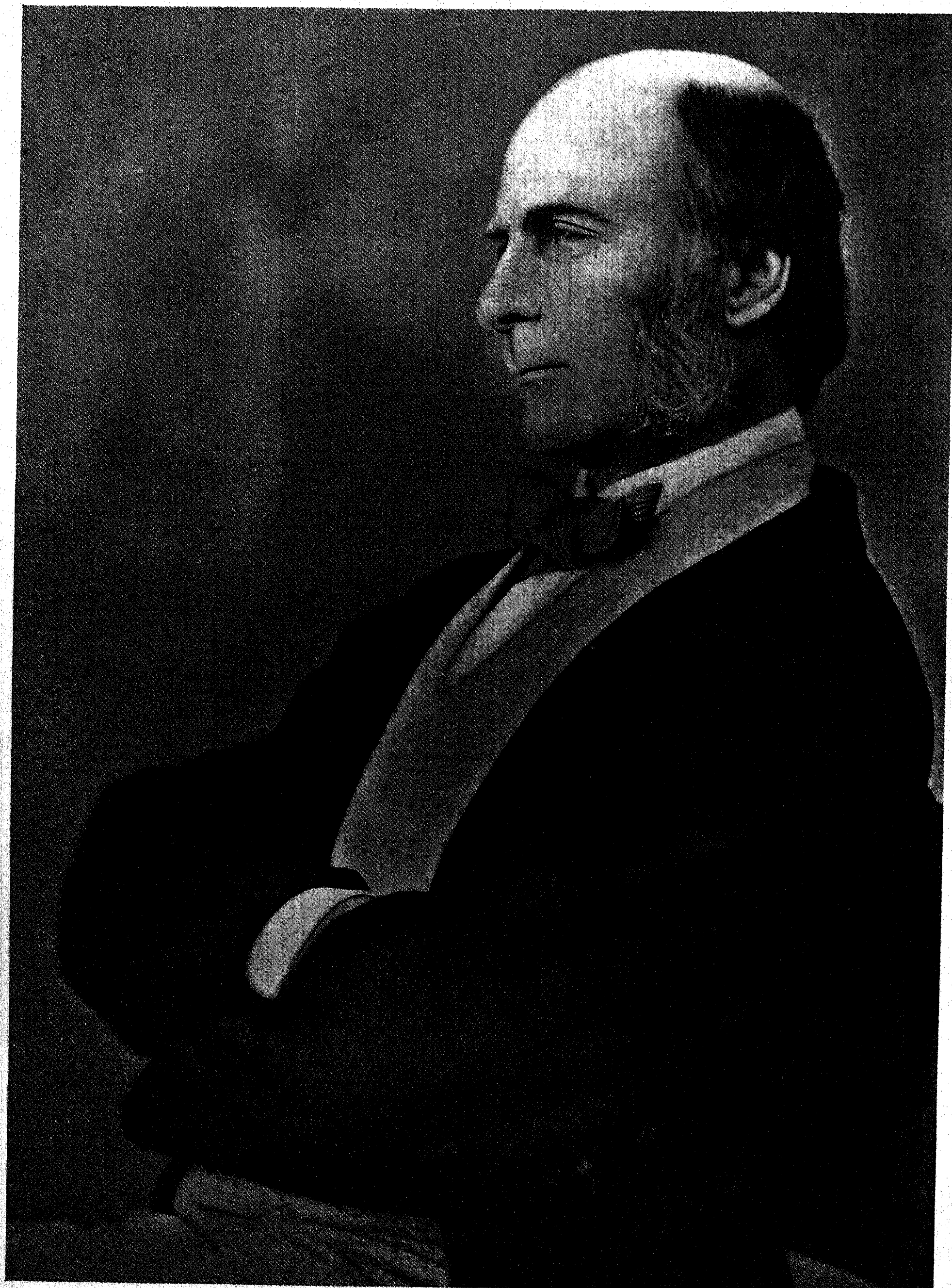


FRANCIS GALTON

1822-1911

Galton made fundamental contributions to many branches of science. The founding of the Eugenics movement is perhaps his greatest achievement. Eugenics he defined, in his will, as "the agencies under social control that may improve or impair the racial faculties of future generations physically and mentally."

ECHOES OF THE EUGENICS CONGRESS

The Trend of Eugenic Thought in 1932 as Reflected in the Papers Presented at the Third International Eugenics Congress

THE following paragraphs have been excerpted from the abstracts of papers presented at the Third International Congress of Eugenics, and from other sources. These excerpts by no means give an altogether complete summary of the subject matter of the Congress. Neither do they give a panorama-in-outline of the entire program, for two important reasons: (1) some of the abstracts did not lend themselves to excerpting and a few were omitted; (2) some of the papers presented were not included in the abstracts. These various views from many angles do, however, give something of a birds-eye-view of Eugenic thought in 1932.

WE are compelled to recognize that medical science, in its humanitarian purpose, preserves in many instances individuals who under the normal biological process of natural selection would be eliminated and cease to be a source of racial weakness. Medical science however need not relax its efforts to cure disease provided that it also seeks effective means of improving the general physical quality and resistance of the race. But unless a race by its own folly took some other short cut to its doom, the gradual lowering of its physical qualities and resistance would seem to be almost the greatest menace to its survival.

If medical science does not address itself equally to sustaining and building up racial qualities and racial vitality and resistance, as well as to individual palliation and relief, it promises to injure rather than improve the racial prospect for survival.

C. G. CAMPBELL.
President, Eugenics Research Association.

THE tendency to self-elimination of particular families may be still further augmented by the small family system if human groups react as do mouse colonies, where it has been found that improved conditions of living increase the incidence of latent defects, such as anaemia and malformations, and consequently cut down the percentage of effective offspring. In any event, the small human family probably provides a less rigorous and stimulating mental and physical discipline than does the large family.

On the other hand, while it may be expected that at the beginning, reduction in the number of offspring will automatically result in the chance elimination of many families from the upper ranks, the same laws of chance will serve temporarily to sustain others of originally no greater potential worth. If among the latter there be the usual amount of selective marriage, a degree of homozygosity may be reached, in a progressively diminishing group, sufficient finally to maintain a self-perpetuating class, even though the individual families continue to be small.

The point of chief theoretical importance is the expectation that in a society where the family is a relatively important feature, general reduction in the number of children promises to act as a significant selective agency in segregating the population into strata much more quickly than would otherwise occur. The average size of its families may consequently become a factor in determining the evolutionary trend of a human community.

C. H. DANFORTH.
Stanford University.

MY firm conviction is that if widespread Eugenic reforms are not adopted during the next hundred years

or so, our Western Civilization is inevitably destined to such a slow and gradual decay as that which has been experienced in the past by every great ancient civilization. The size and the importance of the United States throws on you a special responsibility in your endeavors to safeguard the future of our race. Those who are attending your Congress will be aiding in this endeavor, and though you will gain no thanks from your generation, posterity will, I believe, learn to realize the great debt it owes to all the workers in this field.

LEONARD DARWIN,
Past President of the Eugenics Education Society of Great Britain.

FINALLY, we may inquire: Can we by eugenical studies point the way to produce the superman and the superstate? Progress will come slowly. Man is a poor subject for experimental study; still worse to get to apply to himself established principles. But I think we are justified in having faith that the future will bring precise knowledge in human biology, and education will establish the desired mores.

The past two decades have seen the new Eugenics rise from a mire of ridicule to the solid foundation of a recognized important social factor. It is probable that in the next two decades it will rise still further in public esteem and become regarded as *the most* important influence in human advancement. For, man is an animal, and permanent racial progress in eugenics, must be based on the laws of biology.

CHAS. B. DAVENPORT.
Director, Department of Genetics, Carnegie Institution of Washington.

IN so far as hereditary deforming chondrocysplasia is a relatively common and easily demonstrable affection, it lends itself admirably to study. The disease is a distinct clinical entity, characterized by (1) the occurrence of multiple more or less symmetrical cartilaginous and osteocartilaginous growths within and upon the skeletal system, generally benign, and resulting from a disturbance in the proliferation of the bone-forming cartilage, (2) the occurrence of certain typical secondary distortions and deformities of the skeleton, (3) the demonstration of inheritance in a large proportion of the cases.

DR. ALBERT EHRENFRIED.
Boston, Mass.

THE eugenical ideal must be that every one develops his gifts to enhance the value of his own person, that every one is tolerant with regard to other people's opinions and conduct and that every one is prepared to exert himself in the interest of the whole.

DR. G. P. FRETS.
Holland.

PENNSYLVANIA, because of its geography and industrial history, finds itself with many foci of mental defect. Many methods have been used in efforts to eradicate these foci, institutional care, special classes, supervision, group recreation (to prevent delinquency). Mental health clinics make a special effort at early identification of these defectives. All these methods reach only a limited percentage. The cost of these defectives in the community is constantly mounting, and other methods to eradicate them must be employed.

FLORENTINE HACKBUSCH.
Dept. of Welfare, Harrisburg, Pa.

SCAPULAR types are useful in studies of human inheritance; as morphological constants for correlating coexistent inherited variations, measurements and indices, and as indications of fitness in any people for health, disease, education and duration of life.

To determine who are the unfit, fit or fittest, equal consideration must be given the ever-present variables—inheritance and environment—and the discernible structural inherited variations peculiar to the individual. His family and stock must be recognized, classified, evaluated, and correlated.

The results of investigations, based upon the types of scapulae, justify the inference that the *ideals* of the Physician, Eugenist, Educator, Sociologist and Jurist, expressed *in social and racial improvement,* will be nearer realization through continuous research on inherited variations and fitness problems and that such research will lead to more accurate appraisal of individual fitness for health, disease, education, duration of life, mating and parenthood.

WILLIAM WASHINGTON GRAVES. M. D.
Washington University, St. Louis, Mo.

THE most important causes of death may be conveniently divided into three groups, diseases of middle and old age, in which there is little selective action, and little or no reduction in the death rates, diseases of children as well as of adults, in which there is a moderate selective action, and some reductions in the death rates, and diseases almost exclusively of infancy and childhood in which there is a marked selective action, and in which there is a great reduction in the death rates. As the mortality in infancy and childhood decreases, the infections of the respiratory system become increasingly important. As the etiological factors are to a great extent beyond our control, the very common diseases, measles and epidemic catarrh with their often complicating pneumonia, become the chief agents in the selective elimination process. Different groups living under similar environmental conditions may present marked differences in the adequacy of immunologic reactions to the infectious diseases. These differences are due primarily to the amount of selective elimination of non-resistant strains that has previously occurred.

CHARLES HERRMAN.

MEDICAL science and care, widespread throughout the community, has resulted in survival over the reproductive period of life of many persons hereditarily diseased in mind or body. Figures are given for increasing insanity between 1859 and the present date, increase in certifiable feeble-mindedness in the last twenty years, and lastly, fall in physical vigour as shown by recruiting statistics. This paradoxical phenomenon is a clear example of the contra-selection postulated by Ploetz 30 years ago.

CORA B. S. HODSON.
Hon. Administrative Secretary, International Federation of Eugenic Organizations.

MIGRATION to the north brings the Negro into an unfavorable environment. The climate is against him and tends to aggravate the respiratory infections to which he seems to be peculiarly subject. Most of the northern Negroes live in cities and an urban environment is relatively more unfavorable to them than to the whites. Birth control is extensively practiced by urban Negroes. Its effect is quite evidently dysgenic as it has proven to be among the whites. How extensively it may come to be employed one cannot safely predict. Psychological influences, arising out of unsatisfactory relations to the dominant whites may possibly come to exercise a depressing influence upon the Negro birth rate, especially in urgan communities. If migration redounds to the biological advantage of our Negro population, as it has to many migrating people in the past, the Negroes will be compelled to overcome a number of handicaps and this requires the maintenance of a relatively high birth rate under conditions which will tempt them to restrict it.

S. J. HOLMES.
University of California.

FINALLY, the colonies included families which became differentiated with respect to certain characters, being as remarkable for the lack of certain trait-complexes as they were for the possession of others. Diversity of "stamp" is illustrated by such families as the Adamses, the Lowells, the Dwights, Roosevelts, Wolcotts, Ran-

dolphs and Lees. The personalities which led in our distinctive development have been found to arise from the fortunate crossing of able lines, suggestive of the process we know today as emergent evolution.

This should have for us more than a mere academic interest. For, whatever the present crises, social and economic, may compass by bringing latent powers to the fore, we may not look to them to change materially the basic fabric of our society. That was established long ago in the germinal constitution of our people. How the traits arising from that constitution may be eliminated, conserved, combined and recombined must be the task of our social policies of the future. The present crises cry aloud for effective leadership; failure of such leadership to appear is due to the dying out of highly gifted families, and the low fecundity of others, which though less gifted, would, if producing larger families, increase the chance of producing able individuals. This situation calls for vigorous remedial measures if American institutions are to proceed further along lines laid down by these leaders of the past.

WILHELMINE E. KEY.
Somers, Conn.

FOR ten years the Committee on Exceptionally Able Youths of the Civic Club of Allegheny County, Pa., has been mentally testing the most intellectually promising of the high school graduates of Allegheny County and granting its awards to the highest few. The minimum standard at present is the highest five per cent of the freshmen of an average American college. The number of such awardees on this basis has been 64.

This has given an opportunity to notice whether such attainments are wholly sporadic, directly proportional to schooling, or whether they "run in families" to a significant degree. At every test on the average there are two or more pairs of sibs or cousins included although this would be very rare by chance alone. However, where three sibs have been graduated it has been common for one or more not to be eligible, or not to win an award.

ROSWELL H. JOHNSON.
University of Pittsburgh.

IF the present rate continues, and there is no apparent reason for thinking it will not, by 1934 we will have more than one-half million people in our nervous and mental institutions, alone. One out of every 150 people in the United States during 1930 was a patient in a hospital of some kind. One out of every 290 people was a patient in an institution for nervous and mental disorders. Families that send a child to an institution for feeble-mindedness average twice as many as those who send a child to a university.

In 1928 it was estimated there were 10,000,000 people at large who were socially inadequate. They would include the mentally defective, the criminal, the blind, the deaf, the crippled, the germ diseased, the degenerative diseased, and all other dependents.

Deep seated changes are taking place in the quality and quantity of our people. Some of these changes are beneficial; others are detrimental. Statesmen must take heed of these changes and tendencies. Many of them are fraught with danger to the fabric of our country.

DR. J. H. LANDMAN.
New York, N. Y.

DURING the past ten years much light has been thrown upon the causation of human infertility, particularly in three respects. First, in the typical clinical case the cause is not some single abnormality, but a summation or totality or four or five factors, of which each lowers fertility to some extent while all together depress it below the threshold of conception. Second, in the great majority of cases the responsibility is divided between the two partners; the situation is not, therefore, that of a fertile man married to a sterile woman, or vice versa.

but rather a mating sterile because both of the partners are of subnormal fertility. Third, states of constitutional depression, both endocrine and nonendocrine, are causative factors of infertility fully as important, in the aggregate, as are local abnormalities of the genital organs.

Charles H. Lawrence, Samuel R. Meaker, Allan W. Rowe and Samuel H. Vose.
Evans Memorial Hospital, Boston, Mass.

INASMUCH as the practitioner in his student days has been taught little of inheritance in disease; and has not had his interest aroused as to its significance, he fails most frequently to record his own observations in this field, or if he records them, they may be in such form as to furnish little of useful information to the trained scientists, studying inheritance of disease in man.

Until the medical practitioner is made aware of the importance of heredity as an etiological factor in the production of disease, he will not be apt to have a sympathetic attitude toward public education nor toward any scheme which may be devised to curtail the heavy economic waste of caring for the hereditarily defective. For these reasons, a sound eugenic measure, and one which will bring in large returns in professional and public interest, and which will serve to give far more data on human inheritance than we have at present, is the agitation to have taught in every medical school, not only a more extensive course of the fundamentals of genetics in premedical courses, but the application of this science to problems of human disease. This course should be taught by a medically trained person, familiar with the field of genetics, and it should be taught during the final year of the medical course, when the student has become familiar with the signs and symptoms of disease.

Madge Thurlow Macklin.
University of Western Ontario.

ONE of the effects of this growing interest in our subject is an increased demand for eugenic prognosis. More and more persons contemplating marriage, who have ancestors or collateral relatives exhibiting some hereditary disability, are applying for advice as to the chances of their children being affected by such and such a disease or disability. In the present condition of our knowledge of hereditary diseases it is by no means safe or easy to give advice in such cases. With a view to remedying this deficiency our Secretary, Dr. Blacker, is now engaged in editing a book, to which medical men with special knowledge of diseases recognized as hereditary are contributing, in order to place before the general practitioner the information at present available for giving eugenic prognosis. It is hoped that the publication of this book will stimulate the production of more accurate and abundant data than at present exist. And in this connection I may add that Dr. Blacker has prepared a schedule for recording the pathological elements in pedigrees, and also a second schedule designed for the preparation of full pedigree records for genealogical purposes in such a way as to bring out their eugenic aspects.

Although, then, I cannot claim that any real impression has yet been made upon the fertility of those individuals and classes whose reproduction must be considered racially undesirable, this brief account of the recent activities of the Eugenics Society, and especially its concentration on the questions of the Social Problem Group and Sterilization, will show that steady advance is being made. The importance of the subject of Eugenics is now much more generally recognized than at any previous period, both by the leaders of public opinion in this country, and by the public at large.

Sir Bernard Mallet.
President, Eugenic Educational Society of Great Britain.

THE vital thing, for the population at large, is a relatively low rate of multiplication of those who are, in general, physically and mentally less well endowed, without a decrease in the total size of the population; or, to put the same thing conversely, a relatively high multiplication rate of the genetically sounder germ plasm, *all along the line.* Ideally, the rate should be a function of total genetic worth, there being, from this point of view, no ultimate distinction between negative and positive eugenics. However, the better the genes, especially if they be rare, the more important is what happens to them. Since Galton's time, absolutely no headway has been made in realizing this major aim; in fact, it is widely claimed by eugenists themselves that just the opposite process is increasingly operative, despite their own preachments.

We might as well admit that the forces at work are quite beyond the control of us as eugenists, in the society in which we live. For they are fundamental economic forces. Galton lived too early to appreciate the principle brought out by Marx that the practices of mankind, in any age, are an expression of the economic system and material technique existing in that age. He thought that they could be moulded willy-nilly, from without, into conformity with the abstractions of an idealist intellectual. But the organization of society today is such as to make the primary motive of action, at least among the dominant section, the profit motive. This motive works out in devious ways that are contradictory to the welfare of the race as a whole, despite the fact that some of our modern philosophers, in a defense reaction try to rationalize the two ends into harmony.

H. J. MULLER.
University of Texas.

THIS International Congress is singularly opportune. It is not merely an academic problem we are met to discuss, or a problem of the future. It is not a theory but a condition which confronts us. It is a problem of the immediate present, and, like all sociological problems, the more fascinating because of its very complications. Man does not rise to his best endeavor in face of small problems; it is in the genius of modern humanity to meet and attempt to solve the most difficult. Eugenics is not a human invention by Francis Galton or any of his predecessors or successors. It is a long-known and universal natural law, namely, the survival of the fittest and the elimination of the unfittest. * * *

Prisons, reformatories, asylums, great public financial offerings, great national and local appropriations, great tides of human kindness and generosity, are merely palliatives and temporary expedients. They may for a time gloss over the cataclysm; they can not permanently cure it or avoid its recurrence. *The only permanent remedy is the improvement and uplift of the character of the human race through prolonged and intelligent and humane birth selection aided by humane birth control.* This is the burden of my address; it is the keynote of our third congress.

HENRY FAIRFIELD OSBORN.
Hon. Vice President, Third International Congress of Eugenics.

AS every phase of living conditions affects the quality of the human stock, so for the success of any eugenical program accurate data upon environment is absolutely indispensable, and for the fullest development of the innate qualities of the old pioneer families or of recent importations, the people themselves must be appraised, reminded of their finest traditions, made aware of progressive movements in other communities and encouraged to develop a more wholesome, optimistic attitude and greater pride in the traditions and the possibilities of their community.

H. F. PERKINS.
Director, Eugenics Survey of Vermont.

THE pressing need is for the other states of the Union to adopt laws as rigid as that of Virginia against racial intermarriage, and for the creation of public opinion against social and school intermingling of the races, which leads to marriage and ultimate destruction of race purity.

Separation of the races is the only true remedy, and to this end the Garvey "Back to Africa" movement should be encouraged.

By the extension and rigorous enforcement of the Virginia policy of racial integrity, it is possible to at least postpone the day when there will be but one race in America, a brown or yellow one, inferior to the present dominant race and not greatly superior to the one which was in the past, and still is, largely the servant race.

W. A. Plecker, M. D.
Richmond, Virginia.

ONCE in a while, of course, it is possible to advise people who are about to marry, as to whether they should or should not have children. But again such advice would usually be too late, and at best only a few couples could be affected in this way. While any gain is useful, it is futile to hope that the problem of engenics can be affected materially by such trivial procedures. It will be solved only by mass-measures, so to speak, dealing with the five great factors that make or break a nation: war, immigration, contraception, higher education, and charity.

Paul Popenoe.
General Director, Institute of Family Relations, Los Angeles.

IN studies on the inheritance of mental disease, one important factor has usually been neglected—the direct environmental effect on the children of the unstable parent or parents in the causation of mental disease in the child, quite apart from any inherited constitutional tendency. The problem is infinitely more complicated than most studies in genealogy indicate. It is true that the dynamic concept of mental development postulates that experiences in childhood lead to a neurosis or psychosis only when the constitution of the individual is phylogenetically predisposed to such reactions, but, it is also true that these experiences act as the pathogenic factor in the etiology of the disease. Normally, every child has to make an adjustment between his instinctual desires and the social aims and ideas of the milieu in which he is reared. The energy of the instincts which society says may not find direct expression must be eliminated or repressed, and this process starts practically from birth. The opportunity to live out the instinctive wishes varies widely in different cultures, but the chance to find outlets through which the repressed tendencies can find some acceptable expression varies, not only with the broad social milieu, but with the immediate surroundings.

Florence Powdermaker, M. D.

ONLY rarely is a family specific for one form of allergy. Crosses with negative lines show that an individual with one allergy may pass on various other types to his or her children. There is no evidence that allergy is due to one factor and each type due to a special modifying factor, for the possible modifying genes which must be dominant and which might be brought in by the negative line in these crosses do not produce the same type of allergy in any proportion of the offspring, but, on the contrary, the new types of allergy in the F_1 are of various kinds.

Migraine is more common among females than among males. Crosses between migraine individuals and negative lines show that a male may inherit migraine from either his father or his mother. Migraine is thus not sex linked but the reason for this difference between the sexes is probably to be sought in the physiological differences between them.

M. H. Richards and R. M. Balyeat.

ALL nations shall possess the right to practice a complete biological research, (somatic and germinal), of individuals declaring their intention to reside within the territory; or may prohibit the residence or limit the time of that residence, to aliens from those countries refusing to comply with the investigation or research herein stated. The same rule shall be applied to all individuals who after investigation, prove to be liable to hereditary transmission of any undesirable quality.

D. F. RAMOS.

Havana, Cuba.

A STILL further Indian contribution to civilization, closely associated with the esthetic and spiritual, is the social organization of the small community. It is not merely that Indian social organization may be interesting historically to students of western democracy; it is rather that in its survivals of community arts, village industry, and wholesome rural life, there may be a way out for American industrialism.

The real task, whether in terms of races or individuals, is to encourage and stimulate talent wherever found. Gifted individuals among Indian groups must be aided; the significant Indian contribution in the fine arts must be saved and advanced. In our program of education and adjustment today with Indian people we must somehow find the way to accept understandingly the living contributions they have made and can make to our civilization.

W. CARSON RYAN, JR.

U. S. Indian Service.

THE men at this co-educational college married exactly as much as the general male population of the United States, while at Harvard one quarter remain single. In every decade since 1873, one third of the co-educational women have remained single except that after the crisis of 1893 this third became, alas! two-fifths (two-fifths being the custom at all times in the large women's colleges, according to Whitney and Huntington).

Analysis of the birth deficiency: Among the men the causes were (a) late marriages, (b) great *excess* of one—or no—children in the early marriages—men of impaired health and of inferior personality, and also high-school teachers, being excessively represented; (c) no families of more than five, except in two cases, both college professors. Wealth was favorable to fertility in both men and women, as the 81 men on the "special contributors" list of the college had more children than necessary to replace themselves and 52 women also had.

Among the women, the deficiency was due to about the same causes, dreadfully augmented, however, by the prevalent celibacy. An exhaustive examination of their intellectual ranking by five separate methods showed that those ranking over 90 (or over 80 in mathematics alone) usually married much less than the rest, but those who did marry had so many more children than anyone else that they kept up the per capita for all honor graduates to a point exceeding the average. The five women who had six children or more were all wealthy, and all had fine minds.

CAROLINE H. ROBINSON.

Tunkhannock, Pa.

I HAVE outlined several measures by which the fertility of the gifted could be encouraged. If I do not consider them all equally important, and if I doubt whether some of them are really practicable, I am nevertheless convinced that every means at our disposal should be made use of to achieve our aim. I do not cherish the hope that the measures I have proposed will or can all be put in immediate operation, but they do include a few for which the time is now ripe. It is possible to make the essence and aim of eugenics known by means of propaganda on a large scale; literature, lectures, the stage, the pictures, broadcasting can all be pressed into the serv-

ice of eugenic reform. It is possible to urge upon the governments the necessity of giving tuition in eugenics and genetics of making the study of both compulsory in the training institutes which come into consideration for this object. It is possible to secure here and now the cooperation of the Church. It is possible to accelerate the education of gifted children. It is possible to convince the governments of the necessity of reforming the system of taxation in the interest of eugenic principles, without increasing thereby the State's financial burden. All these measures can now be put to the test. It goes without saying that they cannot be confined to only one country. The International Eugenics Society must be the controlling spirit and frame our policy. It will then be for the national societies to carry out this policy adapting it to the conditions obtaining in the several countries.

J. Sanders.
Physician, Rotterdam, Netherlands.

THE gradual extinction of the aristocracy is of social importance only on the supposition that the artistocracy represents the selection of the fittest, that it inherits the qualities which distinguished its ancestors, and that its disappearance will deprive the body politic of a valuable ferment. But apart from the fact that in many cases families were ennobled by the caprice of the Sovereign rather than for their intrinsic merit, the social loss implied in the extinction of the legitimate male line would only hold good if it coincided with the biological extinction of the family, which may well continue through female or illegitimate descendants to bring its special contribution to society.

Franco Savorgnan.

THIS apparent constancy in the results of the Seashore measures of musical capacities reveals their prognostic value. Herein lies their greatest scientific value. On the basis of information available from repeated tests for the same individuals over a period of time, the Seashore Measures of Musical talent are recommended as scientific tools to be used in basic studies of musical inheritance in famillies.

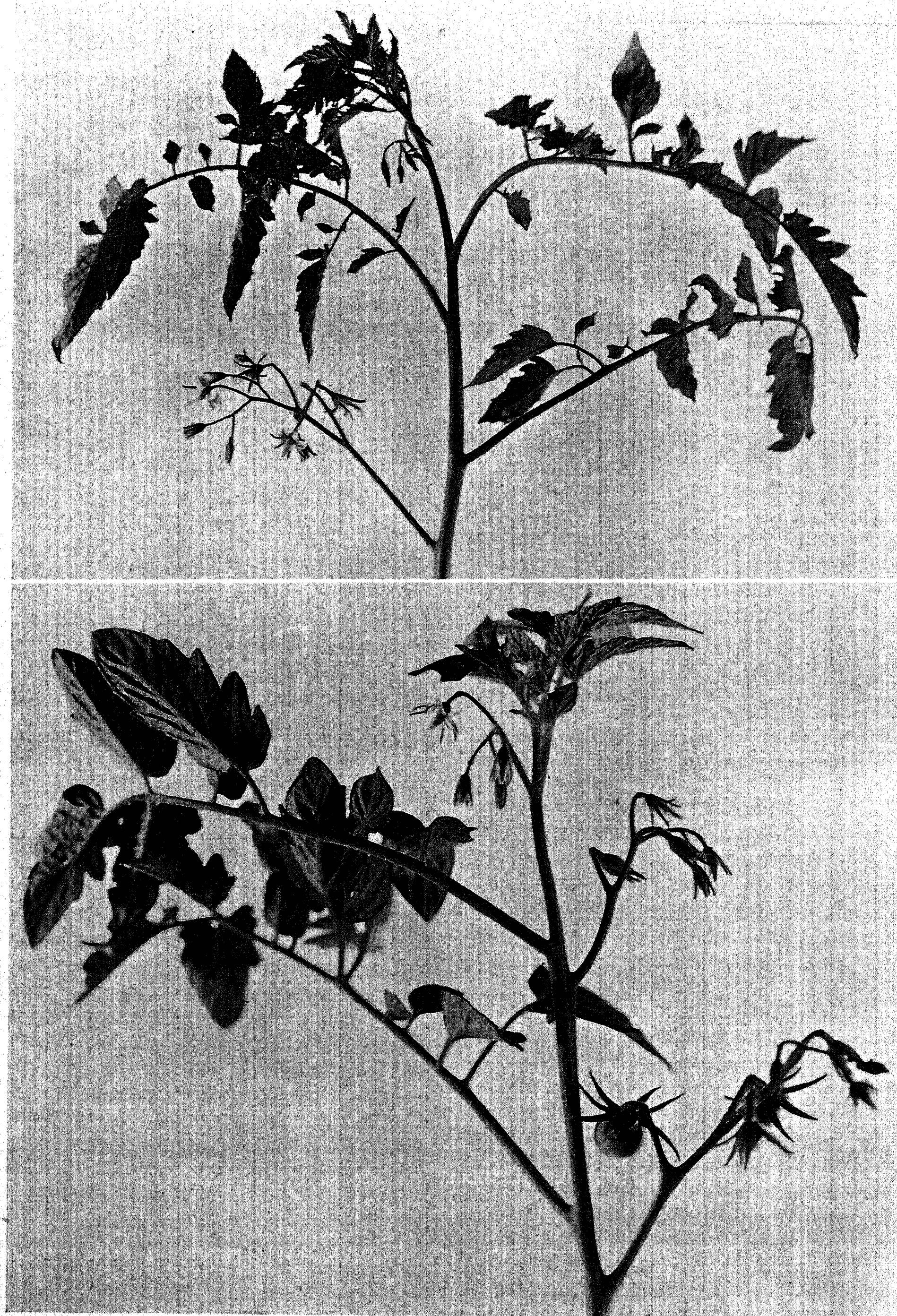
Hazel M. Stanton.
Eastman School of Music, Rochester, N. Y.

APPARENTLY, the important fact is that it is not the actually insane who constitute the real problem of mental disease. The carriers are more dangerous than the cases. The control of the development of insanity means that preventive work must be brought to bear on the carrier and this should begin in infancy.

E. Blanche Sterling.
U. S. Public Health Service.

THE American Negro population of the future will probably be more homogeneous as to ancestry, i. e., there will be a smaller percentage of unmixed Negroes, a large percentage with half or more Negro ancestry, and a smaller percentage who pass as Negroes but have more White than Negro ancestry. The segregation process operative in the inheritance of pigmentation will prevent the development of a population of one uniform hue.

Irene Barnes Taeuber.
Mt. Holyoke College.



CONTRASTING NORMAL AND "SELF-PRUNING" INFLORESCENCE TYPES

Figure 1

The normal inflorescence (above) recurs very regularly on every third internode along the stem. "Self-pruning" clusters (below), in marked contrast, are typically repeated on every or nearly every internode, until the growth of large fruits causes the cessation of growth of the main axis. "Self-pruning" is a recessive character.

INHERITED CHARACTERS IN THE TOMATO

1. The Self-Pruning Habit

JOHN W. MACARTHUR
University of Toronto and Ontario Agricultural College

VARIETIES of the common tomato that are called "self-pruning" are of comparatively recent origin. About eight years ago Burpee's Self-pruning was introduced and disseminated as a variety, whose "plants do not require any pruning, due to their limited branching habit. The fruits are borne profusely all along the vine." Yeager[3] described and illustrated its "remarkable characteristic of determinate growth, producing blossom clusters at the tips of the branches" and emphasized its possible economic value for securing a variety that would ripen most of its fruits before frost.

A more essential and invariable feature in the self-pruning habit is something which is not specifically mentioned in either of the above descriptions, but which is well illustrated in Yeager's figures and in Figure 1, namely a unique spacing of the inflorescences along the stem.

All tomatoes—currant, cherry, pear, peach, and the common large-fruited varieties, including Globe, of which self-pruning is quite evidently a mutant derivative—normally produce the first blossom cluster on the ninth to the thirteenth internode and then repeat the inflorescences regularly every third internode thereafter all through the growth of the stem (Figure 1 above). The main stem, apparently simple and straight in growth as by the continued development of a terminal bud, is really sympodial, composed of a somewhat zigzag series of superposed branches, which continue the same general direction of growth. Each of these branches, or segments of the main stem, terminates after three nodes (and leaves) in an apical inflorescence, opposite the base of which a new auxiliary bud appears; from this arises the next stem of three nodes. This is repeated indefinitely until a long succession of fruit and flower clusters are found all along the seemingly monopodial axis. Since each peduncle is adnate to the stem for a short distance, it appears to arise from the internode.

Two prominent variations from this type occur: (1) the compound inflorescence where each segment of the stem is six internodes long and the flower cluster is very large and much-branched; and (2) the self-pruning, where some of the stem segments are shortened to one or two internodes. In the latter the flower clusters begin at the usual height and then tend to recur on every internode or every second internode; they sometimes skip three leaves, but are then invariably closely spaced again. If fruits set on these clusters nutrition seems to be diverted to them and further terminal growth of the stem segments ceases. Leaves are seen to be interspersed among the close set clusters. Distinct laternal branches arise from the nodes immediately below, and then more basally, on which inflorescences soon appear in similar close succession. The result is that the self-pruned plant covers a much smaller circle of space than do normal plants, but covers this more densely.

The Genetic Behavior of Self-pruning

In crosses of self-pruning with normal plants the F_1 hybrids all produce their blossom clusters in the normal succession with three intervening leaves, the stems are indeterminate in growth, and the plants cover much ground. As

Yeager[3] suggested, self-pruning is a recessive character. In the F_2 a simple and clean segregation occurs on a monofactorial basis. The ratio of 716 normal:202 self-pruning plants shows some deficiency in numbers of the latter class, but the deviation from 3:1 is only twice the probable error and not significant. The back-cross ratio obtained, 83 normal:93 self-pruning is a closer fit to expectancy. As symbols for this pair of genes we have used[2] *Sp* and *sp*, but Yeager's[4] *T* (for indeterminate) and *t* (terminate) are equally descriptive and more brief.

Linkage Relations of the Self-pruning Gene

Self-pruning has been crossed with linkage testers containing fifteen genes already described,[2] that now appear definitely located in seven "chromosome groups." The various F_2 dehybrid repulsion ratios and the cross-over values (Calculated from them by Immer's tables[1]) which are given below, show that independent assortment probably takes place between *Tt* (*Sp sp*) and the following pairs: *Dd* 52.7 $\pm$ 2.3; *Pp* 46.4 $\pm$ 6.5; *Oo* 51.9 $\pm$ 6.0; *Rr* 60.0 $\pm$ 4.2; *Yy* 49.6 $\pm$ 2.0; *Br br* 57.8 $\pm$ 3.3; *Ff* 49.1 $\pm$ 2.3; *Aa* 53.4 $\pm$ 3.3; *Ll* 54.7 $\pm$ 3.2; *Hh* 55.1 $\pm$ 3.0. The special features of the phenotype relationship between self-pruning and the two other atypical forms of inflorescence, compound (*s*) and leafy (*lf*), will not be described here.

The above genes represent groups I to VII, excepting only group IV with whose marker, potato leaf (*c*), self-pruning exhibits a very strong linkage. The repulsion ratio, 117 *CSp* : 50 *Csp* : 52 *cSp* : 0 *csp*, indicates the closeness of this association. One of the *Csp* plants was recognized as heterozygous *Cc*, and produced 25 per cent double recessives. In the back cross generation from F_1 (*Csp* $\times$ *cSp*) $\times$ *csp*, the ratio was 3 *CSp* : 90 *Csp* : 81 *cSp* : 2 *csp*. Here the intensity of the linkage can be efficiently measured, the cross-over percentage being 2.83 $\pm$ 1.9.

The IV linkage group of the tomato, accordingly, contains the loci of *Cc*, controlling cut-leaf and potato-leaf, and *Spsp* (or *Tt*), controlling the normal and self-pruning habits.

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Breeding Karakul Sheep

Das Karakulschaf und seine Zucht (The Karakul Sheep and Its Production), *by* G. Froelich. 148 pages, 43 figures. Price in paper covers, 8.50 marks, cloth 10 marks. F. C. Mayer, Munich. 2nd edition. 1931.

THE Institute of Animal Breeding at the University of Halle for many years has been a center for breeding Karakul Sheep. G. Frőlich, the director of the institute, here presents valuable information concerning the breeding and management of animals of this breed. The author first gives a short history of the breed and a report concerning the present distribution and the ability of acclimatisation of Karakuls. This is followed by a detailed account of the different breed characters, with special reference to hair and skin. Different methods of breeding Karakul sheep are described. The inheritance of the most important Karakul characters is discussed. The concluding chapters deal with management, fur production, and hygiene. There is an extensive bibliography. On the whole, the book is a well-balanced and non-technical account of the most important problems of breeding and raising Karakul sheep. There are many instructive illustrations.

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INHERITANCE OF WHORLS IN THE HAIR OF SWINE

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THE investigation of whorls in swine was prompted a number of years ago because of their prevalence in commercial and purebred herds. In an observation of 12,000 market hogs Craft[3a] found 1.4% affected with whorls. Breeders and judges often hold different views on the significance of whorls in breeding swine. Definite information on the problem was not available and extreme views were not always reconciled, especially in the showring.

Whorls have been known for a long time to occur on the neck, rump, and loin coupling along the spinal column in swine. They occur in the inguinal and post-humeral regions also and are common to the middle and lower cervical areas. Frontal and nasal whorls are common. The whorls that occur along the spinal column are the most conspicuous and it is these whorls that are especially discriminated against in purebred swine. The secretaries of ten purebred swine Breed Associations, who handle over 95 per cent of all pure bred swine registrations in the United States are in accord in considering whorls as a "serious objection especially in boars" and advise that "accepted judges of the various breeds will not place a hog in the money when it has a whorl." Two of the breed associations disqualify for registry animals which have a whorl. (Information supplied by correspondence.)

A study of the relationship of pigs that had whorls was made at this station in 1922, and in the following year a definite breeding program was started. The preliminary study indicated that this defect might be a Mendelian character. If the heritable nature of the whorls could be established it would be of considerable economic importance to breeders.

The hair along the spinal column, and, for the most part, along the sides in swine is normally rather simply arranged. In general, the hair slopes downward and backward ventro-caudally. Variations in the direction of the hair-stream along the spinal column are not uncommon. The hair in such areas opposes in varying degree the normal hair stream. The center of the disturbance is usually on the median line (Figure 4). The affected areas in nearly one hundred cases observed, including those shown in the chart (Figure 5) have been limited to the neck coupling at the top of the shoulder or immediately anterior to it (dorso-cervical) (Figure 4), to the lion (lumbosacral), and rump (sacro-caudal) region (Figure 3). Defects in the hair stream in the rump region may often be found at the base of the tail.

Hair whorls in the area of the neck in the specimens discussed in this paper were, as a rule, comparatively small and often less complete in their symmetry than those caudally located (Figure 3). Disturbances in the normal hair stream on the loin and rump are often very large and occupy almost the entire space from the base of the tail to the last dorsal vertebra. The experimental subjects, in Figures 2 and 3 show typical examples of such large disturbances. It is common practice to recognize all the variations in the normal hair stream along the spinal column in swine as whorls or swirls even though there are essential differences in their pattern detail, some of which can scarcely be defined as whorls. For convenience in this paper, the term whorl will be used in general to designate all types of disturbance.

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Kidd[5] has recognized three closely associated elements, which he groups together as one phenomenon; namely, whorl, feathering, and crest. When whorls are found alone they may have a clockwise or counter-clockwise direction. Eschricht,[4] Voight,[12] Brewster,[1] and Lauterbach and Knight[7] have recognized the right and left whorls in the head of the human. In general, the disturbances included in this study have a central point from which they seem to start (Figure 2-*C*). From this central point, the hair spirals right or left, either coalescing rather smoothly with the normal hair stream or opposing the normal hair stream vigorously and being brought to a sudden stop in a crest (Figure 2-*A*). The feathering (Figure 2-*B*) seems to have its origin in a whorl for the most part. Some exceptions to this seemed to occur, in which the feathering did not seem to have a definite connection with a whorl. The feathering disturbances that occupied a part of the rump area and extended to the loin coupling seemed to become more vigorous in opposing the normal hair stream up in the loin coupling area and terminating in a very definite crest. Craft,[2] in describing a whorl, says: "The hair forms a whorl similar to a back-wash in a stream of water."

Small disturbances which are likely to be overlooked may be found at the base of the tail. These are largely of the feathering pattern. Asymmetrical feathering was also in evidence and occurred for the most part in the area of the neck (Figure 4-*B*). The crest was not always in evidence as there is considerable variation in the manner in which the anterior margin of the disturbance opposes the normal hair stream. Apparently, the center of these disturbances was on the median line of the spinal column.

Permanency of Whorls

Craft,[2] in his study of whorls in Poland Chinas has observed the tendency for whorls to disappear in some specimens when they were about twelve weeks old. This tendency has not been observed in the specimens involved in these studies. In some cases, however, where the original normal hair stream disturbance was small, there was a tendency for the defect to be progressively less conspicuous toward maturity. When only a few hairs are involved in a hair disturbance, their direction may be forced to coalesce more smoothly with the normal stream as it grows longer and stiffer with age.

Whorls Are Inherited

Craft,[2-3] has found that the "swirl is hereditary, and is transmitted through both the sire and dam." The author[8] has previously reported the inheritance of whorls in swine. It is the purpose of this paper to analyze the available data from 27 litters of pigs for the inheritance of whorls. From a total of 253 pigs, 194 were free from whorls and 59 had whorls. In twenty-two litters produced by sows which were carriers of the hereditary influence or which were affected with whorls, 141 pigs were produced without whorls and 59 pigs or almost 30 per cent of the total had whorls. When both parents were affected with whorls 46 per cent of the progeny had whorls. Under similar conditions Craft,[3a] in working with Poland Chinas, found 59 per cent of the progeny with whorls.

Location of Whorls

A record is available in Table II of the location of the whorls but no attempt is made in this paper to explain the probability that there may be a separate factor or factors associated with the factors that produce whorls, which determine the location of the whorls. Landauer,[6] in reviewing the study of whorls in guinea pigs made by Pictet, Ferrero, and Guyenot calls attention to five zone centers where whorls are located, namely, near the base of the tail, loin center, to the front and rear of the loin, and on the neck. In January, 1932, Landauer supplied the author with more recent observations of later contributions from

WHORLS IN CAUDAL REGION

Figures 2 and 3

Typical cases of hair disturbances in the caudal region in swine. The elements of a whorl are indicated in the left-hand picture, where *A* indicates the crest, *B* the feathering, and *C* the center of the whorl.

Pictet and Ferrero.[10] He points out that whorls in the guinea pig are dominant over smooth hair and if the gene *R* for whorls alone is present, the animals have "localized" whorls only in the loin region. There is also a second dominant gene, *G*, which, if present in the heterozygous or homozygous condition, causes whorls to be "generalized" and to appear in the coccygial and neck regions in addition to the lion area. The gene *G* can be expressed only in the presence of the gene *R* for whorls. Also, a third independent gene *D* is postulated by Pictet and Ferrero which determines whorls on the head. This gene also needs the presence of *R* for expression.

In swine it often happens (Table II) that whorls will appear on the neck and in no other place along the spinal column. The pig shown in Figure 4-*B* had the hair defect only on the neck. Whorls on the head are very common in swine. They are located, as a rule, midway between the snout and eyes. They do not appear to be inherited as are head whorls in guinea pigs. In swine they occur apparently independently of whorls along the back.

It will be noticed from Table I that sows 22, 32, 33, 34, and 35 had whorls at the neck coupling and 32 and 35 had in addition, whorls on the rump. Two litters of sow 22, showing a whorl at the neck coupling, when mated to boar 17 with a whorl on the loin coupling, produced 17 pigs of which 5 had rump whorls and 6 neck whorls. Sows 33 and 34 (with whorls on neck) and sows 32 and 35 (with whorls on neck and rump), produced, when mated to boar 37 (which carried both neck and rump whorls), 7 pigs with rump whorls and 15 with neck whorls. The swirl over the shoulder has not been observed so far in the experimental stock used by Craft.[3a]

There were 62 whorls involved in 59 pigs affected of which 32 were in the rump-loin region and 30 in the neck-coupling area (Table II). Nine of the pigs with neck whorls were produced by matings which did not show the neck whorl and 21 were produced by sows which had a neck whorl. Two sows, 33 and 34 (each with one neck whorl), produced, when mated to boar 37 (with one whorl on the neck and one whorl on the rump), 1 pig with rump whorl and 11 with neck whorls. It would seem, therefore, that there is a separate genetic influence which determines the location of whorls.

Foundation Boars

Boars 100 and 101 in the Inheritance Chart, Figure 5, were used for a while in the Experiment Station Herd. Both boars sired too high a percentage of pigs with whorls. These boars did not seem to carry the same inheritance for

TABLE I.—Hair Disturbance in Affected Males and Females

Male	17	Large whorl on rump and feathering forward to loin coupling.
	37	Large whorl on rump and feathering over loin coupling. Small whorl at neck coupling on backline.
Female	22	Small whorl at neck coupling.
	23	Large whorl on rump.
	32	Small whorl and feathering on rump, and neck coupling.
	33	Small whorl and feathering at neck coupling.
	34	Large whorl and feathering at neck coupling.
	35	Large whorl and feathering on rump, and neck coupling.

TABLE II.—Distribution of Hair Disturbances on Basis of Sex and Location Involving 27 Litters—194 Normal and 59 Affected Pigs. (All disturbances Noted Here Are on the Middle Line of the Back). *W* = **Whorl;** *F* = **Feathering; (—) = Whorl and Feathering in Both Locations.**

					Location of hair disturbances			
					Rump and loin coupling		Neck Coupling (dorso-cervical)	
Dam	Sire	Size of litter	Normal hair	Affected	Male	Female	Male	Female
1—	100*	10	10					
3—	101*	11	11					
6—	101	9	9					
7—	100	10	10					
8—	101	13	13					
2*	100	11	10	1	1W			
4*	101	7	5	2	1W	1WF		
5*	101	9	8	1		1W		
7*	101	8	7	1		1WF		
9*	101	9	4	5	2WF	1W		2WF
10*	101	10	8	2		1W		1WF
25*	24*	8	5	3	2W	1W		
25	43*	11	10	1			1W	
26*	43	10	8	2	1WF	1W		
27*	43	9	8	1			1WF	
62*	43	8	7	1				1WF
63*	43	10	9	1		1WF		
71*	43	9	8	1		1WF		
71	43	9	7	2			1W	1WF
16*	17**	10	7	3	1WF	1WF	1WF	
22**	17	11	9	2	1WF	1WF		
22	17	6	0	6	1WF(—)	2WF(—)	1W 1WF(—)	2WF 2WF(—)
23**	37**	10	8	2	1WF	1WF		
32**	37	7	4	3	2WF			1F
33**	37	9	2	7			5WF	2W
34**	37	10	5	5	1F		2WF	2F
35**	37	9	2	7	1W 1WF	1W 1F	3WF	
					5W	6W	3W	2W
		253	194	59	10WF	9WF	13WF	9WF
					1F	1F		3F

— unknown
* carrier
** affected

A B

TYPICAL HAIR WHORLS
Figure 4

Whorls in the neck region in two pigs. The location of whorls seems to have a genetic basis, as the location of the whorls has been observed to be inherited.

whorls inasmuch as sows mated to boar 100 would often produce pigs with a whorl when the same sows mated to boar 101 failed to produce pigs with a whorl, and vice versa. Boar 100 mated to sow 7 produced no pigs with a whorl. However, when sow 7 was mated with boar 101, one pig with a whorl was produced. A probable reason for this will be discussed later in this paper. Matings were planned with a view of combining the inheritance of these two boars in such a way that their genetic constitution as regards to whorls might be determined.

Inheritance Not Simple

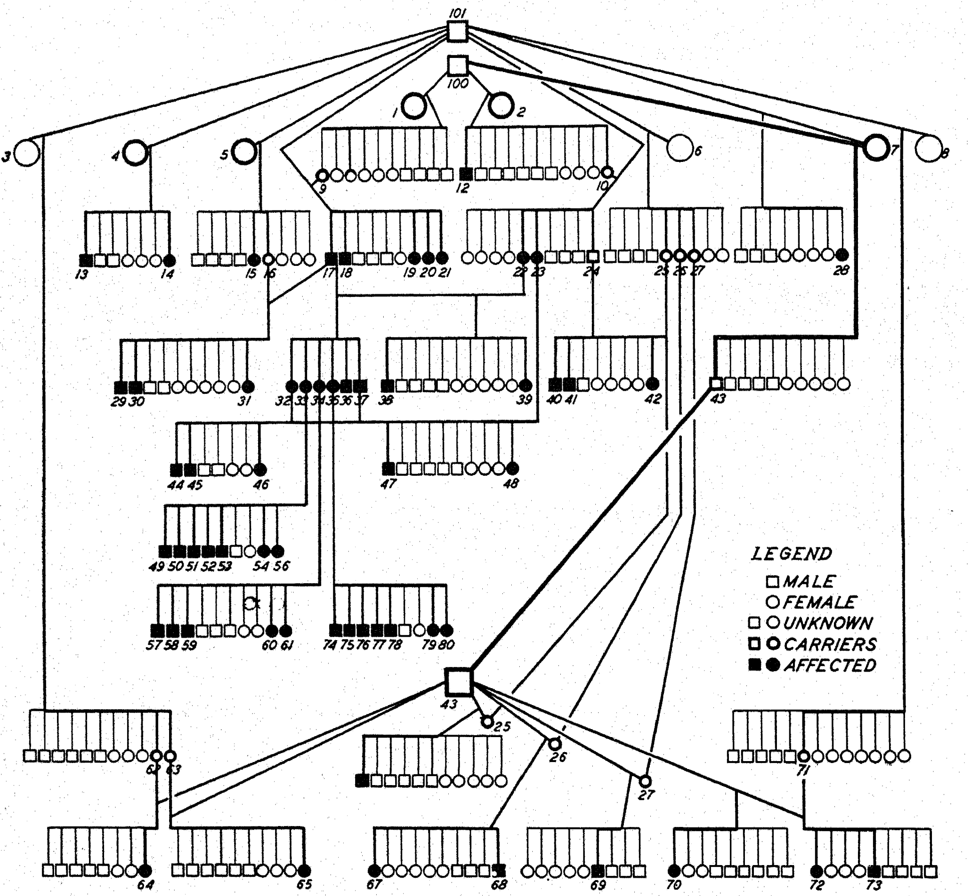
The author[9] has previously reported that the inheritance of whorls is not simple. As a rule, smooth haired parents did not produce more than two or three pigs in a litter that had whorls, and for the most part only one pig with a whorl was produced in each litter. One exception to this occurred when boar 101 was mated to sow 9. The litter produced had five affected and four smooth haired pigs. At first thought the inheritance might seem to be a simple recessive. However, were that the case, then boars with whorls such as 17 and 37 should have sired litters in which all pigs carried whorls when mated to sows that had whorls. Instead, such matings produced both whorled and smooth-haired pigs. It seems rather obvious, therefore, that the inheritance is not of a simple type.

Probably Two Dominant Factors

A two-factor hypothesis* has been used to explain the inheritance of whorls: W and W' are assumed as factors for whorls, both of which must be present in the homozygous or heterozygous condition before whorls manifest themselves. A pig homozygous for whorls would be of the composition $WWW'W'$. Since the factor for whorls is in all probability dominant, then pigs with the following composition would have whorls: $WWW'W'$, $WwW'W'$, $WWW'w'$ and $WwW'w'$. On this basis pigs free from whorls would carry only one or the other of these factors in the homozygous or heterozygous condition or be homozygous for their allelomorph as follows: $WWw'w'$, $wwW'W'$, $Www'w'$, $wwW'w'$ and $wwww'w'$.

This hypothesis does not explain fully the occurrence of only one pig with a whorl in so many litters produced from matings of smooth haired parents. On the basis of this hypothesis, pigs with whorls will appear only with the following matings of smooth haired parents: $WWw'w' \times wwW'w'$, $Www'w' \times wwW'W'$, $WWw'w' \times wwW'w'$ and $WWw'w' \times wwW'W'$.

*The hypothesis of two interacting dominant factors was suggested to the author by W. A. Craft, January 18, 1929, as a possible explanation for the inheritance of whorls in swine. A further discussion of this hypothesis has been made more recently by Craft.[3a]



RELATIONSHIP OF PIGS WITH HAIR WHORLS

Figure 5

Pedigree chart showing the relationship of descendants of boars 100 and 101, assumed to carry complementary dominant factors for whorls.

These matings would produce respectively the following percentages of whorled pigs: 25, 50, 50, and 100. In no case did all of the pigs in a litter, produced by smooth haired parents, manifest whorls, hence apparently no mating involved a sire and dam, one homozygous for one member of the pair of factors and the other homozygous for the other. As a result of 14 litters produced by smooth haired parents, 104 pigs were produced. Of these 24, or approximately 23 per cent, had whorls, and 80 (77 per cent), were smooth haired.

This result is approximately what would be expected if the sire and dam of each mating was each heterozygous for one factor of the pair. With the exception of sows 9 and 10, there had likely been very little opportunity for any one of the factors to become homozygous in the other smooth-haired sows involved, hence and average of over 23 per cent apparently agrees reasonably well with expectation.

Probable Associated Factors

If, as assumed, there are two factors for whorls and the homozygous condition for whorls is *WWW′W′*, then, in view of the fact that whorls

are found in different zones along the spinal column and confine themselves rather uniformly to these zones, and may appear in two or three zones on the same individual, it would seem that some explanation must be made to satisfy the factor of location. As previously pointed out in this paper, parents with whorls on the neck seem to produce offspring with whorls in the same zone and when the whorls are found in the rump-loin region in the parents, the largest percentage of the whorls which appear in the offspring will also be in this zone. Further study is required for a satisfactory analysis of the elements which determine the location of whorls.

Causes for Variation in Whorls

Since the hair disturbances vary in size, shape, and direction, it is probable that genetic factors also influence these variations. Landauer[8] points out that the generalized whorl form in the guinea pig is influenced genetically by one dominant factor and that the localized form is its recessive allelomorph. A complete genetic solution of this phase of the problem in swine is not possible with the data at hand.

Method of Eliminating the Defect

On the basis of the inheritance which seems to prevail it is imperative to discard both the sire and dam of the pigs that have whorls if the defect is to be

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Probable Genetic Composition of Boars 100 and 101

It appears that sow 9 was homozygous for one of the factors. When she was mated to boar 101 she produced 9 pigs, five of which had whorls. Both sow 9 and boar 101 could not have been homozygous for different factors or 100 per cent of the pigs would have manifested whorls. It is very probable that boar 101 was heterozygous for one factor as he sired only 6 pigs with whorls out of a total of 34 when mated to sows 4, 5, 7, and 10, all smooth-haired. If he had been homozygous for one factor, fifty per cent of the pigs produced by these sows should have had whorls, as, no doubt, the sows were all heterozygous for the other factor. If we assume that boar 101 carries one factor, *W'*, in the heterozygous form (*wwW'w'*), then sows 4, 5, 7, and 10 would be of the constitution *Www'w'*. Sow 9 is probably *WWww*. Sow 1 could be *Www'w'* or *WWw'w'*. Boar 100 could only be *Www'w'* or else 50 per cent of the litter, when he was mated to sow 2 would have had whorls. Sow 2 is probably *wwW'w'* and sow 10 *Www'w'*, the *W* coming from boar 100. When she is mated to boar 101 the two factors *W* and *W'* are combined to bring out whorls. Boar 24 in this litter is, no doubt, *Www'w'*, and sow 25 *wwW'w'*, the *W'* coming from boar 101, since sow 6, as she farrowed no pigs with whorls, is probably *www'w'*.

Sow 7 has produced one litter to the service of boar 100 and one litter to the service of boar 101. When mated to boar 101 she produced a pig with a whorl. Hence she is probably *Www'w'*, and of the same constitution as boar 100. Otherwise she would have been expected to produce some whorls when mated to him. Boar 43, then, gets his *W* either from sow 7 or boar 100, and since he sires so few pigs with whorls, he is undoubtedly heterozygous for this factor.

Boar 101 is the sire of sows 25, 26, 27, 62, 63, and 71 and transmitted *W'* to them since their dams apparently were not carriers. Had they been carriers of the same factor then a larger percentage of the offspring of their daughters mentioned above should have manifested whorls. Had they carried *W* then they should have produced some pigs with whorls. It would appear from this analysis that boar 100 is heterozygous for one factor and boar 101 heterozygous for the other factor of the pair assumed to obtain in this hypothesis.

No Animals Homozygous for Whorls

It is not very probable that any of the experimental subjects used in further matings in this experiment, were homozygous for whorls (*W W W' W'*). In two litters, totaling 17 pigs, sow 22 mated to boar 17, both with whorls, produced 8 with whorls and 9 without. This is near the expected ratio if both parents were heterozygous for both factors. Sow 23, when mated to boar 37 produced only 2 pigs with whorls in a litter of 10, and when boar 37 was mated to his litter mates 32, 33, 34, and 35 they produced in one litter each a total of 35 pigs, 22 of which had whorls and 13 of which were smooth-haired. If any of these sows had been homozygous for whorls, obviously all of their progeny would have been manifesting whorls.

eliminated. When a smooth-haired boar mated to smooth-haired sows (that have not produced pigs with whorls to other boars), sires pigs with whorls, the boar is often given the entire credit for producing whorls. As a matter of fact, he is probably no more responsible than the sows that produce pigs with whorls. From the data presented it seems evident that it is impossible for a pig to have a whorl if only one parent carries the heritage for a whorl.

If the two-factor hypothesis assumed as an explanation for the heritage of whorls is correct, one can readily see why the inheritance that produces whorls is so generally distributed. A herd boar which carries one factor will transmit it at least to half his offspring. Subsequent herd boars may carry the same factor and repeat the performance of their predecessor, adding to the concentration of this factor in the herd. Whorls will not appear until the complementary factor is introduced, generally by a new boar. It is easy to see why he may get the entire credit for producing whorls.

The elimination may be even more difficult, if, as previously assumed, there are separate factors that determine the point of location of whorls on the backline. If then, the factors for whorls and for location are inherited separately, it seems to follow that even the two factors for whorls may be present without whorls being manifested. This is purely hypothetical. However, the data suggest this possibility.

The most practical procedure for eliminating whorls in swine is to discard from the breeding herd all sows, and most assuredly all boars, that have whorls. It is almost as important to remove with equal diligence all smooth-haired sows and boars that produce progeny with whorls. Obviously the factors have been handed down through at least one of each of the two paternal and maternal grand-parents but it will require carefully planned matings to determine which of these four carry the undesirable inheritance.

Summary

1. It appears that two dominant Mendelizing factors interact together to cause the appearance of whorls and these factors must be present in the heterozygous or homozygous condition before whorls are produced.

2. Causes of variation in location of whorls, and the procedure most rapidly to eliminate the defects are discussed.

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THREE VIEWS OF EVOLUTION

Recent Discussion of the Causes of Evolution, of the Present Status of Evolution Theory, and of Evolution of Human Behavior

VERERBUNGSLEHRE MIT BESONDERER BERUCKSICHTUNG DER ABSTAMMUNGSLEHRE UND DES MENSCHEN, *by* L. PLATE. Band I. Mendelismus. X+554 pp., 133 figures. Price, paper covered, 26 marks; bound, 28 marks. Jena, G. Fischer, 1932.

THIS is a second (revised) edition of a well known text book of heredity written by the successor of Ernst Haeckel at the University of Jena, who is also director of the unique Phyletic Museum of Jena. The first edition published in 1913 discussed with a thoroughness and exhaustiveness perhaps unequalled in writings of that period the bearing of the newer knowledge of heredity upon the evolution theory. The present edition brings this discussion up to date. A task so stupendous, in view of the enormous literature of genetics in the past twenty years, few would have had the courage to undertake. Plate not only undertook but has also accomplished the task with astonishing success,—assembling, classifying and assimilating all pertinent literature. Students of genetics are greatly indebted to him for this achievement. Most of us are so deeply engrossed in exploiting a particular aspect of the subject that we have little time or inclination to attempt a detached airplane view of the whole field. Geneticists, as a rule, have very little to say about evolution, yet it is chiefly in relation to this general subject that their studies give promise of lasting value. Plate emphasizes evolution as the central problem even in his discussion of Mendelism in the present first volume of a proposed three volume work, the subsequent volumes to deal with: II Sexuality and its problems and; III Special genetics of the most thoroughly investigated animals and of man.

His own basic ideas about evolution which of necessity influence his judgment of the value and significance of particular investigations, the author frankly communicates to the reader in his preface. They are substantially and as nearly as possible in his own words as follows:

1. Mutation has come to include a variety of concepts of different value in relation to evolution, as they differ in hereditary value. We may recognize the following gradation with increasing value in heredity: (a) somatic modifications not inherited; (b) weak mutations including persisting modifications; (c) labile mutations of intermediate races; (d) genuine mutations and changes in the heredity-basis (Erbstock).

2. Genes demonstrable by the method of crossing pertain only to varietal characteristics. In addition to them, it is necessary to assume the existence of a basis of heredity likewise contained in the nucleus (but not in the chromosomes, therefore not statistically demonstrable) which conditions the plan of organization found in genera, families and higher categories of classification.

3. Cell plasma as a basis of heredity is not to be accepted, if we understand thereby discrete parts of the cytoplasm with specific effects. But the physical-chemical constitution of the cytoplasm is of course of the greatest importance since the cytoplasm is the instrument through which the cell operates.

4. Goldschmidt's views advanced in his physiological theory of heredity are unacceptable, for genes are not enzymes but living bearers of energy which force the epigenetically changing cytoplasm to assume particular functions. Neither are genes to be confused with hormones. The different genes of a species and also the different alternative forms (allelels) of a gene are not

merely quantitatively but qualitatively different.

5. Functional adaptation is scarcely capable of explanation without the assumption of an inheritance of acquired characters. Such assumption is justified because as yet no convincing experimental evidence against it has been produced. No one as yet has for hundreds of generations enforced a particular kind of use or disuse and demonstrated a consequent inheritance or non-inheritance. Experimental proof and disproof are alike wanting, but there are hundreds of facts which without it are unintelligible. Lamarckism is therefore to a certain extent justified and is not at variance with the mutation theory since accidental mutations could through use be improved.

6. Progress in phylogeny is accomplished primarily by neo-mutations, that is through the development of entirely new genes either produced by the cytoplasm or originating spontaneously.

7. The theory of descent must in the future as in the past be based primarily on the results of comparative morphology (palaeontology, anatomy, embryology) and on the geographic distribution and life histories of organisms. Genetics is almost never in a position to disclose the genealogical connection between species, genera, and higher categories. But it does disclose the basis for species formation by explaining the mode of origin of varieties and races and the mutual infertility of divergent types.

8. Darwin's views still stand substantially correct; that is, selection plays an enormous rôle, as every geneticist daily observes. Evolution progresses over millions of corpses. In the origin of species small heritable differences are gradually summated so that varieties become races and races species. Darwin recognized quite correctly the cooperation of selectionist and Lamarckian factors although he also gave little consideration to the latter. Whoever speaks of a "crisis of Darwinism" shows only his own incapacity to survey the entire observational field and arrange all in a unified theoretical view.

There is no single topic in the entire field of Mendelism on which Plate has not assembled a superabundance of illustrative material, usually with a critical discussion of the evidence for or against particular hypotheses. The exhaustive topically classified bibliography will be especially useful to the student. An excellent index will allow instant reference to the investigationss of any particular author or on any particular subject or with any particular genetic material.

W. E. Castle.

Bussey Institution.

Evolution and Adaptation

IN a book containing the wealth and diversity of information that is crowded into Caullery's *Le Problème de l'Evolution,** it is difficult to find any few salient points that can justly be isolated for emphasis in a brief review. In this journal attention should perhaps be directed toward matters intimately related to heredity, although in attempting this the reviewer must hasten to state that genetic material comprises but a small fraction of the volume. There is an academic and uninteresting discussion of the origin of life, followed by an entertaining, and stimulating series of chapters dealing with palaeontology, appearance of animal groups, evolution of mammals, parallel evolution and convergence, morphology, rudimentary organs, parasitism, and geographical distribution. These rather hackneyed subjects are treated with a freshness and critical insight that makes the book pleasant reading In fact, it is such a book as graduate students and biologists in general, par-

*Caullary, M. Le Problème de l'Evolution. Pp. 447. Payot et Cie. Paris, 1931.

ticularly geneticists, well might read from time to time, merely for the sake of orientation.

Nine chapters comprising 236 pages are designed to set forth the "fact of evolution," which is done with no lack of interpretative discussion and critical comment. The remaining six chapters, spread over 193 pages, are concerned with the "mechanism of evolution," a subject in which the author concludes (one may infer) that we are still sadly ignorant. In the latter part of the book the treatment becomes somewhat historical, and this is probably as satisfactory a method as any for bringing out the views which have contributed to our present outlook.

Like several other leading French biologists, Caullery is much concerned with the question of adaptation, a problem which is of no small moment to the theoretical geneticist. Admitting freely that much which passes as adaptation might be explained in other ways, the author, nevertheless, accepts the phenomenon as a reality and recognizes four distinct viewpoints that have been entertained by those who would explain it.

Four Theories of Adaptation

1. Special Creation. One of the outstanding exponents of what is called "special creation" was Cuvier, who maintained that not only must the parts of each individual be in harmony, but all nature must be balanced, some species serving as prey and others as destroyers and regulators of propagation. One could not well imagine, says Cuvier, a world in which there were flies and no swallows or swallows and no flies. All this harmonious adaptation was in effect foreseen and prearranged. Caullery calls it "adaptation *a priori*."

2. A diametrically opposite view was introduced by Lamarck, who in 1809 (*Philosophie Zoologique*) made the following significant statement (translating freely): "Nature in the process of producing successively all the species of animals, beginning with the most imperfect or simplest and ending with the most perfect, has gradually made their organization more complex; and as these animals became disseminated through all the inhabitable parts of the globe, each species received through the influence of the circumstances in which it found itself, the characteristics and modifications with which we are familiar." This Caullery would call "adaptation *a posteriori*."

3. A third idea is that transformations, arising from whatever cause, endow certain individuals with special potentialities or qualities which result in their "filtering" into that environment for which chance has made them most suited. This is the "fortuitous preadaptation" of Cuénot (See "The Problem of Adaptation," JOURNAL OF HEREDITY, 18:125-131, March, 1927). A similar idea was foreshadowed by Buffon, who, after describing in great detail the wonderful adaptation of the woodpecker for its particular mode of life, went on to say that this bird "has received from nature organs and instruments appropriate to this (its) destiny or, better, it derives this very destiny from the organs with which it is born."

4. Finally there are others, including Rabaud (*Adaptation et Evolution,* 1922) who for the most part deny the reality of anything except direct physiological adaptation. True morphological adaptation, they would say, does not exist. These students regard the matter negatively: All variations may persist except those which are *too* noxious for their possessor.

Adaptation Still Unexplained

Without following through the elaborate analysis found in the text, we may say in short that nature presents us with a series of examples varying from those which are in fact rather poorly adapted to those which show a marvelous fitness for various special situations. These latter are not merely physiological but often definitely morphological, and in general are fully developed in advance of actual need. But a satisfactory explanation of them still

escapes us. The grades of adaptation are too varied and the evidence of evolution too overwhelming for us to consider seriously the special preordination of Cuvier. In all the years since Lamarck, no one has convinced the world of the hereditary transmission of any "acquired character." To attribute to blind chance some of the marvelously adaptations which we meet would appear to Caullery as näive and unscientific.

Development of the Theory of Evolution

It is a rather interesting idea that a necessary precursor for the theory of evolution was a concept of the species as a definite entity. Leaving aside philosophical speculations of the ancients, we may seek the foundations for modern evolutionary thought in the writings of John Ray who, in the seventeenth century, formulated the notion of species based on the similarity of individuals and the transmission of traits from generation to generation. After that came Linneaus and the dissemination of the species concept with its implied emphasis on exact hereditary transmission. Another fifty years for the thorough establishment of this viewpoint and Larmarck was ready to present his theory which centers around the two well known ideas: (1) Use of an organ or part increases, strengthens, and fortifies it, while disuse results in reduction or, indeed, complete atrophy. (2) Whatever nature has enabled the individual to acquire or lose tends to be transferred to the offspring where it may become a part of the common heritage of the descendants.

Lamarck is given credit for being unquestionably the first transformist. This entitles him to great honor which should not be completely dimmed by the fact that he was no match for Cuvier in an argument, that he was accused of reasoning in circles, or that Huxley made sport of him with his *"buccinator tantum."* The crux of Lamarckism is that species do, indeed, undergo evolutionary change and that the impetus for this change comes from within.

Later Darwin and Wallace came forward with their well known deductions, and gave us the evidence that evolution is directed from without. They were followed by the great popularizers, Huxley and Haeckel, and the development of that body of classical evolutionary theory which belongs to the latter part of the nineteenth century. Caullery comments that the idea of natural selection or survival of the fittest appealed to many as so obvious that proof seemed superfluous. One must seek long and carefully to find even a few good experiments designed to test the theory. Nevertheless, many lines of biological endeavor combine to create confidence in the reality of evolution.

Newer Theories

In the meantime a distinction between germplasm and soma was being emphasized by Weissmann and foundations were laid for genetics (Naudin is placed practically on a parity with Mendel by Caullery) and the mutation theory of de Vries. These developments have tended in a measure to undermine some of the older views. No longer can we think with Lamarck and Darwin of evolution in terms of adult structure. The adult, as such, has nothing to contribute. Only the germ cell in which the adult is, in a sense, preformed, holds the key to ontogeny, to morphology, to adaptation, and to all the gross features which biologists have been studying in the past. We find ourselves at last confronted with the problem of the evolution of egg cells which still seem to differ little from each other except in their developmental potentialities.

Naturally the elaboration of this picture calls for review of the work of many students—of Bateson's famous British Association address, and of the newer, special theories of Lotsy, Rosa, Przibram, and many others. None of them are found to be satisfactory, and ever recurring is the implication that

the main blocks for the structure we would build are still lacking.

Relation of Genetics to Evolution

In some respects it is Caullery's treatment of genetics that to many will seem weakest. For example, in discussing dominance, no reference is found to Fisher; and in the matter of genetic analysis of wild species, there is nothing relating to the studies on Crepis (Babcock et al), or other comparable work. Allusions to similar deficiencies could be multiplied, but there are plenty of interesting comments which geneticists may well consider.

Genetics, we are told, is a "laboratory subject" and its conclusions apply only in the laboratory field. The very controlled conditions which are imposed make improbable any results of evolutionary significance. Genetics leaves adaptation "wholly unexplained." Genetics, it is said, does not produce new species. Closely related species generally differ from each other quantitatively in many minor respects and are mutually sterile, while genetic races differ qualitatively in a few or many respects but are not sterile. For example, the most diversified strains of *Drosophila melanogaster* seem to differ far more than do many wild species but there is no sterility and no doubt as to the specific unity—a new species has not been created. If one does not agree with all these sentiments he may at least find them worth considering.

The Real Problem

When we review the whole field, we find ourselves confronted with an enigma. It seems certain that evolution has taken place, and we see factors that might have contributed to the process, but we know of nothing that is adequate to explain it. Adaptation is in large measure a matter of adult structure, yet its basis lies in the egg, and we know of no way in which the physiological adaptation of the individual can be transmitted to the germ cells. Finally, species are not, as Darwin and others thought, in a state of flux; they are essentially stationary — "genetics shows that." Might it not be, we are asked, that species and groups pass through periods of plasticity during which new species are rapidly evolved, and then relapse into a state of relative fixity when mutations result in changes of no evolutionary significance whatever? Among all the Carnivors in the world today, none appears to be in process of becoming a cetacean; and yet, there is abundant reason to believe that once in the history of animal life, this feat was accomplished. Among questions such as these, Professor Caullery leaves his reader to reflect on the problem of evolution. The reviewer suspects that few will read this book without feeling anew that most of the great biological discoveries are still in the future.

C. H. Danforth

Stanford University.

How We Get That Way

THE EVOLUTION OF HUMAN BEHAVIOR, *by* Carl C. Warden, Assistant Professor of Psychology, Columbia University. Pp. 248. Price, $3.00. The Macmillan Co., New York, 1931.

THE majority of books on evolution have dealt, as the author of the present volume points out, more with anatomical changes than with behavior. ". . . . the evolution of human intelligence, with its almost limitless capacity for cultural development, should be regarded as the central theme of the general problem. The facts of structural evolution must find their larger meaning in terms of the cultural evolution which they have made possible.

The psychological approach, growing out of this conception, offers a new orientation in interpreting the biological and sociological factors involved. The present treatment is as non-technical as could well be without sacrificing something of scientific accuracy."

In two hundred and fifty pages Professor Warden has sketched a broad background of the morphological data supporting organic evolution against which he has logically drawn in figures of historic man with his changing possibilities. It is inevitable that morphological details take up more of the book than the title might suggest, but nevertheless, I know of no one book that touches on so many different aspects of human evolution. The author has condensed into his pages the results of his readings in many fields—a summarization that perhaps makes these pages just a bit difficult to read, clearly written though the text is.

The scope of the book is indicated in the chapter titles: I. Some problems of human evolution, II. The natural kinship of man and animal, III. When anthropoid became human, IV. Traces of early man, V. The coming of modern man, VI. Race and civilization, VII. Present trends in evolution.

The entire treatment is logically mechanistic even to speculation on the origin of life itself which undoubtedly will strike fire in quarters that are usually given to burning up on such issues. It is not my opinion that the book is as popularly written as the writer hoped or as scientific necessity would permit, but nevertheless the story is presented in a thorough and scholarly fashion and is well worthy of consideration as a text or as outside reading in any course in organic evolution.

ROBERT T. HANCE.
University of Pittsburgh.

Science Interpreted

SCIENCE TODAY AND TOMORROW. Compiled from a series of lectures delivered at Moreley College. Pp. 196. London: Williams and Norgate, Ltd., 1932.

The object of the lectures on which this volume is based was to give the audiences of working men and women an outline of the progress recently made in each of the principal fields of science. Consequently the subjects are treated in as simple and non-technical a manner as is possible in matters leading to the uttermost limits of human knowledge. The lecturers included some of the most distinguished British scientists. The chapter on astronomy is by Sir Frank Dyson, the Astronomer Royal; that on psychology by Dr. Emanuel Miller; while Professor F. G. Donnan discusses recent discoveries in chemistry and radiation; Professor G. Elliot Smith, in the chapter on anthropology, elaborates his much-discussed views regarding the origin and dissemination of civilization, looking to Egypt as the cradle of the agriculture, invention and pictorial art which later diffused throughout both eastern and western hemispheres. The chapter on medicine, its present development and possibilities, by Dr. Jane Walker, likewise has much interest to the geneticist, as has also Joseph Needham's discussion of some of the present problems in biology, including particularly his analysis of the laws of growth. To many readers the foundation of scientific rationalization — mechanism — will prove the most stimulating chapter of the book, while others will find interest and enlightenment in Sir Arthur Hill's chapter on botany or Professor W. T. Gordon's lecture on geology.

The mastery of the contents of this little volume will give the reader an up-to-date view of some of the more important problems in the sciences which many of the best minds of our day are struggling to solve.

W. R. COE.
Yale University.

DOUBLE FLOWERS AND MULTIPLE FRUITS OF THE JAPANESE APRICOT*

Clayton O. Smith
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THE Japanese apricot (*Prunus mume,* Sieb and Zucc.), is one of the desirable ornamental trees of Japan. Although selected seedlings have been distributed by the United States Department of Agriculture, and have been advertised and sold by many nurserymen of California, their planting in the state is not extensive.

Two chance seedlings of this species have been under observation for some years at the Citrus Experiment Station. They were grown in an experimental planting from seed imported about 1919 by the United States Department of Agriculture. These seedlings have large fragrant double flowers. One has white and the other reddish purple blossoms.

It is worthy of note that the double flowers of these two varieties should often also have multiple fruits, several fruits originating from an individual flower. This characteristic was not observed in other double or single flowering trees of this species, and can not be regarded as associated necessarily with double flowers. These fruits are of uniform size, normal shape, have kernels with well developed embryos, and are produced in groups of one to four from an individual flower. Sometimes as many as five rudimentary fruits are formed, but some of these are probably unfertilized and become abortive. The fruits are individual carpels and each has its own attachment to the stem. They are not coalesced as is usually the case with twin fruits of almonds, peaches and plums.

Figures 7 shows different ages, sizes, and the smaller abortive fruits. Different counts of a number of fruit clusters developing from individual flowers were made on the reddish purple flowering variety, and in 100 counts showed the following:

47 flowers had 1 fruit
43 flowers had 2 fruits
9 flowers had 3 fruits
1 flower had 4 fruits

The counts made on the white double flowering variety were in close agreement with these results: of 81 fruits, 41 were single, 31 double, and 9 triple.

The white flowering variety (Figure 6, lower) was in bloom January 15, 1932, at a time when flowering shrubs in California were rare. The tree is about 12 feet high with a spread of 8 feet. The shoots are thickly covered with roselike flowers. These are about an inch in diameter, white, fragrant, double, with three rows of petals, and numerous stamens. The bloom does not appear all at once, so the tree may be in flower for some time. The length of the blooming period depends much upon weather conditions. When in full flower the tree is a mass of white, and is agreeably fragrant.

The reddish purple flowering variety (Figure 6, upper) is much like the white one except for the color, the later appearance, and the smaller size of bloom, which is about three-quarters to an inch in diameter. The petals form several circles about the numerous stamens and are a reddish purple (pansy purple, Ridgeway†). The calyx lobes are frequently six in number instead of the typical five (see Figure 7). The flowers appeared February 15, 1932. The bark of the

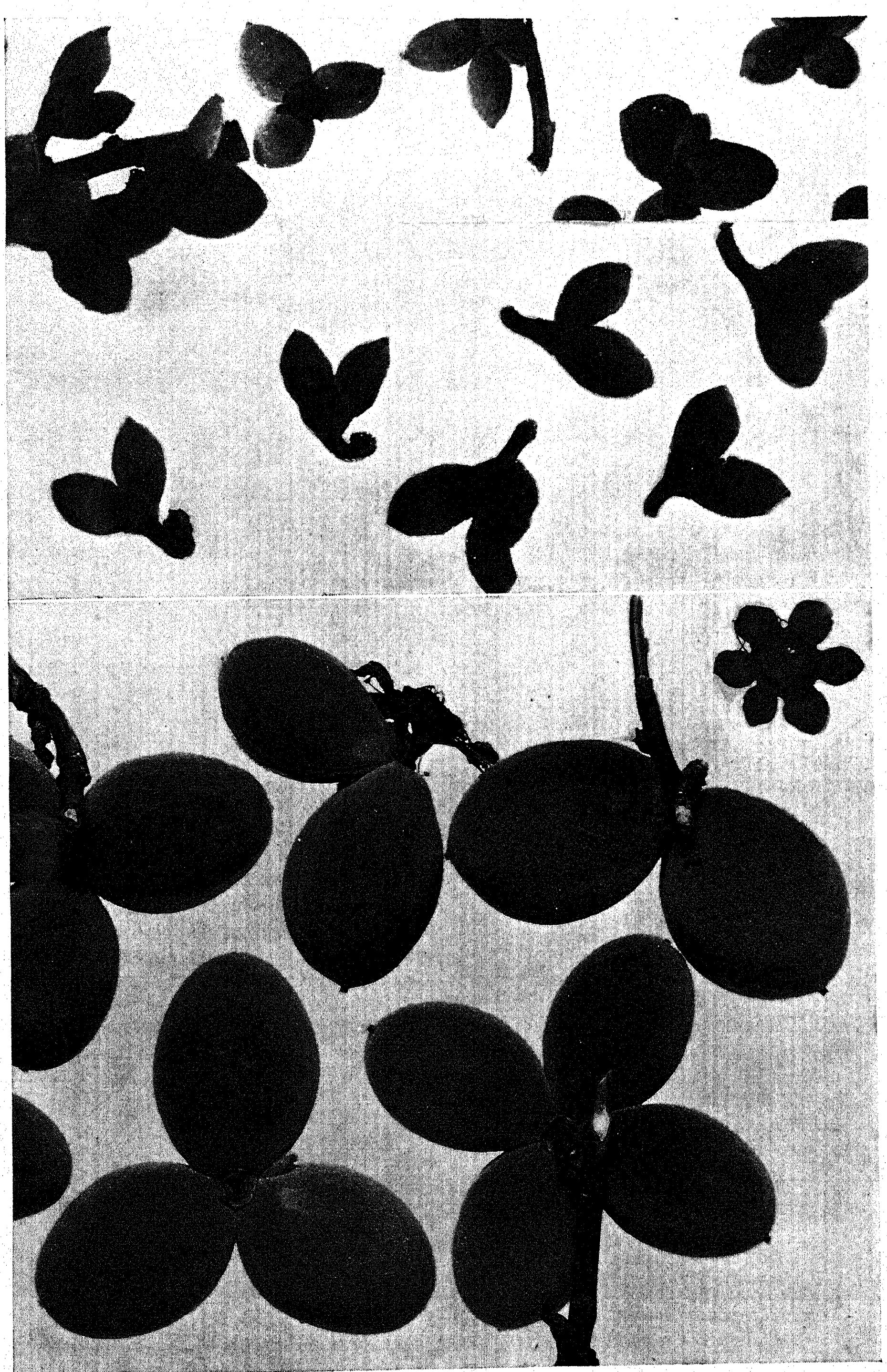
*Paper No. 272, University of California Graduate School of Tropical Agriculture and Citrus Experiment Station, Riverside, California.

†All color references are to Ridgeway, Robert. Color Standards and Color Nomenclature. Washington, D. C., 1912.

DOUBLE FLOWERS OF JAPANESE APRICOT

Figure 6

Two chance seedlings of *Prunus mume* are of interest because of the production of double flowers and multiple fruits. The white-flowered seedling flowers a month earlier than the pink-flowered (above).



MULTIPLE FRUITS FROM DOUBLE FLOWERS

Figure 7

Two stages in the development of multiple fruits from the double-flowered apricots shown in Figure 6. Other double-flowered forms of this species have not been observed to have multiple fruits. What the relation is between the two characteristics has not been determined. The six-lobed calyx often found in the pink flowered form is shown at the right.

young growth is reddish purple to dark perilla purple. The older bark and the cut surface of the older wood has a purple color, but the wood of the young shoots may be white. The bark of the young growth of the white flowering variety is light green to lettuce green, and on parts exposed to the sun it is reddish purple. The reddish purple variety, which is about eight feet high, is more dwarfed than the white variety. The flowers are fragrant, but possibly not so fragrant as those of the white variety.

The important characteristics of the white variety are its early season of flowering, and the large size of flower and tree. The purple flowers of the other variety are interesting and the smaller size of the tree is more suited to some landscape uses.

Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the readers of the JOURNAL *will be reviewed in later numbers.*

EMIGRATION, MIGRATION AND NOMADISM, *by the late* WALTER HEAPE, M. A., F. R. S., Edited with a Preface *by* F. H. A. MARSHALL, Sc. D., F. R. S. Pp. 369. Eleven Chapters. Price 12s 6d net. W. Heffer & Sons, Ltd., Cambridge, England. 1931.

COMMON SENSE AND THE CHILD, *by* ETHEL MANNIN. Pp. 314. 20 Chapters. Price, $2.00. J. B. Lippincott Co., Philadelphia. 1932.

Mrs. Mannin evidently had a very repressed early environment. As a result her Declaration of Independence for children leans amply far the other way. After all, Man *is* a social creature, and one wonders (without being sure) just how the implications of this fact are to be inculcated without undue restriction of child-personality. The book has many excellent points.

THE SCIENTIFIC BASIS OF EVOLUTION, *by* THOMAS HUNT MORGAN, Director of Kerckhoff Laboratories, California Institute of Technology. Price, $3.50 New York, W. W. Norton & Co. 1932.

When we strip evolution of all theories, hypotheses and wild guesses, and confine ourselves to demonstrated facts and legitimate implications from these, as Dr. Morgan has done, genetics has progressed far enough to afford material for a most interesting and inspiring book.

WILL IT BE A BOY? Sex-Determination According to Superstition and to Science. *By* DR. F. OKLAND, Assistant Professor, Aas. Norway. 15 Illustrations. Pp. 116. Price, $1.50. The Century Co., New York. 1932.

Superstitions of sex-determination exploded and the chromosome mechanism discussed, with speculations on the possibility of influencing the sex-ratio.

CHROMOSOMES AND PLANT-BREEDING. *By* C. D. DARLINGTON, Ph. D., D.Sc., Cytologist, John Innes Horticultural Institution. Pp. 112. 25 Illustrations. 17 Chapters. Price. $1.75. The Macmillan Co., New York. 1932.

Recent co-advances in cytology and genetics make the art and science of plant improvement more interesting, perhaps more fruitful, but hardly more outwardly simple than former casual methods of plant breeders.

PROSPECTING FOR HEAVEN: Some Conversations about Science and the Good Life. *By* EDWIN R. EMBREE, Director of the Rosenwald Foundation. Pp. 185. The Viking Press, New York. 1932.

Science has been hard on Elysian Fields; can it find a heaven this side the stratosphere?

HERITABLE CHARACTERS IN MAIZE

XLIII—Zebra Seedlings

H. K. Hayes*
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ZEBRA seedlings first appeared in 1924 as segregates in a first year selfed line of Longfellow yellow flint. Selfed zebras were grown again in 1925 and in 1929 crosses were made with various linkage testers. It received its name because of its close resemblance to zebra plant described by Demerec.[1] Zebra seedlings when grown to maturity and selfed bred true for the character.

Unlike zebra plants, the seedling character can be separated easily from normal in the seedling stage. The best idea of the character can be gained from an examination of the photograph (see Figure 8). The characteristic appearance consists of somewhat irregular chlorotic cross bands which appear on the leaves when the seedling has reached the two or three leaf stage. Before the plant reaches maturity the characteristic zebra banding disappears entirely. Zebra seedlings are somewhat less vigorous than normal seedlings in the same culture.

Segregating progenies were grown in sand during the winter of 1930-31 at University Farm, St. Paul. One group of progenies was grown in sand in a warm greenhouse where the air temperatures averaged between 70 to 75°F. and the variation in temperature was rather great. The seedlings were watered with a nutrient solution when about in the three leaf stage. Segregation occurred in a ratio of 414 normal to 119 zebra and the germination percentage was 96.6. The deviation from a 3:1 ratio is 14.3 ± 6.7. It is suggested that the factor symbol zb_8 be used to represent the character.

Another similar group of F_2 crosses was grown in flats in a greenhouse where the soil temperature approximated about 60°F. The temperature was kept relatively constant by means of thermostatic control and deviated no more than ± 2° from this average. Although this group of progenies remained under these conditions until the four-leaf stage and were carefully examined no zebra seedlings appeared. The seedlings which were grown until about the four-leaf stage under a constant soil temperature of 60°F., without any evidence of the appearance of the zebra character, were taken to the warmer house and watered with a nutrient solution. After about a week the characteristic zebra appearance could be detected and at the end of about a 10-day period there was a ratio of 347 normal to 100 zebra. The deviation from a 3:1 ratio is 11.8 ± 6.1.

The results during the winter of 1930-31 made it seem probable that growing seedlings under conditions where they developed favorably would lead to the expression of the zebra seedling character. Several zebra plants were self-pollinated during the summer of 1931. Only a small amount of seed was obtained. This is to be explained as being a result of the severe drought, rather than indicating a lack of fertility due to the zebra seedling character. Seed of the progenies of selfed zebras and of a further series of F_2 progenies of zebra crosses was sown in soil in a house where the temperature was controlled. A soil temperature of approximately 60°F. was used until the plants were about

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* Chief, Division of Agronomy and Plant Genetics, University Farm. Mr. R. F. Peterson, Research Assistant, gave efficient aid in the studies here reported. Paper No. 1098 of the Journal Series, Minnesota Agricultural Experiment Station.

†Demerec, M. Heritable characters of maize—X. Zebra striped leaves. Journal of Heredity 12: 406-407. 1921.

six inches high and at this time warmer temperatures were used. The seedlings were grown close together and artificial lights were turned on from 12 p. m. until morning. No clear cut segregation of zebras was obtained and while a slight tendency to develop the character was noted the differences between zebra seedlings and normal did not make it possible to classify the seedlings accurately. This led to a careful study of the conditions necessary for the development of zebra seedlings.

Temperature Effect on Development of Zebra Seedlings

Selfed progeny of a homozygous zebra seedling plant were used to study the conditions necessary for the development of the zebra seedling character. In the first study carried out, seedlings were planted in separate flats both in sterilized and unsteamed soil and in sterilized and unsteamed sand. Seed, used for a part of the flats, was treated with Semesan Jr. which is known to aid in protecting the seed from diseases which cause seedling blight. As sterilization of the soil and seed treatment with Semesan Jr. had no affect on the development of zebra seedlings, no further reference will be made to these phases. In the first study of conditions necessary for the development of zebra seedlings, two groups of tests referred to as *A* and *B* were carried out:

A. Seed was sown on Nov. 24 and emergence occurred on Nov. 29. The following approximate air temperatures were maintained: Nov. 24-Dec. 6, 86° F; Dec. 7-14, 71° F: Dec. 15-19, 64° F; Dec. 20-22, 77° F. The houses were under thermostatic control which maintains soil temperatures with a fluctuation of about ± 2°F.

B. Seed was sown on Nov. 24 and emergence took place on Dec. 3. Temperatures maintained: Nov. 24-Dec. 6, 70°F.; Dec. 7-14, 71°F.; Dec. 15-19, 64°; Dec. 20-22, 77°.

Seedlings in trial *A* grew rather spindly as a result of the high temperatures during the early period from Nov. 24-Dec. 6. Because of this condition the seedlings grown by the method described under *B* were somewhat more satisfactory. It will be noted that trials *A* and *B* differed only in that high temperatures were used during the early period from Nov. 24 to Dec. 6 in series *A* and moderate temperature in test *B*. In both trials the characteristic zebra appearance of the seedling leaves was obtained during the final period from Dec. 20-22. The period of low temperatures, Dec. 15-19, caused a retardation in the development of the seedlings. The more rapid growth occurring during the period from Dec. 20-22 led to the development of the characteristic zebra appearance.

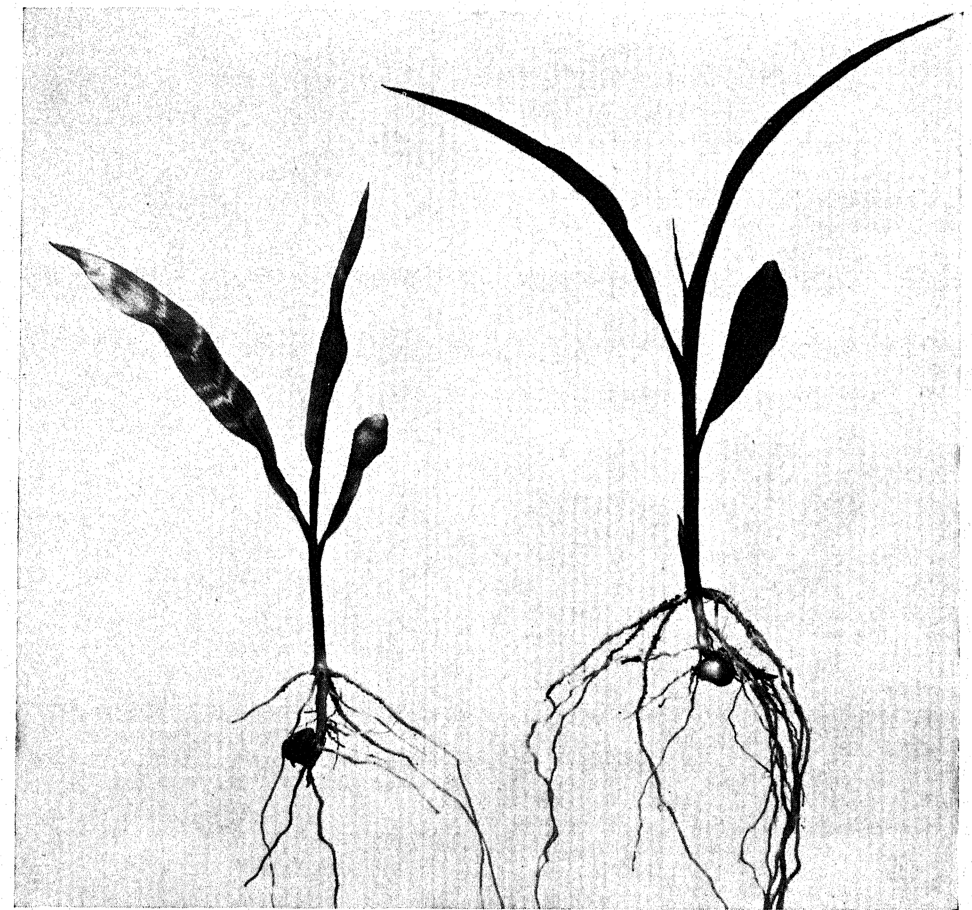
A second trial was made under the conditions given in Table I.

Seedlings in the *A, B, C* and *E* trials were in the three leaf stage on Jan. 5. Thre was no sign of the zebra character. Zebras appeared about Jan. 9 in trials *B* and *C* but not in *A* and *E*. This proves conclusively that constant high temperatures, trial *A,* do not lead to the development of zebra seedlings. Zebras do not develop when the seedlings are started under high temperaturs and then grown under constant low temperatures (trial *E*). Plants in *D* and *G* were in the one-leaf stage Jan. 5 while those in *F* had emerged but no leaves had developed. In trials *B* and *C,* zebras developed, proving that moderate to high air temperatures for the greater part of the time with a 24 or 48 hour period under colder temperatures, 52-54°F., and a return to the higher temperatures was conducive to the development of the zebra apparance.

A further trial was made to determine with a greater degree of accuracy the most desirable method of growing zebra seedlings under greenhouse conditions.

Progeny of homozygous zebra plants were grown in the greenhouse with an average air temperature of 82°F. Beginning on Feb. 29, when all plants were in the three-leaf stage, the seedling plants were exposed to the temperatures given in Table II.

The plants under the constant temperature, trial *A,* appeared normal but



ZEBRA SEEDLING COMPARED WITH NORMAL

Figure 8

Zebra seedling at left and normal at right from the F_2 generation of a cross between zebra and normal. These seedlings were selected to give an idea of the average vigor of the two groups of seedlings. The zebra banding appears only under certain environmental conditions.

indistinct zebra stripes could be found on all seedlings. The seedlings in tests *B, C* and *D* were all clearly zebras. The stripes were clearest in test *D* but *B* and *C* were quite satisfactory.

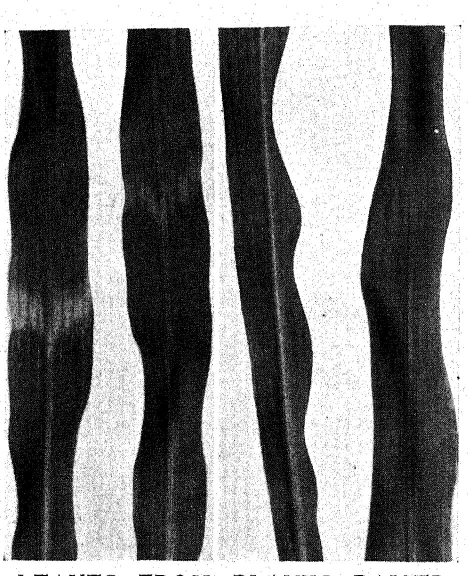
In another comparison the progeny of selfed zebra plants were grown in air temperatures of approximately 75° until the three-leaf stage. At this time four plants were placed for two days in a greenhouse with air temperatures of approximately 55°F. and then returned to the temperature of 75°F., for a period of five days and compared with seedlings, used as controls, remaining constantly under the temperature of 75°F. The seedlings grown for controls remained normal, the four treated plants, two days under 55°F., showed distinct stripes as illustrated in Figure 9.

Linkage Relations

When a homozygous zebra plant from the Longfellow variety was crossed with *Pr* tester the resultant

seeds were all purple aleurone proving the fact that the zebra seedling carried *Pr* in a dominant condition. When crossed with the *A* tester purple seed was obtained also while the cross with the *R* tester gave colorless seed. The results of linkage studies of zebra seedling and six other factor pairs in known chromosome groups are summarized in Table III.

Linkage studies were made with *A*, *Pr* and *R* in the cross between H194, zebra seedling, colorless aleurone and H6, *A* tester. The product method (Immer, 1930)* was used. The ears segregated for colored *vs* colorless aleurone and the color of the aleurone was of two sorts, purple and red. The cross was in the repulsion phase in relation to the *Prpr* factor pair, segregation for purple *vs* red aleurone and zebra *vs* normal seedlings occurring in the ratio of 255 purple aleurone, normal seedlings : 92 red aleurone, normal : 72 purple aleurone, zebra : 27 red aleurone, zebra. The calculated percentage of new combinations by the product method was 50.5 ± 2.4. This indicates that the *Pr* factor is inherited independently of the zebra seedling character. Segregation for aleurone color and normal *vs* zebra seedlings occurred in the cross H194 $\times$ H6, described in the previous paragraph. These results were obtained: 449 colored aleurone, normal seedlings; 382 colorless aleurone, normal seedlings; 140 colored aleurone, zebra seedlings; 97 colorless aleurone, zebra seedlings. The segregation for aleurone color obtained was 639 colored : 479 colorless. This is a deviation from a 9:7 ratio of 10.1 ± 11.3. Apparently segregation for aleurone color is dependent in this cross on two complimentary factors which in this case must be *A* and *R*. The linkage relations between Zb_Szb_S and *Aa* could be studied in the repulsion phase and that between Zb_Szb_S and *Rr* in the coupling phase. The product method was used. For repulsion, i.e. with the *Aa* factor pair the percentage of new combinations was 47.5 ± 2.4 and for coupling in relation to the *Rr* factor pair the percentage of new combinations was 52.5 ± 2.2.



LEAVES FROM PLANTS RAISED UNDER DIFFERENT CONDITIONS

Figure 9

At left, leaves of seedlings, in a homozygous zebra line, grown under a constant air temperature of approximately 75°F. until the three leaf stage, then placed for two days in an air temperature of 55°F. and then returned to the former condition. At right, leaves of seedlings kept constantly under an air temperature of approximately 75°F.

Studies were made of linkage relations of Zb_sZb_s, with *Lglg*, liguleless; *Flfl*, flinty *vs* floury; and *Yy*, yellow *vs* colorless endosperm. The percentage of new combinations in these crosses is given in Table III. None of these characters appear to be linked in inheritance with zebra seedlings.

Summary

1. Zebra seedling (zb_s) behaves as a simple recessive to normal. The characteristic irregular cross bands appear on the leaves when the seedlings reach the two or three-leaf stage. Before reaching maturity this "zebra" banding disappears entirely.
2. Homozygous zebra corn seedlings

*Immer, F. R. Formulae for Calculating Linkage Intensities. *Genetics* 15: 81-98. 1930.

grown in a greenhouse with fairly uniform temperatures either show no zebra stripes or these are very indistinct. If seedlings at about the three-leaf stage are subjected to a temperature of 55 to 60°F. for one or two days and then placed under higher temperatures for a few days (70 to 85°) the stripes become distinct in from one to several days. Repeating the treatment seems to make the stripes plainer.

3. Under field conditions in Minnesota there appears to be sufficient variation in temperature so that the character can be differentiated easily.
4. From a study of F_2 segregating populations, zebra appears to be inherited independently from the aleurone color factors, *Prpr, Rr* and *Aa*. It was inherited independently also of the factor pairs *Yy, Flfl* and *Lglg*.

TABLE I.—Duration of time and air temperatures, Fahrenheit, under which the tests of the development of zebra seedlings were conducted.

Test	No. of Seedlings	Air Temperatures, Degrees F			
		Dec. 28-Jan. 5	Jan. 5-6 24 hours	Jan. 6-7 24 hours	Jan. 7-11
A	4	80-84	80-83	79-83	73-84
B	2	"	52-54	79-83	73-84
C	2	"	"	52-54	"
D	2	46-63	58-61	58-62	73-84
E	2	80-84	52-54	52-54	50-54
F	2	52-55	52-54	52-54	50-54
G	2	46-63	58-61	58-62	About 60

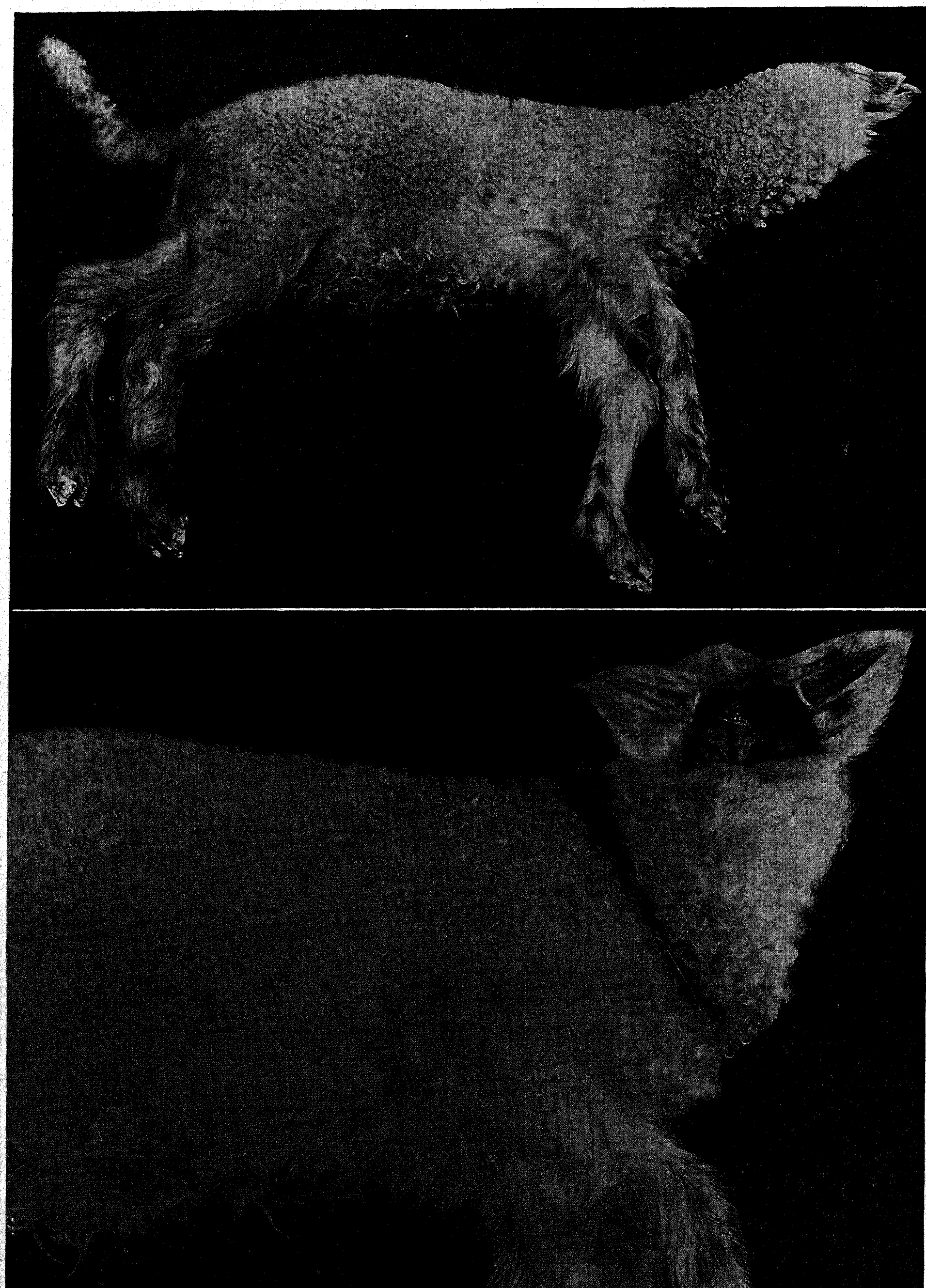
TABLE II.—Temperature conditions in a series of trials with the selfed progeny of homozygous zebras. Seedlings were in the three leaf stage at the time the trial was started.

Test	No. of Plants	Average Air Temperatures, F., each column represents 1 day						
A	7	72	72	72	72	72	72	72
B	6	55	72	55	72	55	72	72
C	6	55	55	72	72	72	72	72
D	6	55	55	72	72	55	55	72

TABLE III. Percentage of new combinations in the F_2 of crosses between $Zb_s zb_s$ and *Prpr, Aa, Rr, Yy, Flfl* and *Lglg*.

Phase	Factor Pair	Number of Individuals				Recomb.
		AB[1]	Ab	aB	ab	%
F_2 repulsion	Prpr	255	92	72	27	50.5 ± 2.4
"	Aa	499	382	140	97	47.5 ± 2.4
F_2 coupling	Rr	499	382	140	97	52.5 ± 2.2
F_2 repulsion	Yy	596	178	154	38	47.3 ± 1.7
"	Flfl	403	371	97	93	50.8 ± 1.6
"	Lglg	351	119	115	32	52.8 ± 2.0

[1] *A* and *a* represent normal and zebra seedling while *B* and *b* represent the other factor pair concerned.



A HEADLESS LAMB

Figure 10

Side view of lamb, showing general appearance, and a closer view of the fore part, showing small papillae in the depressed region between the ears where head ordinarily develops.

A HEADLESS LAMB

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RECENTLY one of my students brought in a lamb, of male sex, born on his father's farm at Cottage Grove, Oregon, on the evening of March 26, 1932, which appeared to be perfect in all respects with the exception that it did not contain any head. (Figure 10). On questioning the student, I learned that he was present when the monstrosity was born. The ewe which gave birth to it was in labor for a long period, practically the entire day, and when the lamb finally made its appearance, it was stillborn. The afterbirth appeared to be normal in all respects. Moreover, the lamb itself was of normal size, being approximately seventeen inches in length from ears to tail and about eleven inches in height. With the exception of being minus a head, the animal seemed to possess all of the other structures of a normal individual.

The ram which sired this headless lamb was a registered purebred Shropshire, while the ewe which gave birth to it was the product of a cross between the Shropshire and Cotswold breeds of sheep. On a previous occasion, this ewe gave birth to twins and these were normal in every respect. An external examination of the monstrosity revealed that where the head is ordinarily found, extending forward between the ears, there existed a series of small papillae. (Figure 10). No openings of any character, such as mouth and nostrils could be discovered. What an internal examination will reveal cannot be stated at this time. The specimen has been preserved for future examination and study.

A COLOR-MOSAIC IN THE MOUSE

A Mutational Mosaic (Brown-dilute Brown) Coat Pattern in a Mouse

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THE number of mosaic individuals which has been recorded in mammalian genetics has been very small.

Several articles have been published on such results on Drosophila data. A few which may be mentioned are: Morgan and Bridges[8], Muller[9], and Morgan, Bridges and Sturtevant[8].

Castle[1] reported on the occurrence of a female guinea-pig which resembled an albino but had spots of blue. Faint pigmented streaks could be seen in the iris of each eye. He explained the phenomena, also a tricolor rat[2], as probably being due to non-disjunction.

Seven mosaic guinea-pigs have been described by Wright and Eaton.[11] Of

this number six were purely somatic in nature; whereas, the seventh showed the characteristic in part of the germinal tissue, as well as in the coat pattern.

Pincus[10] has described three mice which had black-brown coat patterns. The chocolate areas in two of the individuals showed up in positions which were normally white in the piebald strain. Another mosaic mouse has been described by Fisher,[5] tricolor in nature, as probably the result of a somatic mutation.

Castle[3] has also reported on tricolor rabbits: some of the progeny showed the mosaic pattern.

Material

We have raised a large number of F_1 individuals by mating mice of our albino and dilute brown stocks, both highly inbred. These animals are brown in color, as the albino race carry brown. In a litter in the 1BC generation (F_1 ♂ 8556 × dilute brown ♀ 8267) I noticed one animal (♂ 16643) which was a mosaic.

The dorsal surface of this male was brownish in color, whereas the ventral surface was more like a dilute brown. The respective areas were atypical as each had hairs of the opposite type scattered in a heterogeneous manner. There were no spots, characteristic of other mosaics. The distribution of the brown and dilute brown hairs has remained the same since birth (May 28, 1931).

Microscopic examination of the hairs showed characteristic pigment arrangement for both brown and dilute brown colors in the respective areas.

Results

Mosaic ♂ 16643 has been mated to a number of females. The progeny are listed in Table I.

All the young, 60, which were classified by mating ♂ 16643 to dilute brown females were dilute brown in color. Thirty-five young were raised by heterozygous brown females. Of this number, 17 were dilute and 18 intense brown; a typical 1:1 ratio.

Discussion

The theory of somatic mutation will explain the majority of the mosaic mammals reported.

There are a few exceptional cases, however. Wright and Eaton[11] explained the mosaic guinea-pig (♂ No. 8400) by assuming that it "was initially c^dc^d but that a mutation to the dominant allelomorph C occurred in a cell which was ancestral both to part of the soma and part of the germinal epithelium." This male transmitted both intense and dilute gametes, the proportion of each varying from time to time, but none of the progeny showed the mosaic pattern.

Pincus[10] states that somatic non-disjunction will explain the black-brown color patterns he encountered in three of his mice. He believes, however, that it is improbable that two of the three animals would show identical spots in the area which he designated "critical for pigmentation" according to this theory. In this region, which is usually white in piebald mice, "a recessive gene may exercise a controlling effect."

The mosaic pattern which Castle[3] described in the rabbit was also present in from three to four per cent of the offspring. Thus, the condition was germinal, and might be explained in accordance with the genomere theory of Eyster.[4]

Since the animal we found does not transmit brown, the theory of non-disjunction or somatic mutation is adequate as an explanation. To be sure, it is impossible to state whether the individual was initially a heterozygous brown which lost this factor from part of the soma and the germinal tissue, or a dilute brown. In the latter case, a mutation to the intense condition has taken place in part of the somatic tissue.

Castle[2] and Wright and Eaton[11] concluded that if somatic mutation had occurred in their mosaic patterns, the descendants of the mutant cells were capable of giving rise to scattered areas. In the case of our character, brown and

dilute brown hairs were scattered throughout the fur, with the former dominating on the dorsal surface and the latter on the ventral surface.

Summary

A mosaic male sired only dilute brown young when mated to dilute brown females and gave a 1: 1 ratio when mated to heterozygous brown females. Since mosaic does not transmit brown, it is probable that the brown pigment in the pelage is due to non-disjunction, or somatic mutation.

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TABLE I.—Breeding record of mosaic ♂ 16643 when mated to dilute brown and heterozygous brown females

♀ *No.*		*Color*	*Relation*	*No. Born*	*No. Living*	*dBr*	*Br*
8267		dBr	Mother	5	0		
16644		dBr	Sister	15	13	13	0
16642		dBr	Sister	19	17	17	0
18933		dBr	Daughter	13	13	13	0
18935		dBr	Daughter	6	6	6	0
18936		dBr	Daughter	6	6	6	0
18938		dBr	Daughter	8	5	5	0
Total	×	dBr		72	60	60	0
13312		Br*	No	13	13	7	6
12476		Br*	No	25	22	10	12
Total	×	Br*		38	35	17	18

*Heterozygous.

Shull's *Heredity* Revised

HEREDITY. *by* A. Franklin Shull, 2nd ed. XV, 345 pp., McGraw-Hill. $3.00.

THE first edition of *Heredity,* by Dr. A. Franklin Shull, fulfilled a very definite need, that of a brief, clear text for beginning classes in Heredity and Eugenics in colleges and universities. The second edition brings the subject up to date and shows several improvements over the first. The addition of material on chromosomes and sex in plants and of problems at the end of each chapter are to be noted particularly among these. Other changes for the better are the expansion of the first chapter on Rise of Knowledge of Here-

dity, the revision of chapters on Immigration, Population Problems, Evolution, and Heredity in Man, the addition of sections on multiple allelomorphs and lethals. The chapters are short and unified; each is illustrated sufficiently and well. The sections on human heredity and eugenics are critical and restrained.

However excellent a text may be, there are always teachers who would alter it. The reviewer has used Dr. Shull's book for five years and in as many different classes. It has been found more satisfactory in teaching these classes, to introduce multiple allelomorphism and sex linkage after monohybrid crosses and before linkage and to postpone a discussion of evidence for location of genes in chromosomes until after the presentation of sex linkage and linkage, an order somewhat different from Dr. Shull's. In addition, the diagram of meiosis which Dr. Shull uses in his Animal Biology is to be preferred for accuracy and completeness to that in the book under discussion. These differences are of minor importance, however, and in no way reduce the value of the book which is, without a doubt, the best available for its purpose.

The use of such a text as this for a course in Eugenics is strongly to be advised. Too often is such a course concerned with the more superficial aspects of that subject while the fundamentals are neglected. No teacher or student using this text will define Mendelism as the 3:1 ratio, the conception which the student gets from the average course in psychology, sociology, and even at times, biology, and it is to be recommended to teachers and students in these related fields.

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Problems of Population

POPULATION PROBLEMS, *by* W. S. THOMPSON, Pp. xi + 462. McGraw-Hill. New York. 1930.

For many years Dr. Thompson has been engaged in a study of problems of population and he has written, besides many articles, three substantial contributions in this field before the publication of the present volume. His *Danger Spots in World Population,* published in 1929, was a timely discussion of questions of importance for human welfare and the peaceful conduct of international affairs. *Population Problems* is a thorough and well documented treatise on the general field, dealing with population from the biological, economic, social and political standpoint. Essentially, Thompson is a follower of Malthus, as we would expect to find in so competent and fairminded a student of the subject. At the same time, he is quite clear as to the errors of detail into which Malthus was betrayed. He is also a Neo-Mathusian, which Malthus was not, because he believes that birth control is a means of averting the various ills with which overpopulation threatens the race. One cannot do justice to so extensive a treatise in a short review. On the whole, Dr. Thompson's volume affords an excellent text and reference book for the student, and there is no expert in the field who would not profit by its perusal.

In his treatment of the rôle of natural selection Thompson expresses a doubt as to whether our advances in civilization have really reduced the rigor of this eliminative process. So far as I am aware, he is the only one besides the reviewer who has expressed this particular view. There is a useful bibliography of 332 titles. If one could possess only one book on population it should be this one.

S. J. HOLMES.

INHERITANCE OF DOUBLEJOINTEDNESS IN THE THUMB

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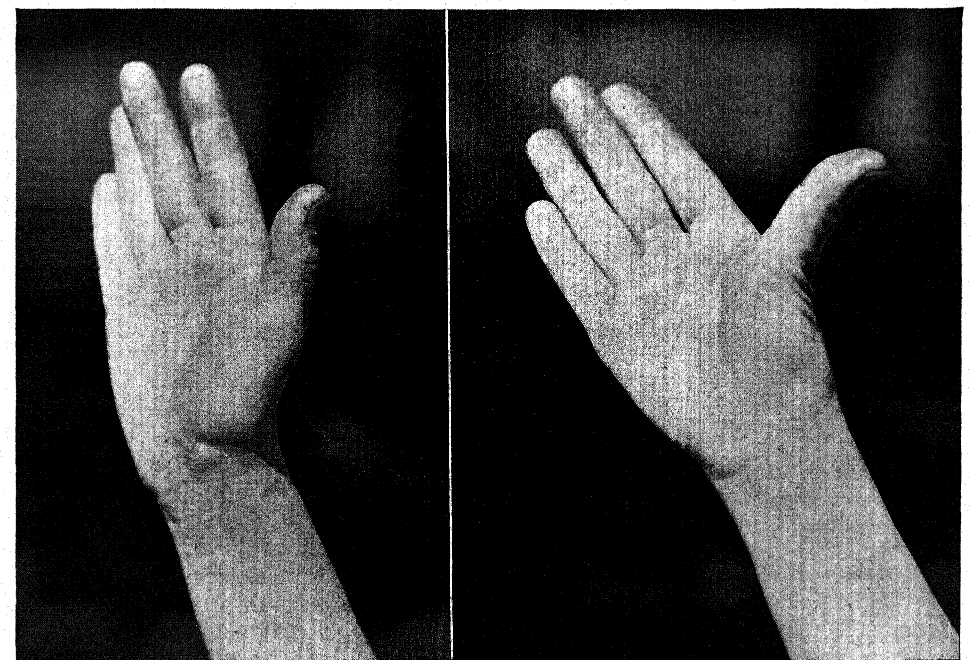


A NEW MENDELIAN CHARACTER

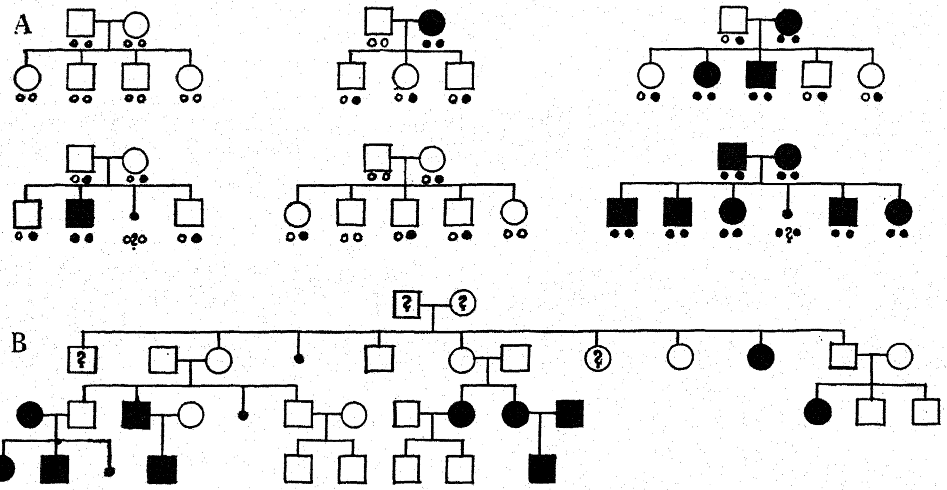
Figure 11

Most people are able to move the thumb only in one direction—inward toward the palm of the hand. A few people are able to bend the thumb outward, as in the photograph at the right. This peculiarity is inherited in some families as a recessive character.

SOME people are double-jointed in the second (basal) joints of their thumbs. The number is not as large as those who can bend their thumbs in only one direction at this joint. In one group, which was requested to raise their hands if they possessed the ability, 5% showed they did. In another group four in forty did, or 10%.

This ability, so far as I can see, will not add to the sum total of individual or group happiness, unless it be to amuse the children of the family. It might be useful in adding an aesthetic touch to the art of "thumbing" for automobile rides, to which it is hoped, even in these depressed times, few readers of the JOURNAL OF HEREDITY have to stoop. But working out the heredity of the trait does add another item to the known list of Mendelian characters. It may occasionally be useful in questions of disputed paternity, and it makes an excellent feature which can be used in the classroom in illustrating Mendelian inheritance.

For several years I have been collecting data on inheritance of this ability. In all this time I have found



INHERITANCE OF DOUBLE JOINTED THUMBS

Figure 12

A—Actual family pedigrees illustrating the six types of Mendelian inheritance. The black squares (males), and the black circles (females), indicate presence of the trait in the individual. The small dots under the squares and circles indicate the probable germinal composition, as judged by the children of these persons. *B*—Pedigree chart of a family clan showing the distribution of the ability to throw the thumb out of joint. These seven pedigree charts are fairly conclusive evidence that this characteristic is inherited in these families as a Mendelian recessive character.

no case where it has behaved as anything other than a typical Mendelian recessive. The only possible exceptions are four cases in which a parent was double-jointed in only one thumb. (All of these four happened to be the left.) In such cases, it was inherited as though the individual possessed the ability in both thumbs.

Of the families questioned, the great majority could not bend the thumbs at the second joints. But there were quite a number in which the trait was present. Taking the records collected, I have studied them to see the inheritance pattern where the grandparents, parents, and children were known. From them I find all six possible Mendelian combinations. These are presented as actual pedigrees with the theoretical germinal composition, judging from the children these persons have produced. The germinal composition is thus represented by the little circles under the larger circles or squares.

The black circles and squares represent the presence of the trait, the white the absence. A black circle or square therefore, means a homozygous recessive, whereas a white circle or square may mean either a homozygous dominant or a heterozygous individual.

The larger pedigree represents an actual family clan showing the distribution in a case where it is fairly prevalent—much more so than one ordinarily finds. This happens to be my own family group, on the maternal side.

I am at present collecting data on the inheritance of the ability to bend the thumb backward at the first joint. This characteristic also runs in families, and it is interesting that it is inherited independently of the double-jointedness in the second joint. As soon as I have sufficient data, I will present another paper which will show its inheritance pattern and how the two are inherited in families, in an independent manner.

FERTILE GOURD-PUMPKIN HYBRIDS

The Inheritance of Factors for Shape and Color in Summer Squash-Gourd-Pumpkin Crosses of *Cucurbita pepo*

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THE problem of inheritance of fruit shape and color in *C. pepo,* owing to the striking diversity of the material, has attracted many plant hybridizers. The most pertinent work on this subject has been done by Sinnott and his co-workers. In a series of papers (1922-31), they have reported the isolation of several factors for shape. The general conclusion they have reached is that each particular fruit shape is the result of a balance of factors, each factor producing a slightly different expression.

Regarding the inheritance of fruit color, Sinnott and Durham[4] have shown that in the body color of the fruit, white is dominant over yellow and yellow over green. It is stated that there may be two independent factors for white. There are apparently various modifying colors entering, which complicate the genetic analysis.

The present work has resulted from an attempt to secure hybrid fruit between the common summer squash and pumpkin types of *C. pepo* and the yellow flowered gourds (*C. pepo* var. *ovifera*). The purpose of these hybridization experiments was definitely to establish whether or not all of the various forms of *C. pepo* were cross fertile. The work reported below resulted from attempts to answer this question.

Observations

The seed was secured from commercial sources. The plants used as parent stock have been inbred for four years. During this interval no radical departures in the original fruit shape have segregated, so it can be safely stated that the parent plants were fairly homozygous for the characters involved.

Cross I

A gourd having a characteristic pear shape (Figure 13, *B* and *D*) was crossed with a summer squash of the white scallop type (Figure 13, *A* and *C*). This cross can be made reciprocally with little difficulty. The fruits resulting from a cross of this kind have a large number of well developed, viable seed.

Description of parents.—The gourd parent was typical of its kind, having small yellow flowers, with a strong running type of vine. It is extremely prolific and has dark green pear shaped fruits. The scallop parent was a selection from the Long Island Bush variety. It characteristically has the bush growth habit, with solid white, rather deeply scalloped fruits.

Table I gives a summary of the results from this cross.

This gives a close approximation to a 3:1 ratio, as a comparison of the expected and observed results show. From

TABLE I.—Segregation in Gourd-Squash Hybrids

Parents	F_1	F_2 Scallop	F_2 Pear shape
Scallop × Pear shape	Scallop	36	15
Pear shape × Scallop	Scallop	40	19
	Observed	76	34
	Expected	82.5	27.5

this it is evident that the pear shape of the yellow flowered gourd is recessive to scallop, and is dependent apparently upon a single factor difference.

Cross II

One of the nest egg gourds (*C. pepo* var. *ovifera*) was crossed with a field pumpkin (*C. pepo* var. Mammoth Tours). This cross resulted in several fruits, some of which contained viable seed. The nest egg gourd has a diameter averaging around two inches, and is oval in shape, with solid white body color. The Mammoth Tours pumpkin is one of the largest types of field pumpkins. It has large stems, flowers, and leaves, with a very large solid green colored fruit.

In this cross the writer was more particularly concerned with the inheritance of color. This is one of the few combinations in which the solid body colorings can be studied without complications by striping, variegation, etc.

The results of this cross are summarized below in Table II. From the data it is evident that the solid body color green is recessive to white and its inheritance can be attributed to a single factor difference.

Discussion

It seems that the pear shape of the gourd can reasonably be interpreted as being due to a single recessive factor. This interpretation is not directly in line with that of Sinnott and Hammond[6]. They have analyzed the inheritance of shape in a number of lines and their results have led them to the following conclusion: "A specific fruit shape seems to be the resultant of a balance between a series of factors differing in the degree and direction of their effect. Shapes which are phenotypically identical may be very diverse genotypically." However, in the light of the results of the above experiments there is no alternative except to interpret the difference in shape as simply due to a single factor. A comparable case has been described by Sinnott and Durham[4] in a cross between disc and sphere shapes. The disc shape was completely dominant to sphere. The F_2 generation gave a 3:1 ratio, thus indicating these shapes differed by but a single factor.

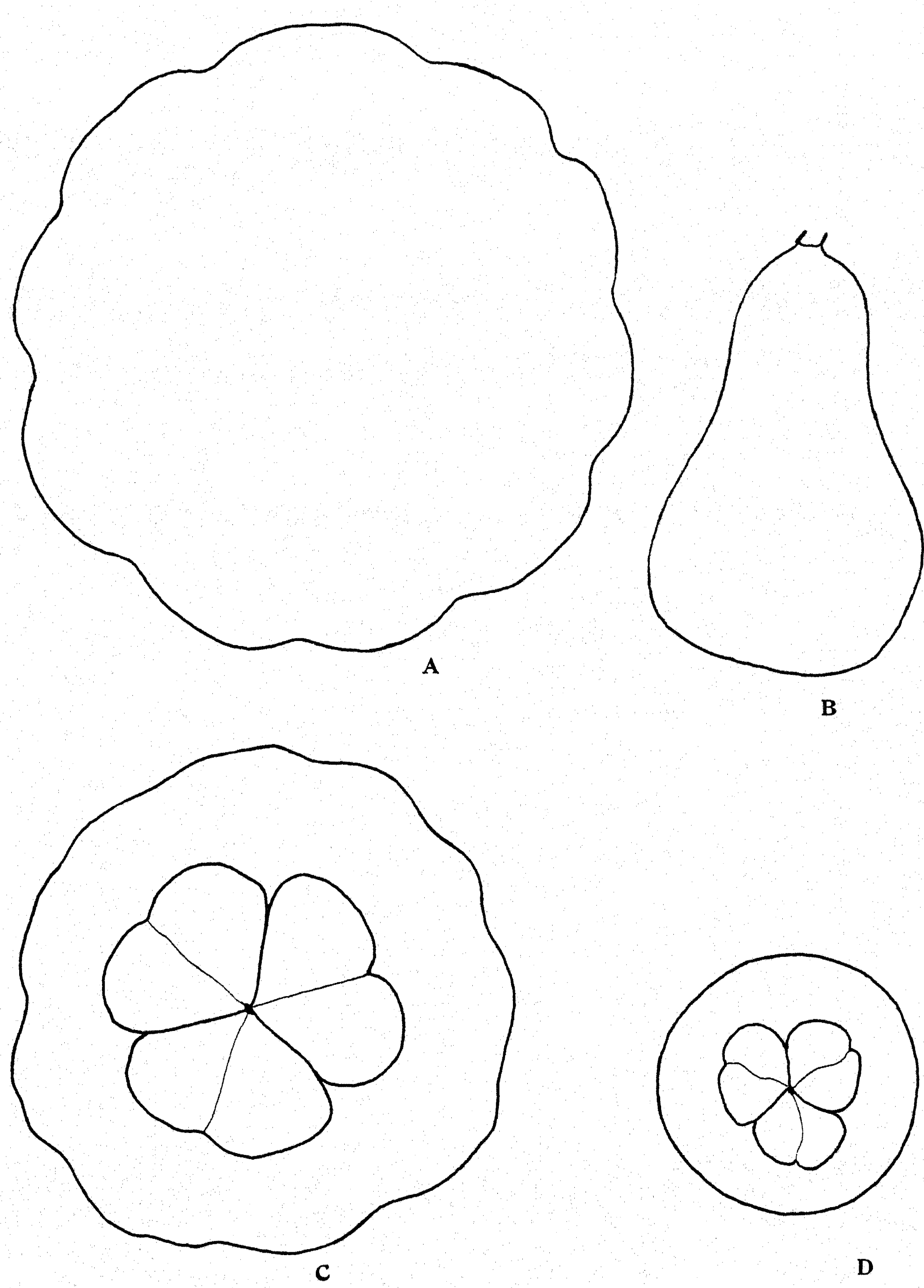
There are other shapes in *C. pepo* which appear to be very complicated and inherited in a manner very difficult of analysis. The cases in which the inheritance of shape is as simple as that described above are very limited. Commercial seed, as a rule, is extremely heterozygous. When a variety is planted it very often segregates into three or four different classes as far as their shapes are concerned. Consequently material of this type is valueless in genetic experimentation until comparatively true breeding stocks have been secured by continuous inbreeding.

In body color of fruit, there are three basic colors, white, yellow, and green. Combinations of these three form a whole series of colors. The inheritance of these seems to be very complex. Environmental conditions, such as nutrition, drought, light, etc., influence these three basic colors and affect the color scheme profoundly. With these various factors at work there appears a whole range of color variation.

In the particular case chosen for this study, the parent stock (Mammoth

TABLE II.—Color Segregation in Gourd-Pumpkin Crosses

Parents	F_1	F_2 Nest Egg (white)	F_2 Pumpkin (green)
Pumpkin × Nest Egg (green) (white)	Nest Egg (white)	35	17
Nest Egg × Pumpkin (white) (green)	Nest Egg (white)	36	13
	Observed	71	30
	Expected	75.75	25.25



SIZE DIFFERENCES IN SQUASH-GOURD HYBRIDS

Figure 13

The parents of the reciprocal cross of a scallop type of summer squash (*A-C*) and a gourd having green pear shaped fruit (*B-D*) was made without difficulty. The F_1 fruits were all scalloped. In the F_2 generation there was typical monohybrid segregation for fruit shape.

Tours) possessed a light green colored fruit. In F_2 the green segregates ranged from very light green to a dark green closely approaching black. An explanation of this situation obviously lies in an investigation of true breeding strains of the greens.

Summary

1. It has been definitely proved that the yellow flowered gourds (*C. pepo* var. *ovifera*) and the summer squash and pumpkin types of *C. pepo* are cross fertile.

2. The pear shape of the gourd is a simple Mendelian recessive to the scallop shape of the summer squash.

3. The green body color of the pumpkin is recessive to the white body color of the nest egg gourd and differs from it by a single major factor.

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6. ——————, and DOROTHY HAMMOND, Factorial balance in the determination of fruit shape in *Curcurbita. Am. Nat.* 64:509-524. 1930.

Sex-Linked Factors Governing Milk Production

A study of the progeny of 728 bulls of the Red Danish breed reported by Dr. Karl Madsen in *Nature* (London), Jan. 30, 1932, suggests strongly that some of the factors governing milk production are sex-linked in their inheritance. The study is based on the fact that a male cannot have sex-linked characters transmitted to him by his paternal grandam. The correlation between the producing ability of the bull's maternal grandam and the bulls' transmitting ability (0.112) was five times as large as the correlation between the bull's paternal grandam and his transmitting ability (0.026). If sex-linkage were not a factor the correlations would be expected to be the same. In a character like milk secretion, which is transmitted by the male but not expressed, such indirect methods are practically the only way that sex-linkage can be demonstrated. Whether Madsen's data are conclusive evidence of the existence of such factors is perhaps open to question.

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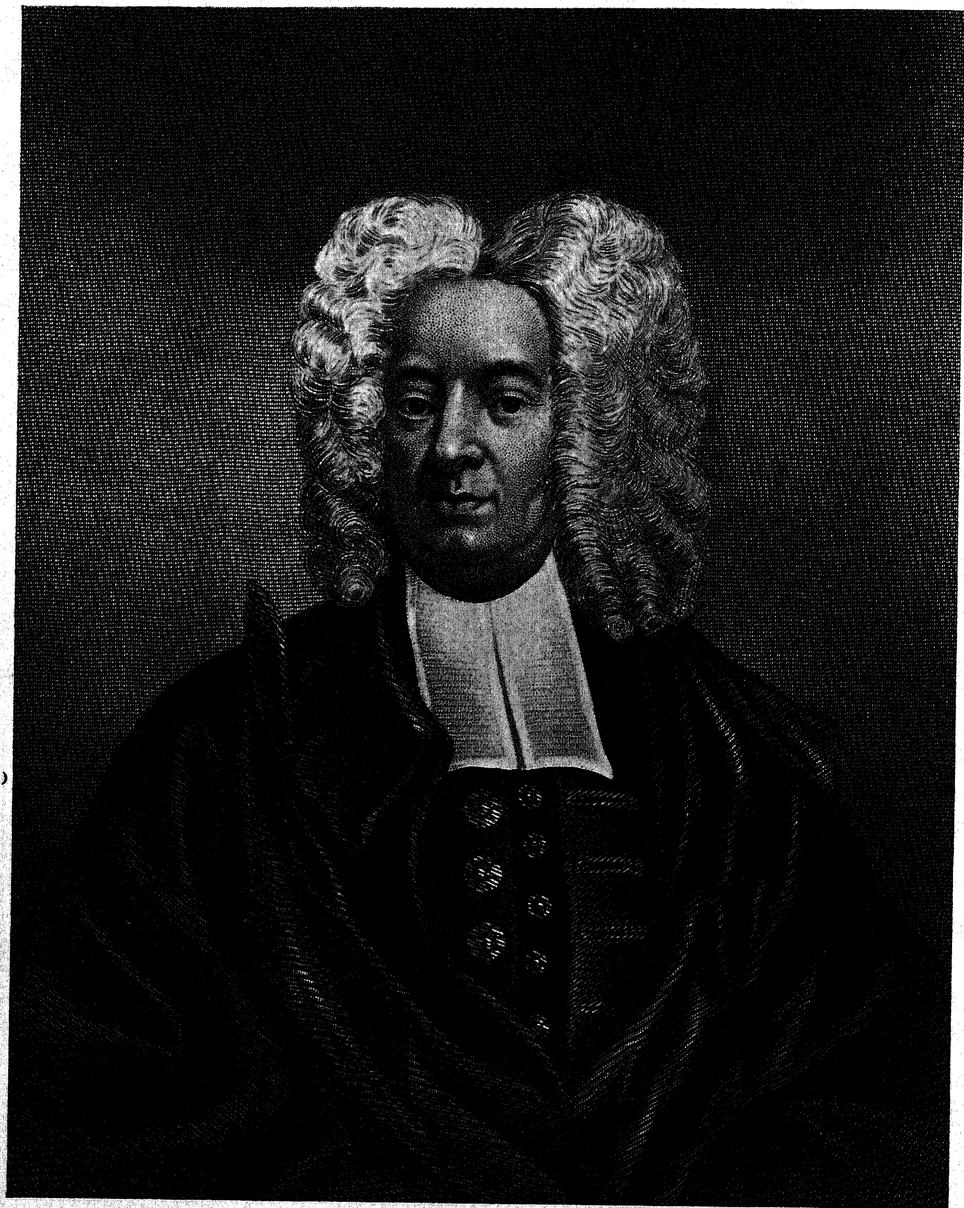

Cotton Mather

FIRST OBSERVED HYBRIDIZATION IN PLANTS

His witch-hunting activities have led posterity to overlook the fact that Cotton Mather "contributed my two mites to the way wherein vegetation is carried on," which contribution is the earliest recorded observation of natural hybridization in plants!

SOME FORGOTTEN RECORDS OF HYBRIDIZATION AND SEX IN PLANTS

1716-1739

CONWAY ZIRKLE
Department of Botany, University of Pennsylvania

THE first half of the eighteenth century was a time of exceptional botanical activity in the English colonies along the Atlantic Coast of North America. A new country filled with strange and interesting plants had just been opened for exploration and European botanists were anxious for specimens. Medicinal plants were in great demand and any educated colonists who sent seed or herbaria sheets to England or the Continent could be assured of an interesting and profitable correspondence with the plant importers.

The colonists who collected, systematized and recorded the distribution of the new plants seem to have been exceptionally able. They kept in close touch with the development of botany in Europe, visited and wrote letters to each other and exchanged ideas and specimens. They imported microscopes, investigated plant anatomy, especially the anatomy of the flower, and devised a number of physiological experiments. The most prominent men in the colonies were keenly interested in this local scientific development and some even joined in the experimentation.

The European botanists were certainly informed of the work of the Americans, although the very real contributions made by the latter, in fields other than taxonomy, have been, with a single exception, completely forgotten.

Most biologists have been so impressed by the overwhelming adequacy of Sachs' *History of Botany* (1530-1860) that any important botanical contribution which he failed to record is apt to remain unknown. Moreover, so greatly is Sachs' judgment admired and his fairness recognized (except perhaps in his treatment of Linnaeus), that he has become the principal arbiter of priority claims even in such a confused subject as that of the discovery of sex in plants. Sachs' evaluation of the contributions of the different workers in this field is still generally accepted.

In order to prove that plants reproduce sexually it is necessary to show, first, that viable seeds can not be produced without the cooperation of some element (pollen) which might be interpreted as male, and, second, that both this hypothetical male element and the egg bear factors which influence the progeny. Camerarius (1694) is cited by Sachs as the first investigator to prove experimentally that pollen is necessary for seed development, while Koelreuter (1761) is credited with having made the first systematic study of plant hybridization, proving incidentally that both parents contribute to the offspring. Sachs rightly emphasized the importance of Koelreuter's work for, as far as he knew, there were no reliable records of plant hybrids before 1761. In fact he mentions only two instances of species crossing before Koelreuter's experiments and both were considered somewhat dubious. He said of the first, "Bradley is our authority for the statement that a gardener in London had obtained a hybrid between *Dianthus caryophyllus* and *Dianthus barbatus* by artificial means as early as 1719" (Sachs, p. 406). The second instance is recorded just as briefly, "Soon after allusion is made (by Linnaeus, 1735) to the artifices used by gardeners to obtain hybrid tulips and cabbages, but the matter is treated rather as agreeable trifling" (Sachs, p. 400). Haartman on

purely taxonomic grounds had interpreted some of his finds as natural hybrids.

Cotton Mather

Forty-five years before Koelreuter crossed different species of *Nicotiana,* however, an American, apparently unknown to Sachs, had observed three important phenomena: (1) wind pollination; (2) hybridization (variety and perhaps species crosses); and (3) the resemblance of the offspring to the male parent.

Cotton Mather is better known today as a witch-hunter than as a member of the Royal Society. While he was undoubtedly very credulous and believed on very scanty evidence much that was later shown to be false, he at least kept in touch with the scientific progress of his time and contributed what he could to the advancement of knowledge. We know that he was informed of the new hypothesis, that the flowering plants reproduced sexually, and he evidently accepted the views advanced by Nehemiah Grew, as is shown by the following passage (Mather, 1721):

> The *Stamina,* with their *Apices;* and the *Stylus* (called the *Attire* by Dr. *Grew*) which is found a sort of *Male Sperm,* to impregnate and fructify the Seed!

The observations of Cotton Mather, which should take their proper place in the history of botany, were described by him in a letter to James Petiver, F. R. S., dated September 24, 1716. This letter, which is now in the files of the British Museum (Sloane Ms. 4065, fol. 255), has probably never been published, but Kittredge (1916) has summarized its contents. In it Mather described hybridization in *Zea* and *Cucurbita.* He repeated the description in his book, *Religio Philosophica;* or *The Christian Philosopher,* London, 1721. The following passage is taken from Essay XXVI, *Of the* VEGETABLES.

> That I may a little contribute my *two Mites* to the illustration of the way wherein *Vegetation* is carried on, I will here communicate a couple of Experiments lately made in my Neighbourhood.
>
> My Neighbour planted a Row of Hills in his Field with our *Indian Corn,* but such a Grain as was colour'd *red* and *blue;* the rest of the Field he planted with Corn of the most usual Colour, which is *yellow.* To the most *Windward-side* this Row infected *four* of the next neighbouring Rows, and part of the fifth, and some of the sixth, to render them colour'd like what grew on itself. But on the *Leeward-side* no less than seven or eight Rows were so colour'd, and some smaller impressions were made on those that were yet further distant.
>
> The same Neighbour having his Garden often robb'd of the *Squashes* growing in it, planted some *Gourds* among them, which are to appearance very like them, and which he distinguish'd by certain adjacent marks, that he might not himself be imposed upon; by this means the Thieves 'tis true found a very *bitter Sauce,* but then all the *Squashes* were so infected and embitter'd, that he was not himself able to eat what the Thieves had left of them.

As the bitterness of the squashes following their mesalliance with the gourds appears a generation too soon, it is regretable that a more careful record was not kept. For until we can trace Fairchild's hybridization of *Dianthus* to some period before 1717, Mather's account is the earliest we have of possible species crossing in plants.*

The spontaneous crossing of different varieties of *Zea Mays* was certain to attract attention sooner or later. Doubtless many more records will be found when the documents of this period have been examined adequately. It is not strange that Cotton Mather's account of cross pollination was overlooked as the biologists of his time were suspicius of the New England "Saints" and did not search their theological works for scientific data. However, it is remarkable that other records of this hybridization have not received more attention.

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*The gourds were *Cucurbita Pepo* var. *ovifera,* the squashes were probably either *C. Pepo* var. *condensa* or *C. maxima;* if the latter, Mather reported a real species cross.

Thomas Fairchild

The first artificial hybrid, of which we have a record, was made by Mr. Thomas Fairchild, of Hoxton, when he crossed *Dianthus caryophyllus* with *Dianthus barbatus*. Although Fairchild's contemporaries gave him full credit for his achievement, Sachs minimized it and the fashion was set which has lasted unchallenged to the present. Sachs[31] did not even mention Fairchild's name, but merely referred to him as a "gardener in London." Focke[15] cited Fairchild's successful experiment (pp. 55, 430) but added, "This success in artificial fertilization was neither used for the advancement of science nor does it seem to have given gardeners any stimulus for further research." Pfeffer[28] went even further by stating (p. 265), "Die wohl früher (1719) ausgefführt Kreuzung zweier Nelken durch *Fairchild* war ein rein gärtnerischer Versuch, der keine wissenschaftigen Bedeutung erlangte." Strangely enough even the recent English and American historians of botany (Green,[16] Roberts[30]) have failed to combat this completely unfair estimate of Fairchild's contribution.

Unfortunately no one has found a really satisfactory description of the *Dianthus* hybridization. Botanists, since Sachs, have seemingly known of this work through a single reference in Bradley's *New Improvements in Gardening, etc.*, which is uniformly dated 1719. This reference is quoted in part by both Green[16] and Roberts.[30] While this record is better than any other yet found, it is incomplete and should be supplemented by other citations. The following quotation is taken from the first edition of Bradley's work which appeared in 1717. Fairchild's experiments were, of course, performed earlier. From Bradley (1717), part I, pp. 23, 24:

> I believe I need not explain how the *Male Dust* of *Plants* may be convey'd by the Air from one to another, by which the *Generation* and *Production* of new Plants is brought about . . . . Moreover a curious Person may by this knowledge produce such rare Kinds of *Plants* as have not yet been heard of, by making choice of two *Plants* for his Purpose, as near alike in their Parts, but chiefly in their *Flowers* or *Seed-Vessels;* for example the *Carnation* and *Sweet William* are in some respects alike; the Farina of one will impregnate the other, and the *Seed* so enliven'd will produce a *Plant* differing from either, as may now be seen in the Garden of *Mr. Thomas Fairchild*, of Hoxton, a Plant neither *Sweet William* nor *Carnation*, but resembling both equally; which was raised from the *Seed* of a *Carnation* that had been impregnated by the *Farina* of the *Sweet William*. These Couplings are not unlike that of the *Mare* with the *Ass* which produces the *Mule*, and in regard to *Generation*, are also the same with *Mules*, not being able to multiply their *Species*, no more than other Monsters generated in the same manner.

Bradley suggests a practical application of Fairchild's discovery (Part II, pp. 84, 85):

> . . . I have endeavour'd to explain how the *Dust* of one Flower will impregnate and enliven the *Seeds* of another, and that from that accidental Coupling the Seeds are so chang'd as to produce Plants with Blossoms varying from those of the Mother plant. I have likewise shewn why double Flowers seldom bear *Seed*, which I conjecture is because the *Male Parts* in them are either not perfect, or else are confin'd from Action by the Multiplicity of the *Petals*. This Consideration leads me to advise the Curious Florists to plant of every good sort of his double Carnations in Beds on a Line in the Middle, and on each Side of them to set at least Two Rows of single ones of choice Colours, and among them some Plants of *Sweet William*, and of the *China* or *Indian Pinks*, which have such Varieties of odd Colours in them, as I shall mention hereafter. The *China Pinks* and the *Sweet Williams*, bearing single Flowers, as well as the single Carnations, may have Opportunities of communicating their *Farina* into the Cells of the double ones, and set their Seeds, which if they do, we shall not only gather a larger Quantity than we could otherwise expect, but likewise be assur'd of great Varieties from them.

The following references supplement Bradley. Miller (1731) under the heading CARYOPHYLLUS, has:

7. CARYOPHYLLUS; *barbatus, hortensis, angustifolius, flore pleno roseo.* The Double Rose-colour'd Sweet John *or* Fairchild's Mule . . . . these continue flowering for a long time, and are extremely beautiful, especially the *Mule,* which produces two full Blooms of Flowers, one in *May,* and the other in *July* . . . .

This passage remained in *The Gardener's Dictionary* until 1754. Perhaps the best description of the hybrid is to be found in a letter of Peter Collinson to John Bartram dated July 22, 1740 (Darlington, 1849).

An instance we have in our gardens, raised by the late Thomas Fairchild, who had a plant from seed, that was compounded of the Carnation and Sweet William. It has the leaves of the first, and its flowers double like the Carnation—the size of a Pink—but in clusters like the Sweet William. It is named a *Mule*—per analogy to the mule produced from the Horse and Ass.

If the following record of the Mule lacks exactitude its source justifies its inclusion. Erasmus Darwin was not only a poet, a physician and a grandfather, but also a well informed botanist. He knew of Fairchild's hybrid and described it in 1781 thus:

Botanic Garden, Part II, Canto IIII

CARYO's sweet smile DIANTHUS proud admires,
And gazing burns with unallow'd desires;
With sighs and sorrows her compassion moves,
And wins the damsel to illicit loves.
The Monster-offspring heirs the father's pride,
Mask'd in the damask beauties of the bride.

When Miller adopted the Linnaean system in 1759 he included the following under the heading "Dianthus":

3. The Mule, or Fairchild's Sweet William, it hath narrower Leaves than either of the former, and is of that Variety called Sweet John: This was said to have been produced from Seeds of a Carnation, which had been impregnated by the Farina of the Sweet William: The Flowers of this are of a brighter red Colour than either of the former, their Bunches are not quite so large, but the Flowers have an agreeable Odour.

When Thomas Martyn revised the dictionary in 1807, the above passage was slightly condensed.

The references thus far quoted do not give us a sufficient basis for judging the character of Fairchild's work. Fairchild himself published very little: *The City Gardener* in 1722 and a short account of some remarkable experiments in the *Philosophical Transactions* of 1724. By far the greater part of his investigations were recorded by his contemporaries: Blair (1720), Bradley (1717, 1721, 1726), Miller (1731) and Collinson (1740). Space forbids an account of Fairchild's many activities. That he was the outstanding commercial florist of his time is shown by many easily obtainable references (Blair, 1720; Bradley, 1717, 1726; Pulteney, 1790; Nichols, 1817; Felton, 1830; Britten and Boulger, 1893). Bradley (1726) referred to Fairchild over twenty times and devoted fifteen pages (Vol. II, pp. 458-473) merely to listing the flowers in the garden at Hoxton with the times of blooming, including in the list the Mule between the Sweet William and Carnation.

It was, however, as an experimental biologist that Fairchild made his chief contributions. At a time when the prestige of the Ancients was still high, when analogy was supposed to furnish proof, and when philosophers still knew enough of the Creator's purposes to arrive at the truth by contemplating the Nature of Things, Fairchild experimented—both for himself and for the more dignified philosophers. His relation to his co-workers is well shown by the honest acknowledgment of Blair in the preface to *Botanical Essays* (1720: . . ."Mr. Fairchild's (*whom I have often mentioned, and to whom I owe all the practical observations I have advanc'd concerning the Vegetation*)." Fairchild also performed many of Bradley's experiments in addition to the most curi-

ous and ingenious ones of his own devising (*Phil. Trans.*, Number 384).

To label the hybridization of Dianthus by the leading experimenter of his generation as "ein rein gärtnerischer Versuch, der keine wissenschaftigen Bedeutung erlangte," in order to give Koelreuter a priority he neither needs nor deserves, would be mildly humorous if such mistatements were not repeated indefinitely.

Philip Miller

Philip Miller was the most widely known botanist of his generation. In his own lifetime his *Gardener's Dictionary* passed through eight full and six abridged editions, together with three editions in foreign languages. Yet in spite of this, his contributions toward establishing the theory of sexual reproduction in plants have been noted only in part and his records of plant hybridization have been ignored.

Miller is credited with being the first investigator to describe insect pollination. Sachs[31,32] erroneously gives the date of this occurrence as 1751, although it had been recorded many times much earlier. From Sachs[31] "Von besonderem Interesse sind die späteren Versuche Müller's (Miller's, in the English translation) von 1751, welche Koelreuter aus dem Gärtnerslexicon (II Theil p. 543) mittheilt, insofern hier zum ersten Mal die Insectenhülfe bei der Bestäubung beobachtet wurde." Sachs, as usual, used Koelreuter's citations and Koelreuter found the reference in the German edition of the *Gardener's Dictionary*, Nurnberg, 1750-51. Green records Miller's observation by quoting directly from the so-called first edition published in 1731, while Roberts quotes from the seventh edition (1759).

Miller is also credited with noting that when female spinach plants were raised apart from the male, they produced seed which contained no embryos. However, his other investigations of sexual reproduction in plants (Cucumbers and Melons) have been left out of the more recent records and his description of spontaneous variety crosses in Cabbage is nowhere evident. This is all the more remarkable because he described his work on *Cucumis* and *Brassica* in many editions of his dictionary.

Miller actually performed his experiments and reported his observations much earlier than our current records would indicate. The date assigned by Sachs, 1751, is of course much too late. Green does not state that Miller's investigations were first published in 1731 but merely quotes from the first folio edition of the dictionary. Miall[24] quotes from the first octavo edition issued in 1724. Miller's first description of his work, however, did not appear in one of his own books. He sent a full account of his observations on insect pollination and his experiments with spinach to Richard Bradley in a letter dated October 6, 1721. Bradley later published this letter in his *Treatise of Husbandry and Gardening*, etc., London, 1726, although an account of Miller's discoveries had already appeared in print.

Miller also informed Mr. Patrick Blair of his discoveries in two letters written on October 19th and November 11th, 1721. Blair recognized the importance of Miller's results and sent an account of them by letter to Sir Hans Sloane. This letter was published in the *Philosophical Transactions*, Number 369.

Observations upon the Generation of Plants, in a letter to Sir Hans Sloane, Bart. Pr. Coll. Med. Patrick Blair, *M.D.*, *F.R.S.*

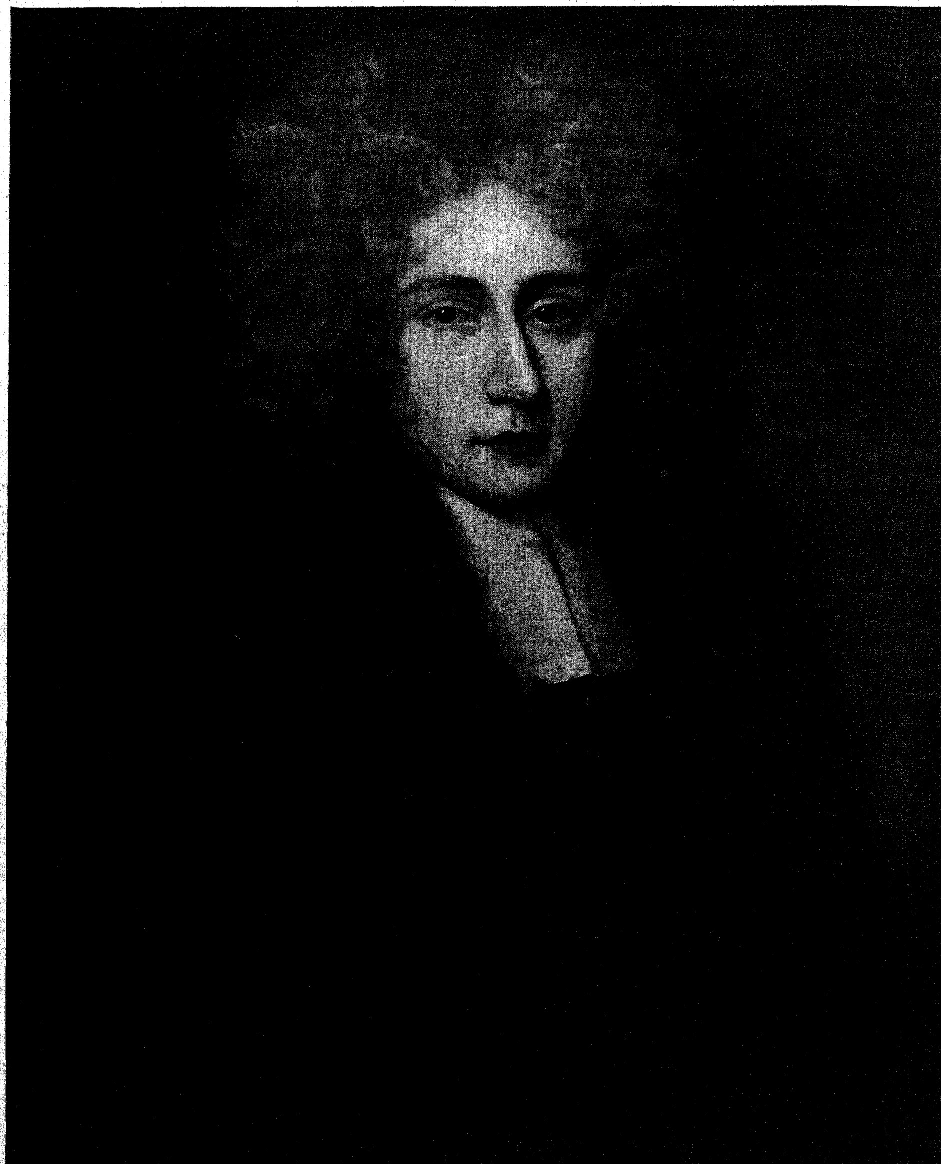
Boston, Dec. 31, 1721.

Honoured Sir,

It is no small Satisfaction, that what I advanced in my *Botanick Esseys* is now so fully confirm'd by Experiments made by some curious Gardeners, among whom is Mr. *Philip Miller*, who writes me,

November 11, 1721.

1. That in Pursuance of my Advice he separated the Male Plants of the Spinage from the Female; the Consequence was, that the Seeds did swell to the usual Bigness; but when he sow'd it, it did not grow afterwards. He searched into the Seed,



PAUL DUDLEY—JURIST AND NATURALIST

The naturalists of Colonial America were many of them leaders in other fields. Paul Dudley, Attorney General of Massachusetts in 1702, and a fellow of the Royal Society, was one of the first to discuss the effects of cross pollination in maize.

and found it wanted the *Punctum Vitae*, which perhaps might have been the Case with Mr. *Geoffrey;* but if not, the female *Embryones* might have been impregnated another Way, as he experimented with twelve Tulips, which he set by themselves about six or seven Yards from any other, and as soon as they blew, he took out the *Stamina* so very carefully, that he scattered none of the Dust, and about two Days afterwards, he saw Bees working on the Tulips, in a Bed where he did not take out the *Stamina*, and when they came out, they were loaded with the Dust on their Bodies and Legs: He saw them fly into the Tulips, where he had taken out the *Stamina*, and when they came out, he went and found they had left behind them sufficient to impregnate their Flowers, for they were good ripe Seed; which persuades him, that the *Farina* may be carried from Place to Place by Insects, and when they happen upon a Flower, whose *Uterus* is capable to be impregnated by such a Dust, it may be thus effected.

I am of Opinion, this will not suit with Mr. *Morland's* Scheme. For tho' we may suppose the *Stamina* of every Flower to be loaded with a due Proportion of the *Farina*, yet this accidental Conveyance of it to a neighbouring Flower, may be rather less than greater than is necessary: So that, if wanting, then those *Embryones*, which had not received its determined Particle into their Bosom, must be defective in Bulk, or barren in growing, but here all were equally fill'd.

2. By a Second Letter, *October* 19, 1721, he informs me, that he bought a Parcel of *Savoy* Seeds of a Neighbour, which he sowed, and planted out the Plants; but was surprised to see the Production: For he had half of them red Cabbages, and some white Cabbages, and some Savoys with red Ribs, and some neither one Sort nor other, but a Mixture of all Sorts together in one Plant. He went to the Gardiner and told him his Tale, who shew'd him, that he was in the same Condition, but did not know how it should come to pass, for he was sure he took special Care in saving of the Seed. Being ask'd how and where he planted them for Seed, he shew'd him them under a *South-West* Hedge, and told him the Manner in which he planted them: First, a Dozen of white Cabbages, then a Dozen of Savoys, and then a Dozen of Red. Then he immediately thought how it came to pass, by the *Effluvia* impregnating the *Uterus* of one another; and it is very common for our Gardiners to plant white and red Cabbages together for Seed, and they are as often disappointed by having a Degeneracy of both Kinds, which they attribute to the Soil, and think that is the Cause: They send to *Holland* for a fresh Supply of Seeds, and say our Soil will not continue that Sort Good. He told them his Opinion, and they laugh at him for it, and will not be turn'd out of their Road, although they should have never so many Experiments shew'd them . . . . . But it is Time to proceed to another Experiment of my Correspondent, Mr. *Miller*.

Being persuaded to it by an ingenious Gardiner, he pull'd off all the Male-Flowers of some Melon Plants so soon as they appeared; but instead of finding, as his Friend informed him, that these Flowers exhausted the Nourishment from the Fruit; he found that, without these Flowers none of the Melons would grow, for that he was deprived of the Fruit which he expected.

Paul Dudley

One early record of hybridization in *Zea Mays* did receive publicity and was incorporated twice by Philip Miller in the *Gardener's Dictionary*, London, 1731, under the headings "GENERATION" and "MAYS: Indian Wheat." The source of Miller's information concerning the spontaneous crossing of different varieties of this New World form was a paper by Paul Dudley, written in New England and sent to the Royal Society. The communication was entitled "Observations on some of the Plants in New England, with remarkable Instances of the Nature and Power of Vegetation," and was published in the *Philosophical Transactions*, number 385.

Paul Dudley was a member of a distinguished Massachusetts family. In 1702 he became Attorney General of the Colony and in 1718 a Judge of the Superior Court. He was a Naturalist

and a Fellow of the Royal Society. The following extract describing variety crosses in *Zea Mays* is taken from his paper of 1724. It has escaped the attention of the historians of botany.

The mention of *Indian* Corn obliges me to take notice of an extra-ordinary *Phaenomenon* in the Vegetation of that Grain, *viz.* the interchanging, or mixing, of Colours after the Corn is planted. For your better understanding this Matter, I must observe, that our *Indian* Corn is of several Colours, as blue, white, red, and yellow; and if they are planted separately, or by themselves, so that no other Sort be near them, they will keep to their own Colour, *i. e.*, the blue will produce blue, the white, white, *etc.* But if in the same Field, you plant the blue Corn in one Row of Hills (as we term them) and the white, or yellow, in the next Row, they will mix and interchange their Colours; that is, some of the ears of Corn, in the blue Corn Rows, shall be white, or yellow; and some again, in the white or yellow Rows, shall be of a blue Colour. Our Hills of *Indian* Corn are generally about four Foot assunder, and so continued in a straight Line, or Row of Hills, and so on; and yet this mixing and interchanging of Colours has been observed, when the D'stance between the Rows of Hills, has been several Yards; and a worthy Clergyman, of an Island in this Province (*The Reverend Mr.* Mayhew, *of* Martha's *Vineyard*), assures me that the blue Corn has thus communicated, or exchanged, even at the Distance of four or five Rods; and, particularly in one Place, where there was a broad Ditch of Water betwixt them. Some of our People, but especially the *Ab-Origines*, have been of the Opinion, that this Commixtion, and Interchange, was owing to the Roots, and small Fibres reaching to and communicating with one another; but this must certainly be a Mistake, considering the great Distance of the Communication, especially at some Times, and cross a Canal of Water; for the smallest Fibres of the Roots of our *Indian* Corn, cannot extend above four or five Foot. I am therefore humbly of Opinion, that the *Stamina*, or Principles of this wonderful Copulation, or mixing of Colours, are carried thro' the Air by the Wind; and that the Time, or Season of it, is, when the Corn is in the Earing, and while the Milk is in the Grain, for at that Time, the Corn is in a Sort of Estuation, and emits a strong Scent. One Thing, which confirms the Air's being the Medium of this Communication of Colours in the Corn, is an Observation of one of my Neighbours, that a close, high board Fence, between two Fields of Corn that were of a different Colour, entirely prevented any Mixture or Alteration of Colour, from that they were planted with.

The above communication was the basis of all of Miller's accounts of the variety crosses in *Zea Mays* which appeared in the successive editions of *The Gardener's Dictionary.* I have seen it in the 1st Octavo Edition (1724), 1st Folio edition (1731), 2nd edition (1733), 7th edition (1759) and 9th edition (1807), and also in the 4th and 5th Octavo (Abridged) Editions (1754, 1763).

James Logan

Another American to recognize the advantages offered by *Zea Mays* for an investigation of sex in plants was James Logan, a Colonial Governor of Pennsylvania. He designed several scientific experiments, duly ran the necessary controls, and succeeded in showing that Indian corn would not produce grain if pollen were kept away from the stigmas. An account of this work was written in Latin and published in Holland (*Experimenta et Maletemata de Plantarum Generatione,* Leiden, 1739), and later reprinted in London together with an English translation (*Experiments and Considerations on the Generation of Plants,* London, 1747).

Logan's work was known to the European Botanists of his time and Sachs gave him full credit for his discoveries. However, Sachs states in a foot note that he was unacquainted with Logan's Essay and based his account of Logan's contributions on Koelreuter's citations. This essay has not been readily available. Harshberger[18] reprinted it in part, unfortunately with modernized spelling and punctuation, and Roberts[29] has quoted from it extensively.

Logan actually performed his experiments some years before 1739, for on November 20, 1735, he sent a detailed description of them to Peter Collinson, who submitted the account to the Royal Society. His letter to Collinson was published in the *Philosophical Transactions,* number 440. The following extract is the earliest record I have found of Logan's investigations.

Some Experiments concerning the Impregnation *of the* Seeds *of* Plants, *by* James Logan, *Esq; Communicated in a Letter from him to Mr.* Peter Collinson, *F. R. S.*

Philadelphia, Nov. 20, 1735.

SIR,

As the Notion of a Male Seed, or the *Farina Foecundans* in Vegetables is now very common, I shall not trouble you with any Observations concerning it, but such as may have some Tendency to what I have to mention—And, first, I find from Miller's Dictionary, that M. *Geoffroy*, a Name I think of Repute amongst *Naturalists*, from the Experiments he made on *Mayze*, was of Opinion, that Seeds may grow up to their full Size, and appear perfect to the Eye, without being impregnated by the *Farina*, which possibly, for ought I know may in some Cases be true; for there is no End of Varieties in Nature;—but in the Subject he has mention'd I have Reason to believe it's otherwise, and that he applied not all the Care that was requisite in the Management.

When I first met with the Notion of this Male Seed, it was in the Winter Time, when I could do no more than think of it; but in the Spring I resolved to make some Experiments on the *Mayze*, or *Indian Corn.* In each Corner of my Garden, which is forty Foot in Breadth, and near eighty in Length, I planted a Hill of that Corn, and watching the Plants when they grew up to a proper Height, and were pushing out both the Tassels above, and Ears below; from one of those Hills, I cut off the whole Tassels, on the others I carefully open'd the Ends of the Ears, and from some of them I cut or pinch'd off all the silken Filaments; from others I took about half, from others one fourth and three fourths, *etc.*, with some Variety, noting the Heads, and the Quantity taken from each: Other Heads again I tied up at their Ends, just before the Silk was putting out, with fine Muslin, but the Fuzziest or most Nappy I could find, to prevent the Passage of the *Farina;* but that would obstruct neither Sun, Air, or Rain. I fastened it also so very loosely, as not to give the least Check to Vegetation.

The Consequence of all which was this, that of the five or six Ears on the first Hill, from which I had taken all the Tassels, from whence proceeds the *Farina,* there was only one that had so much as a single Grain in it, and that in about four hundred and eighty Cells, had but about twenty or twenty-one Grains, the Heads, or Ears, as they stood on the Plant, look'd as well to the Eye as any other; they were of their proper Length, the Cores of their full Size, but to the Touch, for want of the Grain, they felt light and yielding. On the Core, when divested of the Leaves that cover it, the Beds of Seed were in their Ranges, with only a dry Skin on each.

In the Ears of the other Hills, from which I had taken all the Silk, and in those that I had cover'd with Muslin, there was not so much as one mature grown Grain, nor other than as I have mentioned in the first: But in all the others, in which I had left Part, and taken Part of the Silk, there was in each the exact Proportion of full Grains, according to the Quantity or Number of the Filaments I had left on them. And for the few Grains I found on one Head in the first Hill, I immediately accounted thus: That Head, or Ear, was very large, and stood prominent from the Plant, pointing with its Silk Westward directly toward the next Hill of *Indian* Corn; and the *Farina,* I know, when very ripe, on shaking the Stalk, will fly off in the finest Dust, somewhat like Smoak. I therefore, with good Reason, judged that a Westerly Wind had wafted some few of these Particles from the other Hill, which had light on the Stiles of this Ear, in a Situation perfectly well fitted to receive them, which none of the other Ears, on the same Hill, had. And indeed I admire that there were not more of the same Ear than I found impregnated in the same manner.

As I was very exact in this Experiment, and curious enough in my Observations, and

James Logan

COLONIAL GOVERNOR AND EXPERIMENTALIST

James Logan was among the first to put to the test of experiments the many conjectures regarding sex in plants which were current in the early Eighteenth Century. By isolating maize and protecting the silks from pollen, Logan showed that "farina" from the maize tassel was necessary to produce viable seed.

this, as I have related it, is truly Fact, I think it may reasonably be allowed, that notwithstanding what M. *Geoffroy* may have deliver'd of his Trails on the same Plant. I am positive, by my Experiment on those Heads, that the Silk was taken quite away, and those that were cover'd with Muslin, none of the Grains will grow up to their Size, when prevented of receiving the *Farina* to impregnate them, but appear, when the Ears of Corn are disclosed, with all the Beds of the Seeds, or Grains, in their Ranges, with only a dry Skin on each, about the same Size as when the little tender Ears appear fill'd with milky Juice before it puts out its Silk. But the few Grains that were grown on the single Ear, were as full and as fair as any I had seen, the Places of all the rest had only dry empty Pellicles, as I have described them; and I much question whether the same does not hold generally in the whole Course of Vegetation, though, agreeable to what I first hinted, it may not be safe to pronounce absolutely upon it, without a great Variety of Experiments on different Subjects. But I believe there are few Plants that will afford so fine an Opportunity of observing on them as the *Mayze*, or our *Indian* Corn; because its Stiles may be taken off or left on the Ear, in any Proportion, and the Grains be afterwards number'd in the Manner I have mentioned.

John Bartram

I am indebted to Professor Rodney H. True for calling my attention to the following letters collected by William Darlington and included in his book, *Memorials of John Bartram and Humphry Marshall, with Notices of their Botanical Contemporaries,* published in Philadelphia in 1849. Darlington, unfortunately, did not always print the letters in full and often omitted just the passages which today we consider the most important. Most of these letters are now in the possession of the Pennsylvania Historical Society and I have been able to supplement Darlington's selections from his original source. I have not, however, been able to find what appears to be the crucial letter written by John Bartram to Peter Collinson dated April 29, 1740. An undated Ms. in Bartram's handwriting probably includes the essential experiments described in this letter. It may even be a fragment of the letter itself, or more probably a rough draft from which the letter was copied. The writers and recipients of the letters were:

1. James Logan, "President of the Council, and Chief Justice of the Province of Pensilvania."
2. John Bartram, the celebrated Philadelphia Botanist; first to establish a Botanical Garden in America for the cultivation of both native and exotic vegetation.
3. Peter Collinson, F. R. S. and F. S. A., London merchant and Quaker, an importer of plants.
4. Colonel William Byrd of Virginia, explorer, author and founder of the City of Richmond.

1736. James Logan to John Bartram
(Darlington, p. 307)

Friend J. Bartram:

Last night, in the twilight, I received the inclosed, and opened it by mistake. Last year PETER sent me some tables, which I never examined till since I last saw thee. They are six very large sheets, in which the author (Linnaeus) digests all the productions of Nature in classes. Two of them he bestows on the inanimate, as Stones, Minerals, Earths; two more on Vegetables, and the other two on Animals. His method in the Vegetables is altogether new, for he takes all his distinctions from the *stamina* and *styles*, the first of which he calls husbands, and the other wives. He ranges them, therefore, under those of 1 husband, 2, 3, 4, 5, 6, 7, 8, 9, 10, 12, 20, and then of many husbands. He further distinguishes by the styles, and has many heads, under which he reduces all known plants.

The performance is very curious, and at this time worth thy notice. I would send it to thee, but being in Latin, it will want some explanation, which, after I have given thee, thou wilt, I believe, be fully able to deal with it thyself, since thou generally knows the plants' names. If thou wilt step to town tomorrow, thou wilt find me there with them, at E. SHIPPEN'S, or J. PEMBERTON'S, from 12 to 3. I want also to

say something further to thee, on microscopical observations.

Thy real friend,
J. LOGAN.

Stenton, 19th of June, 1736.

1737. Peter Collinson to John Bartram
(Excerpt, Darlington, p. 107)
London, December 20, 1737.

Dear Friend:

I shall now consider the remaining part of thine of July 19.

The magic lantern is a contrivance to make sport with ignorant people. There is nothing extraordinary in it, so not worth thy further inquiry.

Thee art still desirous of a magnifier for flowers. Pray make this complaint to J. LOGAN, and try his thoughts. As thy inquiries seem in some measure to be owing to him, and as thee art his pupil (which no man need be ashamed of), no doubt but he will furnish thee with suitable instruments for that purpose, in order to render thy discoveries more perfect—so undoubtedly more to his satisfaction. . . .

The references in the following to Dr. Tschiffely and to Byrd's letter of March 23rd enable us to place Bartram's communication in 1739. The greater part of Bartram's letter has been published (Darlington, 1849). The most important portions, however, were omitted by the modest Darlington. These I have italicised as they contain one of the passages which enable us to identify the genus investigated by Bartram as *Lychnis.*

1739. John Bartram to Col. Wm. Byrd
(Bartram Papers, Vol. I, fol. 19)

Dear Friend Coll Byrd:

I r'cived thy kind letter by ye post last winter; and another dated March ye 23d, which I received by ye hand of thy friend doctor Tschiffely, whom I received very kindly and made as welcome as my present Circumstance would afford for thy sake having no other acquaintance than thine and another recommendation. I have this spring made several microscopical observations upon ye malle and femall parts in vegetables to oblige some ingenious botanists in Leyden, who requested that favour of mee which I hope I have performed to their satisfaction and as a mechanical demonstration of ye certaity of this hypothesis of ye different sex in all plants that hath come under my notice. *I cant find that any vegetable hath power to produce perfect seed able to propagate without ye conjunction of malle seed any more than animals and by a good microscope ye malle and femall organs is as plainly discovered.* I have made several Successfull experiments of joyning several species of ye same genus whereby I have obtained curious mixed Colours in flowers never known before but this requires an accurate observation and judgment to know ye precise time *when ye femall organs is disposed to receive ye masculin seed and likewise when it is by ye masculin organs fully perfected for ejection.* I hope by these practicall observations to open a gate into a very lar(g)e field of experimental knowledge which if judiciously improved may be a considerable addition to ye beauty of ye florists garden.

The next letter from Peter Collinson to John Bartram has also been published by Darlington (1849) and again I have italicised the portion he omitted.

1740. Peter Collinson to John Bartram
(Bartram Papers, Vol. II, fol. 53)
London, July 23d, 1740.

Dear Friend:

I had the pleasure of thine, of April 29th, 1740. Thy experiment of the usefulness of the *Farina*, is very curious and entertaining, where plants of a class are growing near together, they will mix and produce a mingled species. An instance we have in our gardens, raised by the late THOMAS FAIRCHILD, who had a plant from seed, that was compounded of the Carnation and Sweet William. It has the leaves of the first, and its flowers double like the Carnation—the size of a Pink—but in clusters like the Sweet William. It is named a *Mule*—per analogy to the mule produced from the Horse and Ass.

Writing on these matters, brings to mind the Papaw—an Indian fruit—which in our stoves is produced in great plenty. On this tree, is very remarkably distinct, male, female, and hermaphrodite blossoms, which are very extraordinary to see: but whether the

last is an assistant in generation, or is a sport in nature, is not yet agreed . . . .

Dr. WITT'S hollow-leafed Lavender, is, no doubt, the Side-saddle flower; but what relation it has to *Lavender*, I must leave to him. The plant with tricolor leaves, I am well assured, is your fine *Clinopodium*. Our late severe winter has carried all mine off; so pray send me some more seed—and of the *Lychnis* with Crosswort laves . . . . *The Flesh Colour** Lychnis *does not appear but in its Roome One with a pale Blew Flower phaps this may be that from Susquehannah a new one and very sweet sented which I esteeme much—and I want when thee increases it, that with a white flower with a Red Spott in the Center, and that* Lychnis *with a small white flower* . . . .

What curious experiments did Bartram recount in his letter of April 29, 1740, which caused Collinson to comment, "When plants of a class are growing near together, they will mix and produce a mingled species," and to follow the comment by a description of the then twenty-three year old hybridization experiment of Thomas Fairchild? The inference is obvious: i. e., that Bartram described his own work on species crossing, the same work to which he referred in his letter to Colonel William Byrd.

The following is transcribed from an undated Ms. in Bartram's handwriting which is now in the possession of the Pennsylvania Historical Society (Bartram Papers, Vol. I, fol. 20). The Ms., consisting of a frayed sheet of paper with a large fragment torn from a corner, shows signs of being very hastily written, i. e., words inserted between the lines, words duplicated, letters small and less clearly formed than usual. The paper had been torn before Bartram wrote on it, so it seems improbable that he ever intended to include it in a letter. The Ms. has the appearance rather of a preliminary draft, possibly of the missing letter to Collinson.

. . . . have had for 3 year a lychnis which produced flesh colored flowers male female upon several plants, one large plant which produced malle over most part of summer but produced no seed which ingaged my perticular notice and I observed that that seed was produced which ye sent me distinct male & female plants a year after I had sent from eng(land) some of ye seed of white lichnic which produced male & female flowers distinct upon different plants. It hapened that one female plant flowered neare two weeks before any male flowered which I obsrved daly and observed that tho there was no stamina or anthera in ye flower yet ye capsula was filed with perfect seed which I sowed as soon as it was ripe which came up very well in a few days but by that time that ye capsula of this first flowering white lychnis was near full grown then several malle plants flowered in ye same bed. I then puled of all ye capsulas that was set before any of these male flowers of ye white lic(hnis) appeared & that which was produced after ye male blosoms opened and shed their farina: for I concluded that either that this female white lychnis must be impregnated with ye male red lychnis which grewde 10 yards of it if so then it must pertake of ye nature & color of ye red one or else we should be pusled to reconcile ye ye hypothesis of nescesity of ye male & female parts as after it had happened according to my expectation for it produced flowers great deal paler then ye red & as much higher coloured then ye white. But ye seed of ye white lychnis that was produced after ye male plant flowered produced plants which bore flowers as white as thair original. Moreover I gave docr witt one plant of ye female lychnic which flowered plentifuly with him & produced ye capisula but containing onely something like ye husks of ye seed but no vegitive life on them, by which it apears that ye male parts of vegitables is realy as nexcesary to vegitation.

Chronological List of Important Contributions to Our Knowledge of Hybridization and Sex in Plants, 1676-1761

The following table lists the work of those botanists who discovered by observation and experiment that plants re-

*Not skin-color. In 1740 "flesh-colour" meant light red, like the "meat" of the watermelon.

produced sexually and that it was possible to cross different varieties and species and secure new forms. It is easier to evaluate the contributions of Cotton Mather, Paul Dudley and John Bartram and the re-dated work of Philip Miller and James Logan by placing them in chronological sequence with the investigations of the other contributors.

1676—Grew in an address before the Royal Society, recorded his own belief and that of Sir Thomas Millington, that the stamens are the male organs of the plant and that the pollen acts as vegetable sperm.

1694—Camerarius in a letter to Valentin describes his experiments which proved that pollen is necessary for seed development in *Mercurialis*, *Morus*, *Rianus* and *Zea Mays*.

1703—Moreland tried to discover how pollen influenced the ovules.

1714—Geoffroy reported that the seeds of *Zea mays* and *Mercurialis* are infertile when no pollen reaches the stigma.

1716—Cotton Mather reported certain observations on *Zea Mays* and recorded (1) wind pollination, (2) hybridization (variety cross) and (3) the resemblance of some of the progeny to the male parent; he also reported a cross (?) between *Cucurbita Pepo* and *C. maxima* (?)

1717—Bradley removed the anthers from Tulips, which consequently produced no seed. He also noted spontaneous hybridization in several varieties of *Pyrus Malus* and *Auricula* (*Primula Auricula*).

1717—Fairchild crossed *Dianthus caryophyllus* with *D. barbatus* and found that the progeny resembled both parents. This was the first recorded artificial hybrid.

1721—Miller described (1) several instances of spontaneous hybridization in *Brassica* and the resulting mixed and variable progeny; (2) insect pollination in *Tulips*; and (3) sexual reproduction in *Spinacio* and *Cucumis*.

1723—In a garden in Stenbrohuld, according to Linnaeus, all male flowers were removed from a plant of *Cucurbita Pepo* which, consequently, bore no fruit.

1724—Dudley described wind pollination and variety crosses in *Zea Mays*.

1735—Linnaeus commented on the artifices resorted to by gardeners to obtain hybrids in *Tulipa* and *Brassica*.

1735—Logan showed by controlled experiments that pollen is necessary for the formation of grain in *Zea Mays*.

1739—Bartram, in a letter to Col. Wm. Byrd, states that he has crossed species (*Lychnis*), obtained strange hybrid forms and hoped to develop new horticultural varieties.

1749—Gleditsch fertilized a female palm tree (*Chamaerops humilis*?) in Berlin with pollen brought from a male tree in Leipzig, whereupon the palm produced fertile seed for the first time.

1750—Haartman records forms which he assumes on taxonomic grounds to be natural hybrids in the genera *Veronica*, *Delphinium*, *Saponaria*, *Actaea*, etc.

1761—Koelreuter systematically investigated hybridization and produced artificial hybrids in *Nicotiana*, *Kedmia*, *Dianthus*, *Matthiola*, *Hyoscyamus*, etc.

Summary

A number of early records of plant hybridization made by English and American botanists have been unknown to the historians of the science, while others have been referred to in a manner which minimized their importance. Certain other observations which helped to prove that plants reproduced sexually have been recorded, but mis-dated.

1. The first record we have of spontaneous hybridization is a letter written by Cotton Mather on September 24, 1716, to James Petiver, F. R. S. The account was printed by Mather in *Religio Philosophica: or The Christian Philosopher,* London, 1721. He described variety crossing in *Zea Mays* and the pollination of squash by gourds.

2. The first recorded artificial hybrid resulted from a cross, *Dianthus caryophyllus* × *D. barbatus*. The investiga-

tor who made this cross should be known as Mr. Thomas Fairchild, of Hoxton, not as "a gardener of London." Fairchild's hybrid is described by Bradley (1717), Miller (1731), Collinson (1740), and Darwin (1781).

3. Philip Miller was the first to describe insect pollination (in tulips). The date of his observation was neither 1751 (Sachs) nor 1731 (Green), but was at least as early as 1721. In that year he wrote to Bradley (Oct. 6) and Blair (Oct. 19 and Nov. 11) and described not only insect pollination in tulips and sexual reproduction in spinach, cucumbers and melons, but also spontaneous hybridization in cabbage when different varieties were grown near one another.

4. Paul Dudley in 1724 described wind pollination and variety crosses in *Zea Mays*.

5. James Logan in a letter to Peter Collinson dated November 20, 1735, described his famous experiments on pollination in *Zea Mays*. He later published the results of his investigations in Leiden (1739) and London (1747).

6. John Bartram stated in a letter written in 1739 to Col. Wm. Byrd that he had crossed species within a genus and obtained a mixed and variable hybrid progeny. An undated manuscript in Bartram's handwriting contains an account of hybridization in *Lychnis*.

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Books Received

BOOKS are acknowledged in this column as received, and such acknowledgment must be regarded as sufficient return for the courtesy of the sender. As far as space permits, books that contain material of special interest to the reader of the JOURNAL *will be reviewed in later numbers.*

THE FRENCH RACE: Theories of its Origins and their Social and Political Implications Prior to the Revolution. *By* JACQUES BARZUN, Ph. D., Sometime Mitchell Fellow in History, Columbia University, Perkins Scholar in Political Science, Instructor in History at Columbia University. 275 Pages. 14 Chapters. Price, $4.25. Columbia University Press, New York. 1932.

Theories are all one can have of the origin of most races. The *facts* are too meager and too scattered to get very far. The *theories* which the French hold as to their racial origin influence their actions and thus are as important sociologically as *facts*. It will surprise many "Pros" and "Cons" in the "Nordic Superiority" wrangle to learn that it has been going on at least since the second century A. D.

BEITRAGE ZU EINER GENOTYPISCHEN ANALYSE DER ACKERBOHNE, VICIA FABE L. *By* M. J. SIRKS (Instituit voor Plantenveredeling, Wagening, Holland). With 428 Pages, 43 Illustrations, 100 Tables, Roy. 8vo. Reprint from Genetica Vol. CIII. Price 22 Guilders. XI Chapters. Martinus Nijhoff, The Hague.

All about the genes of the broad bean.

INHERITANCE. *By* PHYLLIS BENTLEY. Pp. 592. Price, $2.50. Macmillan, New York. 1932.

Fictional history of an English family through the Industrial Revolution, the World War, down to these Present Peaceful Times, when the recent mob before Buckingham Palace recalls the mobs that destroyed the first Oldroyd textile machinery in 1812.

INTRODUCTION TO THE HISTORY OF SCIENCE, Volume II, From Rabbi ben Ezra to Roger Bacon. *By* GEORGE SARTON, Associate in the History of Science, Carnegie Institution of Washington. In two Parts. Pp. 1251. Price, $12.00. The Williams and Wilkins Co., Baltimore. 1931.

The conviction that we live in the most interesting and exciting epoch in human history is somewhat shaken by this fascinating, more than encyclopaedic, résumé of science in the Twelfth and Thirteenth Centuries.

THE MECHANISM OF CREATIVE EVOLUTION. *By* C. C. HURST. Pp. xxi and 365. Frontispiece and 199 Illustrations. Price $6. New York, Macmillan Co., 1932.

If "one picture is worth ten thousand words," this is an encyclopedia of many volumes.

FLIGHTLESS--A HERITABLE VARIATION

In the Domestic Fowl*

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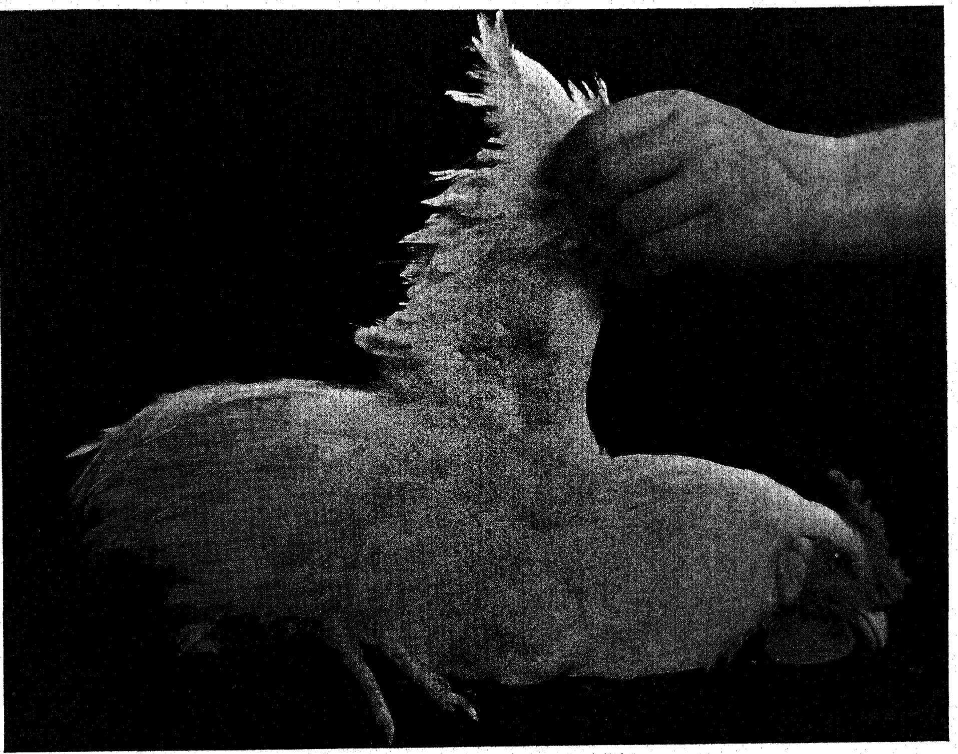

A "FLIGHTLESS" ROOSTER

Figure 3

A young rooster showing the heritable character "flightless." This causes defects of the large feathers of the wings, which break off, reducing the wing area below that necessary for flight.

THE writer's attention was called to the flightless fowl by Mr. A. O. Robertson of Kansas City, Kansas, who wrote that he had been breeding birds of this type for about 20 years.

Description of Character

The adult showing this character is entirely incapable of flight. Since the wings are useless a flightless bird cannot even mount a roost placed at the usual height and when pedigree hatching from females showing this character the trapnests must be placed near the floor. The inability to fly is due to a defective condition of the flight feathers. The large feathers in other portions of the body are also defective. The defect is in the shaft of the feather causing it to break off when subjected to a slight pressure. The shaft of the portion of the feather remaining is very much roughened showing a tendency for the outer surface to scale off.

After a molt the new feathers be-

*Contribution No. 68, Department of Poultry Husbandry.

"FLIGHTLESS" DUE TO DEFECTIVE FEATHERS

Figure 4

Above: Young flightless rooster with wings folded, showing "clipped-wing" appearance. Below: "Flightless" feathers and comparable normal wing feathers, showing breakage of the former.

NORMAL ROOSTER

Figure 5

To show wing development in absence of the dominant flightless gene.

gin to break off as soon as they are mature enough to become dry. The flight feathers continue to break until only the base of the shaft and the quill remain in the wing. Except for the condition of the feathers the wing is entirely normal. The large tail feathers also break off but the smaller, more flexible body feathers are not much affected. When the plumage becomes older there is a tendency for a somewhat roughened appearance to develop, probably due to some breaking of the body feathers.

The character may be recognized in chicks as early as one month of age. The spread wing at this time shows the flight feathers to be irregular in length and many individuals will have most of these feathers badly broken. The shafts of the remaining feathers will be very brittle and have the characteristic roughened appearance.

Experimental Results

The original flightless male was mated to a few normal winged females in the fall of 1931 and produced eleven normal and eight flightless offspring. This at once suggested that the character was dominant and that the original male was heterozygous.

In order to check more accurately upon the mode of inheritance of the character the original male was mated to seven Single Comb White Leghorn females and six hybrids from the cross of Black Minorca male by White Leghorn females. The results of this mating are shown in Table I. The totals were 129 flightless to 136 normals. This is a very close approximation of a 1 to 1 ratio which would be expected if flightless were dominant to normal flight feathers and the original male were heterozygous.

The proportion of flightless in each sex is also very nearly equal indicating that the character has good viability.

Five flightless daughters of the original male by normal females were mated to a Silkie Bantam male having normal flight feathers. They produced 15 flightless to 12 normal offspring substantiating the view that the character flightless is a dominant one.

There has recently appeared in this Journal a discussion by Edward McCrady, Jr., of hereditary local alopecia in the guinea fowl. Although the end results are very similar in the two cases, it appears that these characters have a very different physiological basis. In McCrady's guinea, the wing feathers fail to develop while in the chicken they break off due to a defect in the feathers. Alopecia in the guinea is recessive while "flightless" in the chicken behaves as a dominant.

Summary

The foregoing results indicate that the new character, flightless, is a simple autosomal dominant. Its viability is good and it should be a useful character for future studies of the genetic constitution of the domestic fowl.

TABLE 1. A mating of a heterozygous flightless male to normal winged females.

Mother	Daughters: Normal Feathers	Daughters: Flightless	Sons: Normal Feathers	Sons: Flightless
6026A	5	3	8	7
6027A	9	7	5	5
6039A	5	4	4	9
6044A	1	3	1	2
6046A	8	6	12	8
6048A	4	3	6	9
6054A	2	2	1	0
6056A	9	11	15	14
6057A	4	5	10	6
6059A	1	0	0	1
6060A	5	4	5	4
6066A	7	9	7	6
6067A	2	0	0	1
Total	62	57	74	72

129 flightless to 136 normal feathers.

Variation in Carotene Content of Carrot Varieties

Carotene, the raw material from which the liver synthesizes, vitamin *A*, is the substance which gives carrots and other vegetables a characteristic yellow color. McDonald reports in *Science* (July 29, 1932) the result of a carotene analysis of ten varieties of carrots. The yellow varieties of course contain the most, but even the white field carrots contain an appreciable amount. A garden variety, Early Scarlet Horn, with 2.2 grams of carotene per bushel of carrots, was the richest in this substance. The field variety, Isabell's Maude S. contained only 0.027 grams of carotene per bushel—one eighteenth as much. All the samples used in this experiment were grown under greenhouse conditions, as nearly comparable as possible.

A PEACH MUTATION

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TO THE list of bud variations in deciduous fruit trees that has been brought to the attention of geneticists and horticulturists by the JOURNAL OF HEREDITY,[1, 2, 3, 4, 5] a variation from the normal that was found in a peach tree appears to be a worthy addition.

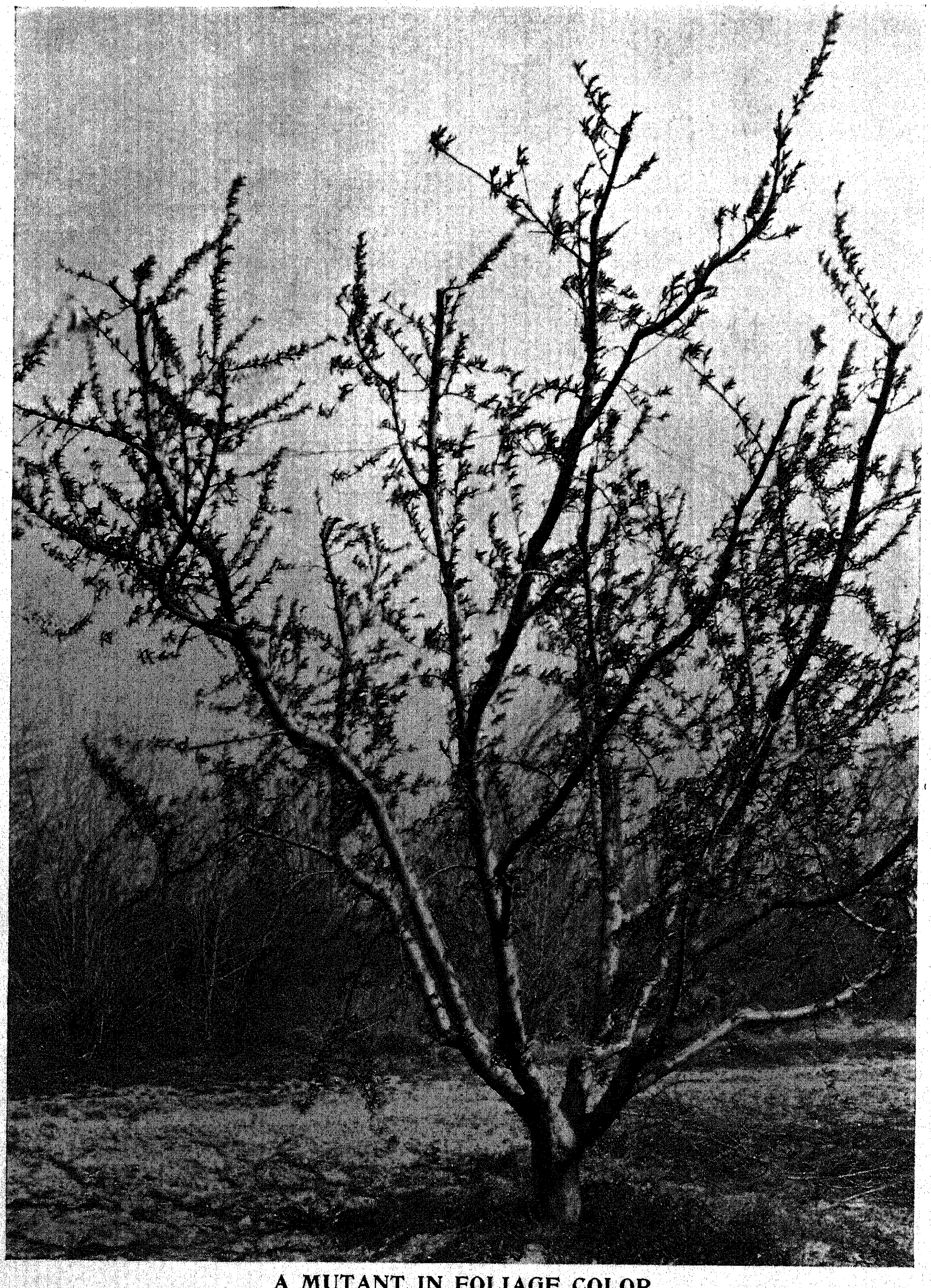
A few years ago the upper portion of a branch of a Johnson peach tree was observed to have leaves of a yellow color, while those on the remainder of the tree were of the normal green color, typical of the variety. The eight-year old tree on which this branch is growing is in the commercial orchard of F. C. Albright, near Nicolaus, Sutter County, Calif. (Figure 6). The branch with yellow leaves is one of the framework branches and comprises about an eighth of the entire tree. It is three inches in diameter at the point where the mutation occurs, which is six feet from the ground and four feet above the crotch of the tree. Above this point all the leaves produced are of the yellow type, while those below show no traces of this color (Figure 7).

The leaves of the mutant portion of the branch are a lemon yellow color in the spring, and all are apparently of the same intensity of color. When they are a few weeks old, they gradually turn green with those nearest the base of the yellow leaf segment of the branch showing the change in color first and changing most rapidly, the greening becoming gradually less apparent as the top of the branch is approached. By mid-summer, when the leaves are about four months old, this branch might be passed unnoticed by the casual observer if his attention were not directed to the top shoots where the leaves still have a distinctly yellowish cast. By careful examination a distinct difference in color may still be recognized between leaves on the entire mutant portion of this branch and those on normal branches. By middle or late August, when the leaves are about five months old, and throughout the remainder of the season, this difference is not pronounced. Mr. L. B. Scott of the United States Department of Agriculture reports the same phenomenon in the leaves of some first season plants from the mutant branch that he is growing at Shafter, California. The leaves of these became gradually more green as described above, up to late summer, but the lemon yellow color recurred to some extent during the fall.

Measurements were made of both yellow and normal green leaves in 1930, and the latter were found to be a trifle the larger. The average area of yellow leaves was 2.42 square inches and that of the green ones 2.64. Shoots on the yellow branch appear to grow equally as well as those comparably located on any other branch of the tree.

All branches of the tree set heavy crops and bear regularly. Fruit produced by the yellow leaf branch ripens at the same time as that on other branches of the same tree, and measurements indicate that the fruit from the two types of branches is the same size. However, fruit from the mutant branch appears to be a little lighter yellow in color. Stones from fruit on this branch are notably lighter red in color than from other branches and would be classed as dull pink, rather than the usual reddish brown color of normal Johnson peach stones.

The visible leaf peculiarities of the sporting branch are retained by scions taken from it and grafted into other peach trees, as shown by two three-year old grafted trees growing near the parent tree and by one-year old trees on the United States Cotton Field Station grounds near Shafter. In no case is there any apparent influence of the



A MUTANT IN FOLIAGE COLOR

Figure 6

An eight-year-old Johnson peach tree which produces leaves of a lemon yellow color on the upper portion of the framework branch at the left. The arrow indicates the point where the change in leaf color occurs.

COLOR-CHANGE AT THIS POINT

Figure 7

Near view of the location where the mutation occurs on the tree shown in Figure 6. The knife indicates the point where the leaf color changes from normal green to yellow.

Figure 8

The union and growth of a yellow-leaf scion from the mutant branch shown in Figure 6 grafted to a normal healthy seedling peach. All leaves of the scion are yellow while all those below the union are of normal green color.

scion on the color of leaves that develop on shoots that grow from the stock portion of these trees. Yellow leafed scions appear to be entirely congenial with the peach in which they are grafted (Figure 8).

Seedlings Yellow-Leaved

The behavior of its progeny adds to the interest of the yellow leaf branch. Thirty-one plants were grown from 100 open pollinated seed that were taken from this branch and planted by Mr. Scott at Shafter. Fifteen of these seedlings had leaves of the same yellow color as the parent branch, while the other 16 have normal leaves. Since these seeds were open pollinated, the

ratios are not significant genetically. The seedlings from the mutant branch and a lot from seed from the normal branches of the tree have grown about equally well. Both yellow and green leafed plants were also obtained from seed from the yellow leaf branch which was planted near the mother tree in Sutter County. All seedlings from the normal branches have green leaves. Blossoms on the yellow leaf branch will be self-pollinated next spring in the hope of securing seed for a new generation.

Aside from the mutant branch and scions taken from it and its seedling progeny, no other tree or portion of a tree with the same leaf color has been found in this orchard, nor have such been found during many surveys of peach nurseries or in orchards in important peach growing sections in California. Since the color difference in leaves of the normal green and the yellow type is as striking in small trees as in larger ones, a large number of trees in the nursery and in young orchards may be satisfactorily surveyed, for the color variaiton is sufficiently striking to be recognized as far as the tree itself may be distinguished. The behavior of material from this branch on propagation indicates that the yellow foliage is not caused by a virus disease or a nutritional condition.

The fact that a portion of the plants grown from seed produced on this mutant branch show the leaf characteristics of the mutant indicates that the reproductive as well as somatic tissues have been affected in this variation, which apparently originated as a bud sport.

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One Anthropologist's Logic

ANTHROPOLOGY AND MODERN LIFE, *by* FRANZ BOAS, PH.D., Professor of Anthropology at Columbia University; former President of the A. A. A. S. Pages 246. Price, $3.00. W. W. Norton, New York, 1932.

PROFESSOR BOAS remarks that the difference between the biologist and the anthropologist is that the former permits reason alone to reign in his domain, the latter will acknowledge no such complete domination. It does not require even a glance at the title to convince the reader that the book was not written by a biologist. Personally we have no complaint with admitting emotion into our domain, but prefer it seasoned with at least a sprinkling of logic.

We read on p. 173, " - - - the children whose permanent teeth erupt early are also taller on the average than those whose permanent teeth erupt late." Also, "Therefore all the bodily measurements of children of the rich are greater than those of the poor of the same age." With the accuracy of these sweeping statements we are not concerned at present. We are merely accepting them and arranging them in logical sequence. We might say that since it is the taller children who get their teeth early, and since it is the children of the rich who are taller, that as a biologist and a reasoning being, we are led to conclude that it is the children of the rich who get their permanent teeth earlier. Not so the an-

thropologist. With a rare combination of emotion and reason the senses at once (in fact we only turn the page before he senses it), that it is the children of the poor who get their permanent teeth early!

Nor can the biologist, weighted down with reason, scale those educational heights over which the wings of emotion so swiftly carry this anthropologist. We had been under the impression, erroneous apparently, that grading children on the basis of their mental abilities at school was not half bad. If they were too stupid to pass the work they remained behind with children of a younger age until their mental level was reached. But alas! We were mistaken. We are not yet sure just which age is to determine their advancement or their educational rating, but it is to be their *physiological age*. Perhaps they are to be promoted when they get a new tooth, or when they add another inch to their stature, or a pound to their weight. I recall a certain college girl who made a brilliant record, and who did not lose one of her deciduous teeth until she was past 21; also a highly intelligent medical student of 25 who had never developed any hair on his face. Perhaps these students should have been back in high school waiting for teeth and beard to grow. Where would we place those numerous mental deficients whose physiological sexual maturity comes at the normal chronological age? Shall they go to high school because their physiological development entitles them to be there?

So much for education; what are Boas' views on Eugenics? We confess to being an ardent eugenicist; but we also confess to tolerance of the opponent's viewpoint when presented logically; but then Prof. Boas himself admitted that he was an anthropologist, so we are not responsible for any lack of toleration. He does admit that there are certain pathological traits that are inherited, and that "defective classes whose deficiencies are proved by rigid methods to be due to hereditary causes" should be suppressed. But he adds that the "serious demand be made that - - - - in each and every case the hereditary character of the trait must be established before it can be assumed to exist." On the other hand, is it not just as imperative that the trait in question be proved beyond doubt to depend upon environment before such dependence can be assumed? If we should not sterilize our mental defective because we have not adequately proved that his defect is hereditary, should we expend millions of dollars upon expensive philanthropic measures designed to alleviate environmental conditions that we have not rigidly proved were responsible for the defective conditions in question?

He objects to eliminating the undesirable strains because of the loss of the normal persons who would have been born along with the undesirable as a consequence of Mendel's second law. If the theory that environment is so largely responsible for all man's ills be true, we have no right to let normal persons be born into families in which others are defective, for at once they are subjected to a very undesirable environment, and thus liable to become defectives themselves. Those who object to the eugenic program talk about the "social rights" of the defectives. They lose sight of the social rights of the normal person. Is he not within his right to refuse support for his neighbor's physically and mentally defective children if it means inadequate support of or actual suppression of his own normal offspring? Has the defective the right to suppress the children of the normal? Yet this is what happens when our modern system of taxation compels the normal man to pay for the food, shelter, even the clothing, of the physically and mentally defective and of the criminal, before he can claim from his own earnings one cent to support his own normal child. There are "social rights" in this matter it is true, but it is time that some advocate of the rights of the normal man should come forward, and demand his fair share of his own earnings to look after his own children.

Only one more statement will be dealt with. To deal adequately with the fallacies in this book would require a volume fully as large, which would be hardly worth while. This statement (it cannot be called argument, because that implies some element of logical reasoning), is as follows. "Another aspect of the problem is of much more vital importance to mankind. The object of eugenics is to raise a better race and do away with increasing suffering by eliminating those who are by heredity destined to suffer and to cause suffering. I believe that the human mind and body are so constituted that the attainment of these ends would lead to the destruction of society. The wish for the elimination of unnecessary suffering is divided by a narrow margin from the wish for the elimination of all suffering." Boas goes on to say that for the development of the race there must be suffering, lest the cessation of all discomfort lead to the degeneration of the race. Would he recommend that we abolish all anesthesia, so that mankind could suffer adequately again for the ennoblement of their souls during surgical operations? If his argument be true why does he insist a few pages earlier upon eliminating all "the social conditions that have raised a poverty and disease stricken proletariat, social conditions which remorselessly push human beings into helpless and hopeless misery?" If hereditary suffering be for the good of the race, that it become not too effeminate, why do away with environmental suffering? Are these poor wretches not contributing to the furtherance of civilization by keeping alive the torch of suffering which sears their bodies but elevates their souls? Why abolish the grinding cruel labor that saps the strength of men and women; why destroy the very agony which is the stepping stone up which man stumbles toward his ideals?

But enough of Prof. Boas. Eugenics he fears may prove a two edged sword; philanthropy has already proved itself one. It is a whole barbed wire entanglement in which mankind is enmeshed, perpetuating the very conditions which it aimed to abolish, through its constant aiding of those who need it to reproduce still more defectives who in turn demand still more philanthropy.

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A Friend of the Negro

EMBREE, E. R. Brown America, the Story of a New Race. New Yark, The Viking Press, 1931, pp vi+311.

THE title of Mr. Embree's book is not intended to convey the idea that the American people are destined to become brown in color, or even yellow. Whether or not Mr. Embree believes that the Negroes will eventually be absorbed by the whites is not made entirely clear. Miscegenation, as most students of the subject agree, is less frequent than formerly. But the author remarks that "No races in history have lived side by side over long periods of time without almost completely mixing their bloods; and in the long future, this course may prove to be inevitable in the United States." With this remark the subject is dismissed. Apparently the author is not worried over the outcome, whatever it may be.

The title "Brown America" is chosen to emphasize the fact that the American Negroes are forming a new race, brown in color, and combining the characteristics of whites, Negroes and Indians. The percentage of Negroes having some admixture of white blood is impossible to estimate. It may be reasonably safe to assert that it lies somewhere between 20 and 80 per cent. The white blood of the light colored mulattoes will gradually diffuse through the darker Negro population and eventually produce a brown hue. That the new mixture which is being formed is

(*Continued on page* 478)

NEMATODE RESISTANCE OF PINEAPPLES

Varietal Resistance of Pineapple Roots to the Nematode Heterodera Radicicola (Greef) Muller*

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TWO varieties of pineapples, Cayenne and Hilo, are grown commercially in Hawaii. The roots of both are subject to attack by the root knot nematode, *Heterodera radicicola.* These frequently become so abundant that they interfere with the normal root development of the plants. To what extent these nematodes damage the plants and how much other, more obscure, conditions contribute to their failure in some areas is not known. The nematode galls are obvious, and the failure of plants has been largely ascribed to their influence. Contrary to this conclusion is the fact that in some areas the roots of pineapple plants were found to be heavily infested with nematode galls, yet the plants remained healthy and vigorous. No economical method has been found to control or prevent nematode attacks on pineapple plants.

The present paper is a report of the results of experiments to test the different available pineapple varieties for resistance to nematode attack.

Review of Literature

Not all of the following papers were available to the writers. Hence it has been necessary to depend on abstracts and statements in other articles for some of the reviews. Such assistance has been properly accredited.

The response of plants to nematode attack

The presence of nematodes on the roots of a plant does not always result in material damage to the host. Indeed nearly forty years ago Vuillemin and Legrain[34] followed a year later by Queva[27] according to Steiner,[31] showed that certain species of plants might even derive distinct advantages in the possession of *Heterodera radicicola* galls on their roots.

The sweet potato is one of the many cultivated hosts of *H. radicicola.* Weimer and Harter,[36] also Poole and Schmidt,[25, 26] agreed that damage, when evident, is more severe on light, sandy soils than it is on other types. Indeed the former writers remarked: "In the plot at Baldwin Park, the Nancy Halls yielded about as well as the Porto Rico; in fact, in this plot, although the plants were fairly heavily infested, the nematodes seemed to have little effect on the yield."

Attacks of *Heterodera schachtii* on potatoes appear to cause some retardation in early growth. Nevertheless, recovery takes place before maturity and ordinarily a normal crop of potatoes is harvested. Miles[18] stated that "From the data given in this table (IV) it is apparent that eelworm intensity has little effect on yield." Others who have arrived at similar conclusions are Morgan,[19, 20] Smith and Miles[30] and Triffit.[33] Miss Triffit's recent work showed that potato plants rather quickly overcame the retardation of early growth. Morgan and Peters[21] do not entirely agree but express their belief that good agricultural practices can counteract, or prevent the damage caused by *H. schachtii.*

Statistical treatment of nematode problems

The only extensive statistical work in relation to plant parasitic nematodes,

*Technical Paper No. 33 of the Experiment Station of the Association of Hawaiian Pineapple Canners, University of Hawaii.

†Geneticist and Associate Nematologist, collaborators.

found by us, was reported by Smith and Prentice.[29] In their study of numbers of *H. schachtii* cysts in the soil, they adequately discussed their technique and gave the probable errors for their counts. Following their method Smith,[28] also Smith and Miles,[30] determined the cyst populations of soils.

Goodey,[10] too, has presented in tabular form the degree of susceptibility of certain legumes to the attack of *Tylenchus dipsaci,* giving the percentages of healthy and deformed seedlings with the standard errors. However, he concluded that he could have developed a more accurate determination of susceptibility and, in fact, suggested such a method. Steiner[31] (pp. 510-511) offered another comment on the work that also seemed to impair its utility.

Others have given some attention to comparative damage between varieties of plants with tabulation of data but without statistical analysis. Fromme[8] decided that his tabulations designed to reveal relative susceptibility of wheat varieties to infection by *Tylenchus tritici,* showed no significant results. Leukel's[13,14] data gave the comparative damage to several varieties of wheat infested by the same nematode. Kanred alone appeared to be resistant. He concluded also that emmer and spelt exhibited less damage than wheat.

Response of different species or varieties to nematode attack

McClintock[16, 17] working with *Heterodera radicicola,* reported that onions and peanuts were quite resistant. Iron, Brabham, and Victor cow peas are more resistant that other varieties of cow peas. Three varieties of soy beans and certain velvet beans showed less resistance, while cereals and grasses did not appear to be seriously affected. He obtained resistant peach root stock and an immune plum on which he grafted desirable, but susceptible, varieties. However, it was Neal,[22] according to Webber and Orton,[35] who is credited with being the first to attempt to find nematode resistant root stocks. That he clearly recognized the importance of using immune varieties is shown by his statement; "I believe the use of trees that are not susceptible to the root-knot for stocks on which to bud or graft the susceptible varieties is the proper solution of the root-knot problem."

Poole and Schmidt[25, 26] grew about 30 varieties of sweet potatoes in North Carolina fields infested by *Heterodera radicicola.* Weimer and Harter[36] had already completed such an experiment in three localities in California. Both found divergences from normal yield of sweet potatoes in some of the varieties. Unfortunately, for statistical analysis there were no check plots of uninfested soil from which to obtain normal yields for comparison with yields of infested plants. There was also no assurance that the soil planted to these varieties was uniformly inoculated; the percentage difference in yields did not hold true throughout the tests.

Wilfarth[37] cited cases of apparent immunity occasionally found among sugar beets in fields infested with *H. schachtii* and suggested the selection of such individuals for the propagation of seed. Husfeld[11] later attempted such a program but his 10,000 plants proved to be as susceptible as the population obtained from infested plants.

Nilsson-Ehle[23] tested several varieties of barley for attacks of *H. schachtii.* Chevalier I and II apparently were immune. He considered immunity in these varieties to be conditioned by a single dominant gene.

Numerous writers have commented on the obvious susceptibility of some varieties of plants. Most of them have reported, however, host preference of nematodes rather than differential susceptibility of the host to nematode attack; but the literature has not always carefully distinguished between these two concepts.

Atkinson[1] was one of the first to publish a list of plants attacked by *H. radicicola.* The list was divided into the classes, "badly affected and slightly affected."

Isbell[12] has recently reported the dis-

"ROOT CAGES" USED IN EXPERIMENTS
Figure 9
Root observation boxes used in these experiments. Size 3½×12×16 inches.

covery of a selection in pole snap beans which is immune to the attacks of *H. radicicola*. This appears to be the first instance of immunity or resistance to nematodes noted in *Phaseolus vulgaris*.

Byars, Johnson and Leukel[5] made observations on rye, oats, spelt, and emmer, and remarked that *Tylenchus tritici* severely attacks rye. Byars[4] points out that this nematode parasitizes wheat more extensively than the other cereal grasses.

From this brief review it is evident that numerous observations have been made regarding the influence of nematodes on their hosts. A few writers quite definitely state that there is no evidence that the particular varieties of plants tested are affected to any appreciable extent by nematodes. On the other hand, there is an extensive literature concerning the damage to certain crops by nematodes. Such material has not been considered as it lies outside the scope of this paper. In all the work reviewed, it appears that no attempts were made to determine statistically the relative amount of injury caused by nematodes to different varieties of a single species. Our present efforts are directed toward ascertaining whether the available varieties of pineapples are equally affected.

Experimental Methods

The root observation boxes used in this work were the "root cages" of Comstock[6] that have been modified somewhat in use at this station. The details of their construction have been fully described by Dean.[7] Two are shown in Figure 9.

The plants were grown in a synthetic soil which had been steam sterilized. The planting material consisted of slips, except in the case of the variety known as Lot 520. Two plants were grown in each observation box. Throughout the experiment the plants were grown under the shelter of a lath house.

After the plants had produced roots varying from 5 to 8 inches in length, one half of the plants in each variety was inoculated with nematodes by placing about 50 egg masses on, or just in front of, the growing point of the visible roots. The remaining half was retained uninoculated to serve as a check.

The inoculum used originally came from nematode egg masses and larvae in pineapple field soil. In order to obtain sufficient quantities of egg masses for inoculation purposes, Godfrey's[9] method of nematode propagation was used. Thus, the nematodes passed several generations on Groit and Whippoorwill cowpea roots immediately prior to their transferrence again to pineapple hosts. When inoculation work was started, roots from these plants were washed free from soil. The protruding egg masses then were picked off and placed in beakers of water. Pipettes were used to distribute each lot of eggs over the root tips of the pineapple plants. A heavy and practically simultaneous inoculation of the observation boxes, therefore, was possible.

While no work has been carried out in an effort to determine whether biological strains of *Heterodera radicicola* exist in Hawaiian soils, observations, so far, have not detected any. The fact that all of the varieties of pineapples grown in the experiment showed abundant, and

rather uniform, nematode attack (Table 3, Column 6) is sufficient proof that the inoculum was satisfactory. Nevertheless, some discussion of this subject has appeared in the literature. Tischler's[32] report, as cited by Steiner,[31] and the latter's excellent paper, also, strongly support the conception of biological strains. On the contrary, Bessey[2] and Malloch[15] found no such evidence for this species of nematode. The present paper attempts to throw no further light on this interesting question. It recognizes the possibility of such a condition existing without, however, in any way vitiating the results set forth herein. Finally, it should be noted that only one kind of host was present in each observation box so there was no problem of host selection involved.

The first planting consisted of 30 plants of each of the following varieties: Cayenne, Hilo, Lot 520 (F_1 hybrids of Wild Brazil × Cayenne), Natal, and Pernambuco. These were started in May 1930. A second group was planted the following September. The varieties then available were Cayenne, Hilo, Ruby, Taboga, and Wild Kailua. The soil was obtained from the same source and other factors were kept, as nearly as possible, the same as in the earlier planting.

As already noted, Cayenne and Hilo are the varieties grown commercially in the Hawaiian Islands. The Lot 520 is a vegetatively propagated hybrid, produced by crossing Cayenne with a wild pineapple from Brazil. It is not grown commercially. The remaining varieties are grown to some extent in other tropical countries.

Six months after date of inoculation, when the plants were approximately 8 months old, the soil was carefully washed from the roots and the following set of records was made.

On check plants:

1. Number of long roots and short roots per plant.
2. Number of old roots and new roots per plant.
3. Length of each root.
4. The amount of fine lateral roots for some of them and notes concerning relative abundance of laterals for all plants.

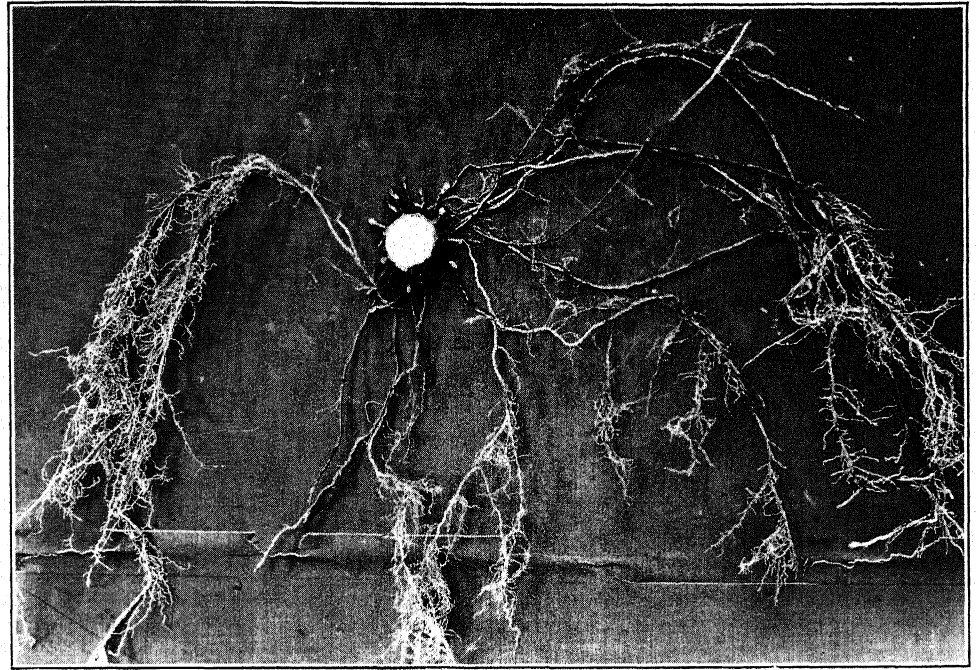
On inoculated plants:

1. Number of long roots and short roots per plant.
2. Number of old roots and new roots per plant.
3. Length of each root.
4. Number of terminal galls per root.
5. Number of non-terminal galls per root.
6. Degree of gall infestation of fine fibrous laterals on each main root.

Due to the fact that it was desired to keep these plants for further observations it was not possible to determine the actual number of small galls on fibrous roots. They were, therefore, estimated on a scale ranging from "trace" to 10. The upper limit, 10, being the maximum infestation considered possible—such a condition not yet being determined. Where less than 5 galls were seen a record of "trace" was made. Five to ten galls were recorded as an infestation of 1, and so on. While this was a rough approximation of the actual infestation, and admittedly one not subject to mathematical analysis, it is felt that it gave a sufficiently accurate picture of the general infestation of lateral roots. These laterals, themselves, varied greatly in abundance in the different varieties so that their numbers, in relation to the small galls upon them, had to be evaluated during the course of our rapid inspection of each root system.

Each plant, after the record of its roots had been made, was placed immediately in the field. After the plant crop is harvested they are to be removed and the roots again examined for degree and severity of infestation.

Throughout this paper the word resistance is employed to designate the tolerance the plant evidences in its reactions to nematode attack. Some plant species are immune to this nematode but no immune pineapple plants are yet known. Some varieties of pineapples, however, are shown herein to be less severely damaged in their roots when attacked. They are considered, therefore, to be more tolerant to nematodes than are others. This tolerance or resistance, when quite pronounced, may



ROOT SYSTEM OF A FIELD-GROWN CAYENNE PLANT

Figure 10

Further growth of visible short roots was abruptly stopped by the formation of terminal galls, produced by nematodes.

serve all practical purposes in growing pineapples where an immune variety cannot be found. Thus, resistance and immunity to attack may mean quite different things but further treatment of this aspect of the subject is unnecessary here. Very satisfactory definitions for these and other terms have been given by Orton.[24]

The authors wish to acknowledge their indebtedness to Eric Ashby, Kenneth R. Kerns, L. H. Kang, and K. Ishiyama who gave valuable assistance in conducting the experiment.

Comparison of Root Systems of Varieties in Check Plants

The ratio of roots to tops and the percentage of fibrous roots was determined for a portion of the check plants of five varieties, data of which are given in Table 1. Root volume was determined both by water displacement and by wet weight; the green tops were weighed. The fibrous roots were separated from the larger roots and the volume determined by the water displacement method. The percentage of fibrous roots for the different varieties was thus obtained.

The largest root to top ratio difference is that between Pernambuco and Hilo, 13.9 ± 2.83. The difference in top to root ratio is approximately 4.90 times the probable error and, therefore, represents a true difference in root systems of these varieties. We should expect differences of the same order as those which distinguish aerial parts of the varieties and Hilo normally produces a larger top growth than Pernambuco.

The percentage of fibrous roots in the different varieties, as shown by the figures in Table 1, is in accord with conclusions drawn from observations of the roots, a condensed summary of which appears later. Lot 520, obviously,

has the largest quantity of fibrous roots and Cayenne the smallest. Natal, Hilo and Pernambuco are about alike.

In comparing the different varieties in regard to number of roots produced, as shown in Table 2, it is evident that differences exist between some varieties and do not exist between others. Thus the difference between Hilo and Taboga is 16.61 times its probable error. Taboga and Ruby have definitely fewer roots per plant than have the other varieties.

The number of roots produced early by a plant may have a bearing upon the future development of its root system in the presence of nematodes. It has been a common observation that in infested fields the early roots ordinarily grow out well and attain considerable length. Later roots, on the contrary, usually are thought to be stopped promptly by severe nematode attack and nearly all of them contain large terminal galls. This quite typical condition is well illustrated in Figure 2. Later in this paper it will be shown exactly to what degree nematodes curtail root growth. At this point it is sufficient to call attention to the fact that considerable variation exists between varieties in the number of roots produced early in the growth of the plants (Table II, Column 5). For example, Cayenne produces 82 per cent of its roots early while Pernambuco sends out 98 per cent of its roots at that time. Hilo, too, may be considered superior to Cayenne in this respect while Natal is inferior, 20 per cent of

TABLE I. Root to top ratio and percentage of fibrous roots in varieties (Checks).

No. of Plants Used	Variety	Wet weight in grams		Ratio	Root Volume cc.	Per cent Fibrous Roots
		Tops	Roots			
2	Cayenne	402	21	19.0 ± --	19.0	26.0
7	Hilo	582	18	32.8 ± 2.69	18.0	35.7
3	Lot 520	715	27	26.7 ± --	25.0	44.0
7	Natal	381	17	22.7 ± 1.22	15.7	30.0
7	Pernambuco	402	23	18.9 ± 0.92	22.0	36.0

TABLE II. Average number, length, and the percentage of old roots per plant and the total root length per plant for check plants.

1.	2.	3	4	5	6
Variety	No. of Plants	Aver. No. Roots	Aver. length	Per cent old roots	Total length of roots per plant
Cayenne	12	39.08 ± 2.48	9.08 ± 0.62	82.00	341.60 inches
Hilo	28	42.57 ± 1.81	10.50 ± 0.21	91.11	435.40 "
Lot 520	14	25.00 ± 1.97	7.50 ± 0.31	83.00	187.50 "
Natal	14	24.80 ± 1.07	12.50 ± 0.02	79.00	310.00 "
Pernambuco	14	34.30 ± 2.26	10.80 ± 0.36	98.00	370.44 "
Ruby	14	16.50 ± 0.81	11.71 ± 0.31	90.90	198.90 "
Taboga	14	11.50 ± 0.50	13.14 ± 0.39	92.54	152.50 "
Wild Kailua	14	20.14 ± 0.70	13.35 ± 0.28	92.55	262.20 "

its roots appearing as new or later produced ones.

Varietal differences in total root length also are pronounced. It is recognized that this feature must be correlated with other factors as well as number of roots and average length of roots. Likewise the size of the root system may have some bearing (other conditions being uniform) on the size of plant and fruit yield. In regard to total length of root system, Hilo exceeds all other varieties tested with 435.5 inches of roots while Cayenne ranks third with a root length of 341.6 inches. It is interesting to note that Taboga and Wild Kailua roots are of approximately equal average length, but while the former has only 11.5 roots per plant with total length of 152.5 inches, the latter produces 20.1 roots and a total length of 262.2 inches.

(To be concluded)

"IAROVIZATION"

A New Factor in Plant Breeding Technique

IT IS announced that the difficulties experienced by plant breeders in crossing plants of different blooming dates can now be overcome by the employment of a series of new methods worked out at the Odessa Institute of Genetics and Plant Breeding by Prof. T. D. Lissenko.

The details of Lissenko's methods have not been published outside of Russia but the strikingly novel ideas upon which these methods are based together with some rather convincing illustrations are presented in the June, 1932, number of *Der Züchter* by A. A. Sapegin, the Director of the Odessa Institute.

According to Lissenko, the development of an organism manifests itself chiefly in well recognizable consecutive stages each of which is characterized by its own specific morphology and physiology. The individual developmental stages require for their completion different complexes of environmental conditions just as different biotypes require such different environments for their normal development.

Lissenko has worked out, for instance, the temperature and temperature duration requirements for the first development stages of wheat. In speaking of the first development stage he, apparently, has in mind the pre-germination stage, i. e., the period when the swollen seed is just going through the most primary life processes of metabolic activity.

Light and darkness are other essential components of the environmental complex, particularly in the case of the so-called "short day" types of plants. Darkness for these "short day" types is just as indispensable as light and is, especially, essential for the completion of the vegetative and the transition into the reproductive stage. But what is particularly striking is Lissenko's alleged discovery that this darkness does not necessarily have to alternate with light and that it can be applied in the required quantity all at once. In other words, a plant can receive its full quota of darkness in its early pre-seedling stage and can then be grown to maturity in uninterrupted light. This is not only true of darkness but also of temperature and other factors making up the environmental complex of the plant. Lissenko's experiments with wheat, millet, corn, cotton, sorgo, sudan grass, vetch, and soybeans have shown that the influence of the direct external factors and their duration, necessary for the completion of the vegetative and for the transition to the reproductive state, is required in exactly the same manner whether the plant is in the state of an ungerminated seed which has been only slightly stimulated into its primary life processes by soaking or whether it is in the state of active vegetative advance.

This occurrence has been designated by Lissenko with the term "Iarovizatzia" which, unfortunately, cannot be anglicized. "Iarovization" has already been adapted by Soviet plant breeders for the purpose of shortening the vegetative period thus accelerating their variety breeding work two or three times. The matching of the blooming periods in parent plants, the breaking up of the persistent vegetative stage in some hybrids and bringing into maturity any exotic plant in a totally strange environment are some of the other possible applications claimed for "iarovization" in plant breeding work.

The direct application of "iarovization" to agricultural practice is also manifold. It has already been employed in Soviet Russia it is claimed in 1930 and 1931 on an area of over a quarter of a million acres in cases where winter wheat has frozen out or where it has been killed by drouth. By "iarovizing" their remaining seed material and sowing it in the spring the Soviet farmers were enabled to obtain a normal yield of high quality grain. In regions where hot, dry winds prevail during the critical period of seed formation the possibilities offered by "iarovization" in shortening the vegetation period are especially important.

Lissenko asserts that "iarovization" does not by any means supplant plant breeding but rather supplements it, that all work on "iarovization" was developed on the basis of differences in the degree of reaction of different biotypes against changes of external conditions. Lissenko expects that in the future "iarovization" will become superfluous through breeding, i. e., that such varieties will be obtained which will require no more "iarovization."

A. J. Bruman.

Division of Foreign Plant Introduction
U. S. Department of Agriculture.

For Livestock Breeders

FERTILITY AND ANIMAL BREEDING, *by* F. H. A. Marshall and John Hammond, 3rd Edition, pp. 1-50. Ministry of Agriculture and Fisheries, Bulletin No. 39. London, 1932.

For the breeder of domestic animals and pet stock this bulletin should be exceptionally useful at it describes in detail the structure and functions of the male and female genital organs as well as the various steps in the process of reproduction.

The authors, although primarily interested in science and research, have a thorough understanding and appreciation of both the scientific and practical phases of animal breeding. Accordingly they have presented their subject from the viewpoint of the practical breeder. The publication is written in an understandable style, technical terms are used sparingly and carefully defined when used, and illustrations are presented where needed.

Breeding management designed to insure normal reproduction is discussed from several different angles, and the discussion is extended to include the selection and mating of breeding animals, the effect of nutrition on reproduction, and the rearing of the offspring. Considerable attention is directed toward the pathology of reproduction, and corrective methods to be employed in case of breeding disorders are outlined. The comparative anatomy, physiology and breeding habits of domestic and pet animals and fowls, together with the practical problems specific to each, are presented in considerable detail.

Many theories and fancies, some of which appear to be well supported by scientific data and others which appear to be questionable or without foundation, are also discussed. In some instances theories are defended by the authors which are subject to question by other investigators, yet the publication is written in an exceedingly interesting and instructive style, is well organized and brief, and should be of great value to the animal breeder.

W. W. Swett.

U. S. Bureau of Dairy Industry.

EFFECT OF STORAGE TEMPERATURE ON DATE OF FLOWERING

In the Paperwhite Narcissus

DAVID GRIFFITHS and R. C. WRIGHT
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INVESTIGATIONS during the past three years have been conducted into the effect of storage environment on the development of the flower in the Paperwhite narcissus. This season about two dozen lots of bulbs have been employed in various ways in both commercial storage and controlled temperatures in refrigerated storage. In these investigations with Paperwhites the point at issue is one of effect of the storage temperature on time of flowering and growth of plants. There is none of the element of keeping quality involved, for the Paperwhite bulb will keep in any storage temperature from 36° F. to the limit of the temperature conditions obtaining in the various situations in which they have been handled from South Florida to the District of Columbia. These bulbs do not rot as do the Dutch stocks.

No attempt will be made to go into details now except with reference to five flats of 35 bulbs each of Florida grown stocks. These are emphasized because they point to a method of treatment which may possibly be of commercial importance, and which is of some interest to breeders and to students of effect of environment.

It should be noted that in all cases it was information on the effect of the storage environment on the comparative date and other flowering characteristics that was mainly sought; consequently it was not practicable to attempt to get plants in early. On the contrary, a handling was practiced which was considered to be average and such as was best calculated to meet the requirements of both early and late lots.

Planting (in flats) occurred September 22 to 25 for all but one or two of the lots mentioned here. After planting, the flats were wet down and placed outside covered with straw until the tops began to appear. On October 20 all of them were placed on the benches of the greenhouse and carried without heat until the first one came into blossom, after which a night temperature of 45° to 50° was specified. Bulbs from both North and South Carolina were also carried at various temperatures mostly for the entire storage season, but a few were taken from storage and potted for forcing August 1.

As a general principle, the effect of cold storage is a dwarfing one on the Paperwhite as on other varieties of narcissus. In all lots stored at 36°, 40°, 50°, and 55° F. for the entire season there was an intolerable amount of dwarfing. Those stored at 36° invariably made practically no growth of top and but little of root. The condition from 40° was but little better. From 50° to 55° the dwarfing was still too great to be commercially tolerable. At 60° there was still dwarfing, although without checks for comparison the plants might be considered normal. Not until a temperature of 70° was reached was the growth considered normal of the bulbs submitted to controlled temperatures for the entire storage season, from June to the end of September.

Bulbs were subjected to 55° and 60° respectively from June 12 to August 1, then potted and placed in a cellar where the temperatures ran 70° to 80° during August and slightly lower in September. In both cases

EFFECT OF STORAGE TEMPRATURES
Figure 11

Reaction of Florida grown Paperwhites to various constant temperatures, August 15 to September 20, following normal storage. Upper left, 60°F, blossomed November 13; right, 55°, blossomed November 8; middle left,, 50°, blossomed November 17; right, check, blossomed November 25; front, 36°, blossomed December 8.

the growth was erratic and unsatisfactory, and the flowering and rooting subnormal.

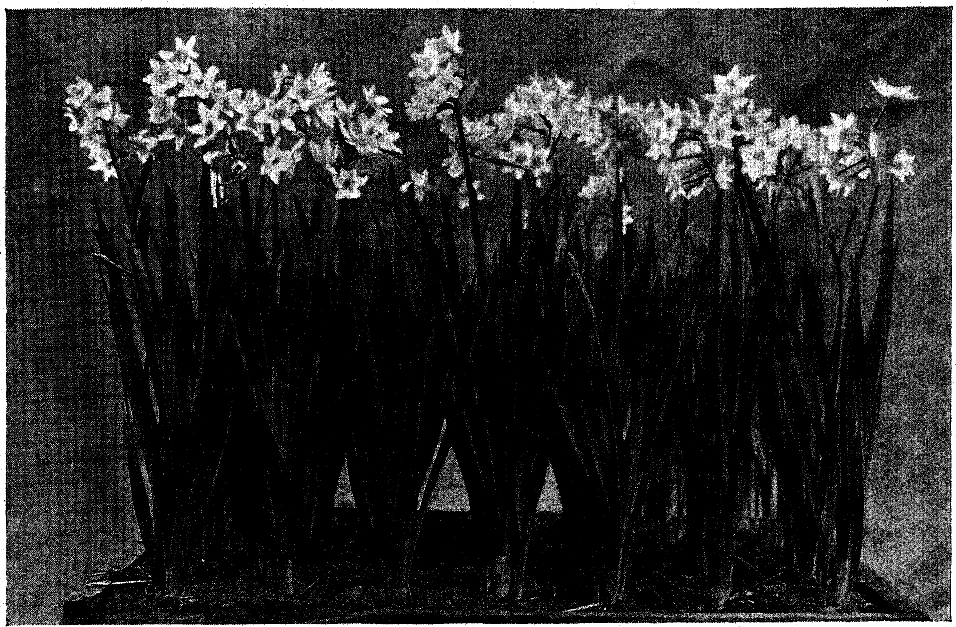
The bulbs of the main Florida stock which are of most importance were received the middle of June and put in ordinary commercial storage on Arlington Experiment Farm, Rosslyn, Va., until August 15, then subjected to various controlled storage temperatures until September 22 to 25, when they were potted. The handling from this date on was identical with that accorded to those in cold storage the entire season.

The behavior of these bulbs was very interesting and important from the commercial point of view. A summary of their behavior follows:

In the reactions there are two points not brought out above that are particularly interesting, namely, the dwarfing of the plants at 50° and 55° was not noticeable in the size of the leaves but in the number of shoots which came out of a bulb. A most unlooked-for reaction was an apparent curtailment of shoot development in the lots stored at 50° and 55°, and a negligible if any diminution at 36°

Table I—Summary of Reactions of Florida Grown Paperwhites to Storage Temperatures, August 15 to September 22

Temperature in Degrees F.	*Date of Flowering*	*Condition of Plants*	*Condition of Flowers*
36	12/8	slightly dwarfed	slightly dwarfed
50	11/17	subnormal	subnormal
55	11/8	subnormal	subnormal
60	11/13	subnormal	subnormal
Check	11/23	normal	normal



EARLY BLOSSOMING OF TREATED BULBS

Figure 12

The flat shown in upper right of previous figure when in height of blossom. The bulbs were exposed to a storage temperature of 55°F. The plants blossomed two weeks before the check lot.

and 60°. The flat stored at 36° was very much retarded, but when it came into full flower about the middle of December the shoots were as numerous as in the checks, but the stems were shorter although not detrimentally so. It will be seen that the reaction is a complicated one.

The defect of low temperatures on the inflorescence was more striking but it ran parallel with the effect on the plants. The greatest floral injury occurred in the bulbs stored at 50° and 55°. It was such as not to be commercially tolerable at 50°, but only slight at 55°. The diminution in the size of the flower, while evident, was not serious in either the 50° or the 55° temperatures. The diminution in the number of florets at 50°, however, was serious. It is thought that there was some injury of this kind at 55°, but it was not serious enough to attract attention when the flat was in blossom. This kind of reaction was wholly unexpected; consequently, since the inflorescence was commercially acceptable, the flowers were cut at full anthesis with satisfactory spikes of blossoms. A comparison of the number of florets from 50° and the check, however, has been made and shows a yield of 284 and 430 respectively, each having 35 bulbs. The flat stored at 36° also had 35 bulbs, but produced 416 florets.

A reading on single pots of six bulbs each, South Carolina grown but stored under controlled temperatures as designated below, throws further light on the subject.

One pot in this lot was stored at 55° and was planted September 1, placed in the rooting cellar to September 22, and then carried like the others. It produced six spikes and 30 florets from six bulbs, starting to open November 22.

It is realized that the practical man who has followed us up to this point may think that cold storaging of Paperwhites produces very compli-

cated results. So it does! But there appear to be a few lessons that can be learned from the data.

1. That storing the bulbs at 55° F. for six weeks before potting the latter part of September induces a precocity of flowering of over two weeks with a floral modification which is commercially tolerable. In other words, the treatment appears to be practicable if the flowers are needed earlier than they come naturally.

2. That a temperature of 36° F. for the same length of time retards flowering beyond the point at which flowering occurs from commercial storage, with floral injury that is commercially negligible.

3. That storage of Paperwhites below 60° F. for the entire storage season from June to the end of September should not be considered.

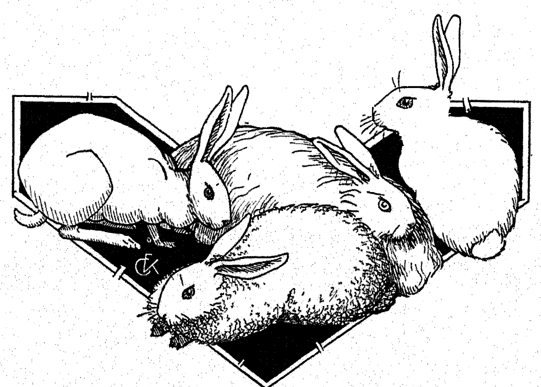
4. All this seems to coincide in a general way (but with modifications) with the work of Blaauw[1] who found that to induce precocity of flowering in the tulip it was best to submit it to a temperature of about 70° for a month and then to 50° until potting.

1. BLAAUW, PROF. A. H. Proc. Konin. Akad. van Wetenshcappen te Amsterdam, 10: 10, p. 1342, 1926.

Table II—Effect of Storage Temperatures on South Carolina Grown Bulbs Stored from June 15 to September 22.

Temp. in Degrees F.	*Date of Flowering*	*No. of Spikes*	*No. of Florets*
50	....	....	no growth
60	11/12	6	32
70	11/28	6	70
Check	11/27	6	87

EDITOR'S NOTE: The results reported above on the effects of storage temperature on narcissus bulbs evidently parallel the iarovization experiments of Lissenko, in that a temperature treatment of dormant bulbs has drastically affected their later development. Since Mr. Bruman's note called attention to Lissenko's work, American research along this same line has come to our attention. A report of this is anticipated in an early number of the JOURNAL.



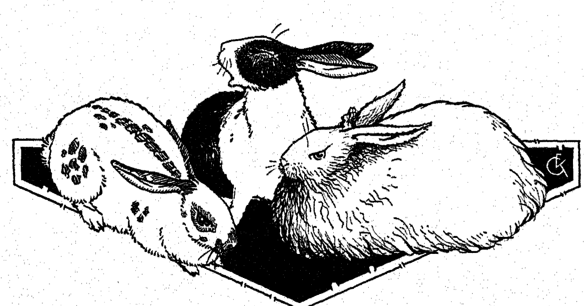
GENETIC ART

ART is based on the physical and living world about us, and on our knowledge, theories, and beliefs of how it is put together. The Madonnas and saints of Raphael and Michaelangelo and their contemporaries reflect the religious outlook of the Middle Ages. It is more than a coincidence that the greatest agricultural and botanical encyclopedias of ancient and medieval times were Chinese and that the art of China is based on the more common species of cultivated and wild plants of that region. To what we may trace the modernistic, broken mirror era of Twentieth Century art will be left to the reader's conjectural ability.

The program of the recent Genetic Congress was adorned with a series of conventional designs based on such important genetic media as the pea, the fruit fly, the mouse and the maize plant. These decorations, which added so greatly to the artistic merit of the program owed their origin to the combined inspirations of Dr. E. M. East and Dr. Clyde E. Keeler. Dr. Keeler has generously consented to prepare a series of genetic sketches and designs to be used when opportunity offers in the make-up of the JOURNAL. The first contributions to this series, reproduced herewith, and on the cover of the JOURNAL, deal with two genera that are Keeler's special pets, and the chosen field of much genetic research—mice and rabbits. It is hoped that in later issues we will be privileged to publish sketches illustrating the genetic peculiarities of other organisms.

The design appearing on the cover of the JOURNAL illustrates the genetic relationships of dominant spotting in mice. A single dose of this gene produces the type of mouse illustrated by the mother—with a white blaze on the forehead, a white belly, and possibly variegation elsewhere on the body. When the character is inherited from both parents so that the genes are present in double dose it produces a lethal form of anemia. The offspring who are so unfortunate as to be blessed with a double inheritance of this gene are so weak that they are not able to compete for their rightful share of their mother's milk. This is, however, not the sole reason for their failure to survive, because even when artificially fed they soon die.

The four rabbits shown on the opposite page illustrate various combinations of characters. On the left is a rex rabbit, next to it an angora, at the right a normal and below an angora-rex. The angora and rex

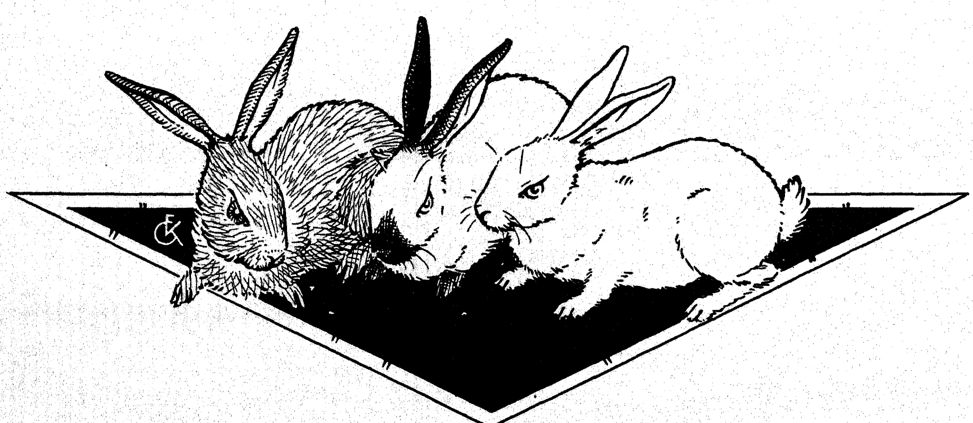


genes are inherited independently. Rex is a recessive to normal hair development, characterized by the absence of the guard hairs and reduction in length of underfur hairs.

On the preceding page are shown three linked genes in the rabbit. These produce two coat patterns, English and Dutch and the angora coat. The characteristic results of three allelomorphic genes are shown below. These are chinchilla, Himalayan, and the albino. The gene for normal coat color, that is, for the coat color of the wild rabbit, is also an allelomorph in this series. A given rabbit can inherit any two genes in this series, but cannot inherit more than this number.

A pair of linked genes in mice is shown above. These are the genes for recessive hairlessness and recessive piebald. A normal mouse is also shown. This form of spotting is different from the dominant spotting in mice discussed in an earlier paragraph.

R. C. C.



TWINNING, SEX RATIOS, AND GENETIC VARIABILITY IN BIRTH WEIGHT IN SHEEP

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THE literature on the sex ratio in sheep shows fairly close agreement in the findings of seven different American, British, French, and German investigators who observed more than 80,000 sheep in all. With the exception of two reports, both based on small numbers, ewe lambs outnumber ram lambs in all cases. The figure most generally given is a little more than 49 males to a little less than 51 females. The ratios reported are based on numbers born, or secondary sex ratios. The primary ratio, or proportion of the sexes at conception, would be practically impossible to obtain and the tertiary sex ratios or proportions among those reaching maturity were not studied. Some additional information about sex ratios, twinning, and genetic aspects of birth weight comes from a recent examination of data from the Iowa Station flock.

The Iowa Agricultural Experiment Station has maintained a small flock of sheep under farm conditions for many years.* The flock is used mainly to study the effects of different rations for wintering the ewes. It was started in 1914 with ewes of mixed breeding showing Leicester, Merino, Lincoln, and Shropshire blood. Purebred Hampshire rams have been used on the flock ever since and, as the replacements for the ewes have been chosen from within the flock, the flock now consists entirely of high grade Hampshires outwardly indistinguishable from purebreds. The rams have always been purchased and usually have been so little related to each other that almost no close breeding has been practiced. There have been a few exceptions to this and even a few matings of sire to daughter. Usually only one ram is used and is kept in service for only one season. Records of 1,019 lambs born from 1915 to 1930, inclusive, were studied.

Sex Ratio and Litter Size

Two hundred and sixteen of the lambs were born as singles, 722 were twins and 81 were triplets; or, of the ewes which lambed, nearly 36 per cent bore singles, nearly 60 per cent bore twins and less than five per cent produced triplets. Among the singles 47.7 per cent were males, among the twins 49.6 per cent were males and among the triplets 40.7 per cent were males. Four hundred and ninety-three of the 1,019 lambs were males which is 48.4 per cent of the total. The differences in the percentages of males found among singles, twins and triplets were not statistically significant although the percentage of males among triplets is low enough to suggest that it might be worth further investigation.

About five per cent of the lambs were stillborn. Almost the same proportion were stillborn among the single and the twin lambs. The triplets included a slightly higher proportion of stillbirths than the singles or twins. Twenty-two of the stillborn lambs were male and 27 were female—an insignificant difference between the two sexes in pre-natal mortality.

Do Identical Twins Occur in Sheep?

Three sex combinations are possible among twins; namely, both rams, ram and ewe, or both ewes. If sex determination is purely a matter of chance determining which kind of spermatozoon

*Under the direction of Dr. John M. Evvard and later of Prof. C. C. Culbertson.

fertilizes the ovum, the distribution of the three kinds of twins should be according to formula $(p+q)^2$ where p is the proportion of rams and q is the proportion of ewes among all lambs born in the flock. One possible cause for deviation from this rule of the binomial square would be the frequent occurrence of identical (monozygotic) twins which would increase the proportion of like-sexed twins. Indeed this is the common basis for estimating the proportion of identical twins among human births.

Table I shows the actual numbers of twins of each sex combination in this flock and the numbers expected on the basis (1) of the sex ratio among these twins, (2) of the sex ratio in this whole flock, and (3) of a general sex ratio of about 49 per cent males reported from many other flocks.

The actual numbers agree with the expected numbers very closely. There is a slight excess of twins of unlike sex instead of a deficiency, as would be expected if identical twins were at all frequent in sheep. This is in agreement with Laplaud and Garnier who report a twin ratio of 17♂♂:37♀♂: 16♀♀ from the Vaux-de-Cernay flock. This, of course, proves only that the proportion of identical twins in sheep is low. Such twins perhaps may occur at rare intervals. In a group of data no more extensive than ours something like 20 per cent of the like-sexed twins would need to be identical for the difference between actual and expected numbers to be statistically significant. Hence as the matter rests now, these data give no positive indication of the existence of *any* identical twins in sheep, yet they do not rule out the possibility that such twins may occur, at least occasionally. The probability of their occurrence is not large enough to warrant the investigator's going to much extra expense or trouble in seeking like-sexed twins for pairing against each other in order better to control genetic differences in experiments on nutrition and management of sheep.*

Genetic Influences on Birth Weight

Part of the variation in birth weight was caused by genetic differences between the lambs, part of it by definite environmental influences and part by what, for lack of a better term, may be called accidents of development. The data permit some crude estimates of the relative importance of these causes of variation.

The analysis consisted of comparing the standard deviations of individual birth weights found when all lambs are studied as a single group with the corresponding standard deviations found within classes when the population was divided according to the independent variable being studied. Table II summarizes the calculations for the weights of litter mates as compared with the weights of all twin lambs. The squared standard deviation or variance of birth

TABLE I—Actual and Expected Numbers of Twinnings of Each Sex Combination

Kind of Twin	Observed	Expected: from ratio in twins	from ratio in flock	from general ratio
Both rams	87	89	85	87
Ram and ewe	184	180	180	180
Both ewes	90	92	96	94

*Since this was written a paper by Clark has appeared (*Proc. Amer. Soc. An. Prod.* for November, 1931, pp. 207-209) quoting a twin ratio of 38♂♂:67♂♀:34♀♀ from the Alexander Graham Bell flock, a ratio of from 87♂♂:187♂♀:83♀♀ from a Merino flock reported by "Bernadin," and announcing a ratio of 129♂♂:273♂♀:121♀♀ from the flocks of the University of Minnesota. In all save the small Bell flock the twins of unlike sex actually outnumber (although insignificantly) the twins of like sex, thus making it appear still less possible that identical twins occur frequently among sheep.

weights among all the 174 twin males was 2.57, while the variance found within the litter for birth weights of twin males was only 1.33. In short the variance was 48 per cent less when the study was restricted to lambs born in the same litter than it was when all twin males were included. Expressed in more familiar terms, 48 per cent of the squared standard deviation in birth weights disappears when "litter" is "held constant." The corresponding figure for twin ewes is 46 per cent and for twins of unlike sex is 44 per cent. The weighted average is slightly more than 45 per cent.

It is worth while to inquire more precisely into what is included under the term "litter." The effects of differences in litter size do not enter into the problem at all since the analysis is confined to lambs all born in litters of two.

First of all, litter mates have the same sire and dam and should thus be somewhat more like each other genetically than other lambs are. Secondly, litter mates develop in the same uterus contemporaneously and therefore are subjected to influence by the same general variations in the nutrition or other physiological conditions of the dam during their fetal development. Thirdly, litter mates are, of course, born in the same year. All lambs born in the same year are subject to the same influence of weather variations, differences in management and general influences of that kind which might not be the same for lambs born in different years.

What then, could cause litter mates to differ in birth weight? In the first place, there are genetic differences between full sibs. In a random bred population and for characteristics without dominance, the genetic variance between lambs from a single pair of parents should be just half of the genetic variance between all lambs. While these lambs were not strictly random bred, yet only a few were very closely bred and only the dams of the first few lamb crops were distinct outcrosses. Hence these data come close to fulfilling the condition of coming from a population bred at random (within the limits of the Hampshire breed). All lambs born in a single year were by the same sire (except for the 1918 lamb crop) and therefore half brothers, yet many different rams were used in the period of these experiments and there are many more differences between lambs having neither parent in common than there are between those having a sire in common. The existence of these half sibs would have a slight tendency to cause more than half of the truly genetic variance to appear in the variance found between litter mates. If dominance were general among factors affecting birth weight the correlation between full sibs would be somewhat lowered. This would tend to cause more than half of the truly genetic variance to be found in the variance between litter mates. Whether dominance is frequent among the factors affecting birth weight is debatable.

TABLE II—Analysis of Variance in Birth Weight Showing the Effect of "Litter"

Group of lambs studied	Variance due to	Degrees of freedom	Sum of squares	Mean square	Portion of variance excluded from remainder
Twin males	"Litter"	86	329.1	3.83	$\frac{2.57-1.33}{2.57}=.48$
	Remainder	87	115.3	1.33	
	Total	173	444.4	2.57	
Twin females	"Litter"	89	348.4	3.91	$\frac{2.67-1.44}{2.67}=.46$
	Remainder	90	129.7	1.44	
	Total	179	478.1	2.67	
Twins of unlike sex	"Litter"	183	828.9	4.53	$\frac{3.15-1.77}{3.15}=.44$
	Remainder	184	325.6	1.77	
	Total	367	1154.5	3.15	

The second cause of differences between the birth weights of litter mates is what we have called accidents of development such as perhaps might arise from one embryo becoming implanted in the uterus in a position more favorable for nourishment than the other.

A third cause of differences between litter mates may be competition between them of such a nature that the one which first gets a slight advantage over the other is thereby enabled to gain a still greater advantage. That competition exists is certain from the fact that twins are not as large as singles and triplets are not as large as twins. That the competition could sometimes result in an ever-increasing advantage to the one which first gets ahead seems plausible, although how a critical experiment could be planned to measure the extent of such competition is not clear.

In short, we find among these twins that slightly more than 45 per cent of the variance in the birth weights of the individual lambs arises from the combined effects of controlled environment, i. e., controlled to the extreme degree of development within the same uterus and of genetic differences which exist between lambs not full brothers or sisters to each other but which do not exist between litter mates. Probably this is a little less than half of the total genetic differences. On the other hand 55 per cent of the variance in birth weights of twins in this flock arises from uncontrollable environment (including the possible effects of intra-uterine competition) and from genetic differences which exist between full brothers and sisters.

If we let h^2 represent the portion of the total variance in birth weight which is truly genetic, e^2 the portion which would disappear if it were possible for all the lambs to be developed in the same uterus contemporaneously without being genetically closely related to each other, and d^2 represent the portion of the variance caused by "accidents of development" (including intra-uterine competition which may not affect all lambs to the same extent), we have the following equations:

$$e^2 + \left\{ \frac{h^2}{2} - x \right\} = .45$$

$$d^2 + \left\{ \frac{h^2}{2} + x \right\} = .55$$

Two things prevent solution for e^2, d^2 and h^2. In the first place there are only two equations. In the second place we are not sure just how much more than half of h^2 is included in the second equation, i. e., the value of x is unknown although it must be rather small.

A very rough approximation to the value of h^2 can be made by comparing the variance between paternal half brothers and sisters with the gross variance. The basis for that analysis is shown in Table III.

One ram was used each year (except in 1917-18, when two were used). Hence separation of the lambs by year born separates them roughly into groups of paternal half brothers and sisters. The difference in weight of males and females was so slight (although statistically significant) and the proportion of the sexes was so nearly

TABLE III—Analysis of Variance in Birth Weights of Twin Lambs by "Year" and Ration

Variance due to	Degrees of freedom	Sum of squares	Mean square	Portion of variance excluded from remainder
Total	721	2082.8	2.889	$\frac{2.889-2.588}{2.889} = .104$
"year"	15	256.0	17.065	
Remainder	706	1826.8	2.588	$\frac{2.889-2.506}{2.889} = .133$
Ration within year	51	185.6	3.640	
Final remainder	655	1641.2	2.506	

equal in different years and lots that sex differences in birth weight could be disregarded in this analysis. Unfortunately (for our purposes) division by year also separates them into groups exposed to the same seasonal influences or general differences in the kind of rations fed. Thus one cannot be certain how much of the 10.4 per cent of the variance, which disappears when they are thus separated, is due to their having the same sire and how much is due to their having been exposed to the same weather variations or to variations in the general value of all the rations fed in a single season. When the weights are separated according to the rations fed to the ewes within each year, 2.9 per cent more of the variance disappears. If we make the assumption (which seems extreme and cannot be rigorously tested) that the differences between seasons have just as much effect on birth weight as the differences between rations within years, there is left 7.5 per cent of the gross variance to be attributed to the influence of the sire.

This should be about one-fourth of the total genetic fraction of the variance (h^2). Thus h^2 would be estimated at about .30. We regard this as probably an extreme upper limit, since it may include some increase in the general average size of the ewes in the flock and also since seasonal influences may have been more important than ration differences within the year. The yearly average birth weights for twin lambs were as follows:

1915—8.59
1916—8.28
1917—7.47
1918—8.45
1919—9.15
1920—9.38
1921—9.80
1922—9.67
1923—8.74
1924—9.72
1925—9.61
1926—9.96
1927—8.91
1928—9.21
1929—9.05
1930—9.57

These show a downward trend for the first two years, then an upward trend for four years and after that there is no distinct trend. On account of this lack of regular trend, we attribute very little of the variance in birth weights from year to year to any steady change in the ewe flock but the possibility that the yearly variations were caused by seasonal conditions to a greater extent than we have estimated still remains.

Our estimate therefore is that not more than 25 to 30 per cent of the variance in birth weight actually found in these data was genuinely genetic (h^2), while about 30 to 35 per cent was caused by tangible environment (e^2) and something like 40 to 45 per cent arose from intangible environment or "accidents of development" (d^2). These estimates involve so much approximation that they must be very tentative but they seem to be the most accurate we can make from the data in hand. Perhaps some one with access to records from a large flock of a single breed in which several rams have been used each year can make a sounder estimate for that flock. These percentages, of course, would be expected to vary somewhat in other flocks. Thus in a flock where several breeds were kept or in a flock recently crossbred one would expect the percentage of genetic differences to be larger and in a pure line or (which is the nearest thing to that in animal breeding) a highly inbred flock consisting of but one inbred line one would expect the genetic portion of the variance in birth weights to approach zero.

Summary

The sex ratio in sheep is normally about 49 per cent males to 51 per cent females.

Identical twins, if they occur at all in sheep, must be rare.

The genetic portion of the variance in birth weight in the Iowa Agricultural Experiment Station flock of high grade Hampshires can hardly exceed 25 to 30

per cent. The portion of the variance arising from differences in tangible environment is at least 30 to 35 per cent, while 40 to 45 per cent or more arises from intangible environment or accidents of development.

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A FRIEND OF THE NEGRO

(*Continued from page* 458)

"astonishingly uniform in its physical character" is clearly an overstatement. The remark is based upon the conclusions of Herskovits, which the author has taken rather too uncritically. The Negroes imported from various parts of Africa were a very diversified lot, like the ancestors of our white population. There is no adequate evidence that anything very remarkable is happening beyond what might be expected from the fusion of two or three diversified racial stocks.

The greater part of Mr. Embree's volume is concerned with the history and achievements of the American Negro, and the relation of Negroes to whites in different regions and periods. Having grown up in Kentuck with his Abolitionist grandfather, to whom he pays a debt of filial gratitude in one of his chapters, Mr. Embree has long been familiar with Negroes and their ways. In his later position of leadership in the administration of the Julius Rosenwald Foundation, which is devoted mainly to promoting the welfare of Negroes, he has had unusual opportunity of becoming acquainted with the broader problems with which the Negroes are confronted. His training and present position may account in a measure for his adoption of the rôle of apologist for the brown man. He writes with a strong sympathy for the Negro in his struggles against unfavorable conditions, and he writes also with an unusual insight into Negro psychology. "Brown America" deserves to be widely read. As the author explains, there is an amazing lack of knowledge of Negroes, even among people who have spent their lives among them. Mr. Embree's book will be very helpful in leading to a real understanding of our brown neighbors.

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University of California.

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HAIRLESS RATS DURING DEVELOPMENT

Frontispiece

A and *B*—Hairless female and hairless male twenty-eight days old; *C*—Young hairless rat, one month old. The hair is all shed except for the vibrissae; *D*—A hairless male at three months. A slight regeneration of hair occurs at this age; *E*—A hairless female at three months. There is less hair regeneration in the female. This mutation arose under conditions that render it improbable that it is related to other genes for hairlessness in the rat.

A HAIRLESS MUTATION IN THE RAT

Willard Wilder,* R. M. Bethke,* C. H. Kick* and W. P. Spencer†

HEREDITARY loss of hair has been described under various names in several species of mammals including rabbits[1], house mice[2,3,4], white footed mice[5], rats[6,7], swine[8], dogs[9], cattle[9,10], horses[9], sheep[11], and man[12]. The published accounts of these mutants indicate that they are not all parallel or identical, as they differ widely in their phenotypic expression and degree of dominance or recessiveness.

Roberts[6,7] has made a study of and published two notes on the inheritance of hypotrichosis or hairlessness in rats. The present paper is an account of the origin, mode of inheritance and phenotypic expression of an independent mutation to "hairless" in the rat colony of the Nutrition Section of the Animal Industry Department of the Ohio Agricultural Experiment Station. As a contribution to the study of relative mutation rate in different genetic loci in mammals it seemed worth while to record this case. Furthermore, the conditions under which the mutant character was first observed were rather unusual.

In the past ten years upwards of 20,000 rats have been raised in the Animal Industry Department. The original stock came from the Wistar Institute about ten years ago. Eight years ago females of this stock were crossed with Wisconsin piebald stock males. At this time a wild male was accidentally introduced into the colony. At present the stock colony contains no self colored rats; about half the individuals are albino and the other half black piebald with a few agouti piebald. No attempt is made to segregate color patterns in the various nutrition projects constantly in progress. For eight years the stock has received no new blood. It has been inbred to such an extent that the appearance of a new recessive trait in the colony is beyond reasonable doubt due to mutation. The stock rats are given a standard stock diet.

Origin of Hairless

In order to study the effect of fluorine in the diet, 11 litters of stock rats twenty-four days old were taken and distributed one rat per cage from each litter in eleven different cages. These rats and their descendants used in the experiment were designated by numbers. These eleven groups received a diet differing for each group in the amount of rock phosphate containing fluorine. The three females and two males in Cage 14 were given a medium dosage of fluorine. The diet for this cage consisted of:

Ground yellow corn	71.95 %
Soybean oil meal	10.00 %
Casein	10.00 %
Yeast	3.00 %
Cod liver oil	2.00 %
NaCl	1.00 %
Ruhms rock phosphate	.70 %
Piqua limestone	1.35 %

From the third litter of female 8258 of Cage 14 two females and a male were chosen and numbered male 451, female 452, and female 453, and from the third litter of female 8254 were chosen male 454, female 455 and female 456. These were placed in a new cage and continued as part of the F_1 generation of the fluorine experiment. From the second litter of female 452 three rats were chosen and designated male 1436, female 1437, and female 1438. From the second litter of female 453 three rats were selected and numbered male 1439, female 1440 and female 1441. These six rats in a new cage formed part of the F_2 of the fluorine experiment.

In the second litter of female 1437 a peculiar condition of the young at weaning age was observed. Some of

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the litter lost all of their hair and became absolutely bare. This litter contained eight young and was reduced to six at birth in accord with procedure in the experiment. Of these five were weaned, two normal females, one normal male and two hairless females. These rats were born on December 1st, 1930, the first of the new hairless mutant to appear. In the third litter of female 1438, born January 17, 1931, the same condition was noticed. This litter of nine was reduced to six and at weaning there were four normal females and two hairless males. Litters four, five and six of female 1438, litters two and four of female 1440 and the sixth litter of female 1441 contained each one or more hairless young. Subsequent breeding tests have shown hairless to be recessive to normal. It appears from the above that of the six rats of the F_2 generation all four females, 1437, 1438, 1440 and 1441, were heterozygous for hairless and of the two males 1436 and 1439 one was heterozygous for hairless and the other was homozygous normal.

All hairless rats trace back directly to female 8258, one of the original five rats placed in Cage 14. As two of her daughters, female 452 and female 453, had hairless descendants in the F_3 generation it is possible that they received the *h* gene from female 8258 or her mate. Another possibility is that male 451 or male 454 was heterozygous for hairless and on mating successively with female 452 and female 453 transmitted the hairless gene to the offspring of both. In this case the mutation could have arisen in the F_1 generation of the fluorine experiment. It seems from the pedigree most reasonable to suppose that the mutation had occurred in one locus of a single male of Cage 14 or of female 8254 or female 8258 or that it arose in male 451 or male 454 of the fluorine experiment. Had the recessive gene arisen by mutation earlier than this it would have been expected to appear in homozygous form in some of the other ten cages or their descendants, as these all came from the same 11 litters of stock rats. Without further evidence we are not justified in attributing the origin of hairless to the fluorine diet although it is quite possible from the pedigree records that the mutation occurred after the rats were placed on the fluorine diet. It seems best to consider the fluorine diet and the advent of this new type as coincident and not showing any causal relation. The fluorine diet is unnecessary for the expression of the hairless character and apparently has no more effect on these than on normal rats. Most of the hairless rats have been raised on the standard stock diet minus the fluorine ingredient.

Description of the Hairless Character

At birth hairless rats are normal in weight and size. The hair comes in as in normal rats. About the 19th or 20th day a hairless ring appears around the mouth of the rat. By the 23rd day the hair commences to fall out all over the body (See Frontispiece, *A* and *B*). In a few more days the rat is completely hairless (Frontispiece, *C*). In three or four months hair begins to come in on the shoulders and back. This coat of hair is thin and the hairs are long and coarse. There is some sexual dimorphism as hair regeneration at this time is greater in males than in females (Frontispiece *D* and *E*). In about a month this hair comes out and the skin becomes flabby and falls in deep folds around the head, neck and legs, giving the rat a rhinoceros-like appearance. On aging the skin becomes covered with small, scaly lumps and sores. The toe nails grow abnormally long and become curved (See Figures 1 and 2). At this time the rats are very sensitive, and due to the tender skin they object to being touched. As they grow old they become thin and die. Our hairless rats live only about one-third as long as the normal rats.

The hairless females are very frequently sterile. In our stocks only five hairless females have given birth to

"RHINOCEROS" RATS
Figure 1

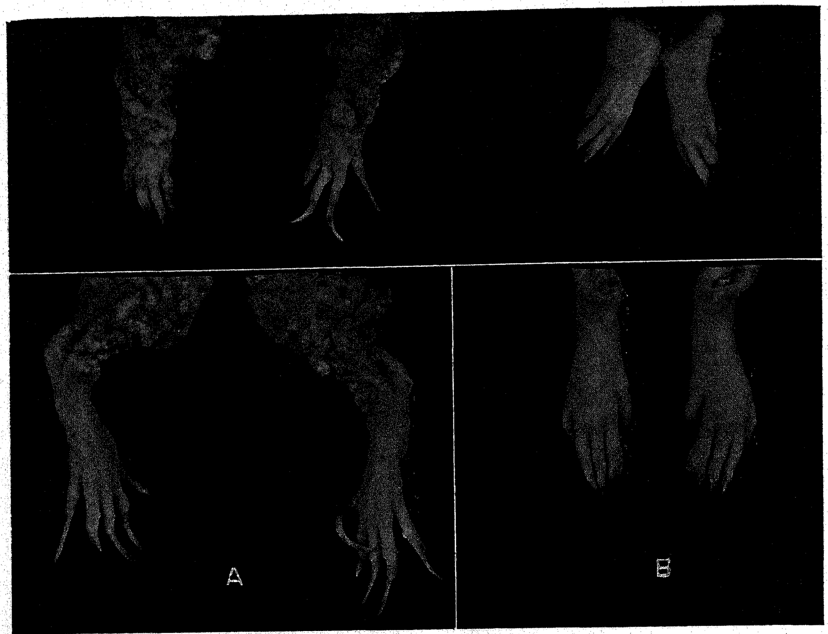
A—A hairless male at seven and one-half months. The thick, deeply wrinkled skin is covered with small, scaly sores. The toe-nails are extremely long. *B*—A hairless female at seven and one-half months. The skin is so deeply folded that the eyes are almost shut. The hairless rats only live about a third as long as the normal. Many of the females are sterile.

litters. Like other epidermal structures, the mammary glands are abnormal and in most cases seem partially or almost wholly atrophied. However, one hairless female did nurse two of her young from a litter of eight. The others of this litter died at birth or soon after. It is possible to transfer the young of hairless mothers to foster mothers. The hairless males approach the normal in fertility. The stock is best maintained by mating hairless males to females heterozygous for hairless. Crew and Mirskaia[4] have shown in hairless mice that in later generations the sterility of the females has become progressively less. They interpret this as due to the introduction into the stock through continued breeding of modifying factors tending to nullify the effect of the hairless gene as regards sterility. Our experiments have not progressed sufficiently to demonstrate the accumulation of these favorable modifiers. This mutation in the rat seems to parallel quite closely the mutant of the same name in the mouse as described by Crew and Mirskaia[4].

The Genetic Behavior of Hairless

Like the hairless reported by Roberts[6,7], this mutant behaves as a simple Mendelian recessive, sexually dimorphic in that homozygous females are often sterile and that hairless males tend to regenerate more hair than do females during the wave of hair regeneration about the third or fourth month.

In 25 litters from heterozygous normal to heterozygous normal rats. the distribution was 140 normal: 45 $\pm$ 3.97 hairless. The sex of the offspring was recorded in sufficient cases to show that the character is not sex-linked. In six backcross litters of heterozygous normal females to hairless males the ratio was 21 normal: 17 $\pm$ 2.08 hairless. In a number of crosses



LONG CLAWS OF HAIRLESS RATS
Figure 2

A—Legs of an old hairless rat. The skin is deeply folded, covered with sores, and the nails very long. *B*—The legs of a normal rat are shown for comparison. The extreme development of claws has been noted in other hairless mutations.

albino was shown not to be linked to hairless. We have made no further linkage studies.

Summary

1. The occurrence of a "hairless" mutation in the rat is recorded.

2. The hairless rat develops a normal coat of hair which is lost at weaning age. The skin becomes wrinkled and covered with scaly sores. The nails grow very long. The females are generally sterile and the mammary glands are deficient. There is a slight regeneration of hair about the third month. The hairless rat dies at an earlier age than the normal.

3. Breeding experiments show that hairless is a simple Mendelian recessive and not linked to albino.

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"MEDICAL GENETICS"

A Necessity in the Up-to-date Medical Curriculum

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THE inclusion of a course dealing with pathological heredity in man in the medical curriculum is one of the outstanding needs in medical education today. The medical student will become the practitioner of tomorrow, with the duty and responsibility of maintaining not only the health of the individual but of the community; of caring for not only the immediate well being of the population but of planning for the continued well being of the people a generation hence. At present, one side of his education in this respect is inadequate; it has not kept pace with the accumulated knowledge. It is true that there is no room in the present crowded medical curriculum for such a course, which seems to be the main reason for not including it. If the course be essential, and many of the advanced medical educators think that it is, then the medical curriculum will have to be altered to include it, or lengthened so as to permit adequate training in this respect.

The purpose of this brief note is not to deal with the medical curriculum as a whole, hence much that might be said in this respect will be omitted. It might be pointed out, however, that much that is taught the medical student today has no real relation to the ultimate purpose of his education—the practice of medicine. Lectures that take up many hours of time merely repeat what the student could read with ease from a textbook. They are a mediaevalism retained from the days when there were no books, and teaching could only be carried out by word of mouth. Lectures should give only what the student cannot glean from his textbooks. This is not the fault of the professors necessarily. Many students rebel when lectures take them off the beaten track. Elaborate laboratory experiments which are excellent from the standpoint of the person intending to do research in that particular branch, but which contribute nothing to the student's actual medical knowledge, should be performed at the lecture desk by the demonstrator, thus saving hours of time for the student to spend on the acquisition of a knowledge of disease, its symptoms, its prevention and cure. A knowledge of heredity in disease is of more importance to the practitioner than is the memorizing of many elaborate chemical formulae, or of an intimate knowledge of the numerous theories of urine secretion.

Teaching Medical Heredity

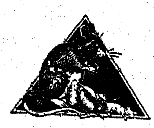
For the time being such a course in heredity would mainly be in the form of lectures, supplementing what is in the few textbooks dealing with pathological heredity in man. There would be practical work as opportunities occurred in clinical material. It would come in the last year of the medical course when the student was thoroughly familiar with the symptomatology of disease and would in no way interfere with the clinician's province in teaching. Students do not gain this knowledge of their own accord if left to acquire it by themselves. It is becoming increasingly important as the death rate from constitutional disorders mounts, due to the decrease in death rate from infectious diseases. Unless the medical schools introduce this work into their curricula, the medical profession will find itself in the anomalous situation of being forced to recognize that the laity know far more about certain phases of disease than they themselves do.

It is true that genetics is a young science, and that genetics as applied to medicine is a mere infant. But it is

a very lusty one and will keep on crying until it is heard. Although only in its infancy, and although we cannot speak with that precision upon many of the topics related to it that we should like, we will make progress only as we acquaint our medical students with what we know, and inspire them to test the truth of our statements in the light of clinical observation, and report on the tests. As in all sciences, ideas will have to undergo constant revision. Because we knew so little about the treatment of pernicious anemia did not prevent us from teaching what we knew. Our knowledge of pernicious anemia has been entirely revolutionized within the last few years, but that revolution would not have occurred had we waited until we knew enough to have a revolution. If we are like the old sinner who "could not get converted because he was not fit to be converted," and who answered when told to get fit, that he "wasn't fit to get fit," we shall make no progress.

Only as we arouse medical educators to the necessity of this course in medical genetics, will we arouse the premedical schools to adopt a program to assure adequate preparation of their students for such training by giving them the fundamentals of the science of genetics. Although many medical schools assume that these are taught, that assumption is not always justified. The medical schools frequently do not require it, and the premedical education meets only requirements.

Only as we teach the students a course in pathological heredity at the end of their medical curriculum after they have acquired the fundamentals in their premedical work will we have teachers trained to bring illustrations into their teaching of Embryology, Anatomy, Physiology, Bio-chemistry, and the clinical subjects. The course in pathological heredity is not one which will be of mere scientific interest; it holds possibilities of being a great asset in preventive medicine and public health. For instance, an adequate appreciation of the importance of heredity in the development of cancer enormously simplifies the problem of detecting in time a vast majority of incipient cancer victims. For this reason, I strongly urge the adoption of a course of Medical Heredity in medical schools, so that we may have a medical personnel as conversant with the subject of inheritance in disease as they are with aseptic technique or with the surgeon's knife.



Smooth-Awned Wheat

Smooth-awned barleys have long been known. Recently varieties of barley have been developed which combine this desirable characteristic with other agronomic desiderata (Hayes, H. K., JOURNAL OF HEREDITY, October, 1926, 371-281). Smooth-awned forms of wheat have only recently been noted. These have agronomic possibilities similar to those of smooth-awned barley. That the smooth-awned characteristic is inherited as a recessive character is reported by S. J. Sigfusson (*Sci. Agri.* 13 (3): 185-193, 1932). The history of this form is discussed, and its possible mode of origin, perhaps by mutation.

DOMINANT VS. NON-DOMINANT GENES

In the Multiple Factor Hypothesis of Size Inheritance

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THE inheritance of most quantitative characters is commonly explained by what is known as the multiple factor hypothesis. The explanation, as set forth in our text books, begins by assuming that two races differing in respect to some quantitative character are to be crossed. Both races are considered to be homozygous for a number of genes, the larger race for dominant genes and the smaller race for the corresponding recessive genes, which are assumed to be without influence on size. The cross then may be represented as *AABBCC* .... by *aabbcc* .... There is the further assumption that the several genes represented by the large letters have each the same numerical value, thus giving the heterozygote, *Aa,* half the value of the homozygote *AA; Bb,* half the value of *BB* and so on. In this case the genes are usually called non-dominant. The offspring of this kind of a cross will, therefore, fall halfway between the parental races, which is a not uncommon experimental result of crossing races which differ in some quantitative character.

Although text books carefully point out that this explanation of the inheritance of quantitative characters is subject to certain minor qualifications, it is so good a statement of the general opinion of geneticists that we may safely proceed to consider whether or not it is sufficient to account for all known facts in regard to the inheritance of this class of characters. For convenience of discussion the word "size" will often be used in place of "quantitative characters."

The distinguishing features of the multiple factor hypothesis are these:

1. The genes (factors) are multiple.
2. Each gene represented by a large letter contributes a definite amount, or increment, to the size of the individual, and may be spoken of as a plus gene, but the genes represented by the small letters contribute nothing.
3. The effects of the plus genes are, generally speaking, cumulative.
4. The size of the individual is determined by the number of plus genes, the greater the number, the larger the individual.
5. The value of each plus gene is approximately the same as that of every other plus gene.
6. The heterozygote has half the numerical value of the homozygote.
7. From 5 and 6 it follows that the genes of the multiple factor hypothesis are unlike other genes since they are all uniformly non-dominant, whereas other genes show a variable dominance which is nearly or quite complete in a majority of cases.

This hypothesis of multiple non-dominant genes leads to the following consequences:

1. The F_1 of a cross between homozygous races differing in size comes exactly halfway between the parental races.
2. Matings between any male and any female in F_2 or later generations always produce offspring which average halfway between their parents, (allowance being made, of course, for sex differences in size when there are such).
3. Matings between individuals within a race which is not homozygous should also produce offspring which average halfway between their parents, where allowance is made for sex differences.
4. The phenotype and genotype are identical in numerical value.

When the consequences of the multiple factor hypothesis are compared with observation they fail to agree with it in several respects. These respects are:

1. F_1 of a cross between races differing in size is not always exactly intermediate, but may, as a whole, either approach the higher parent, the lower parent, or may exceed the high parent, Table I. Cases where F_1 is smaller than the small parent appear to

TABLE I

Table I.—Examples of instances in which the mean of F_1 is not exactly halfway between the parent races, but is above or below this point. The position of the mean of F_1 relative to both parental means is stated as a per cent of the amount by which the parental means differ, fifty per cent representing, of course, the offspring halfway between the parental means. In preparing this table allowance has been made for the influence of random sampling by excluding all instances—except three(*)—in which F_1 is less than sixty per cent and more than forty per cent of the difference between the parental races. Two of the exceptions to this rule indicate that the same character may behave differently with different stocks of the same species. While the data in this table serve the purpose of drawing attention to the point it is desired to illustrate, they were gathered with other ends in view, for which due allowance must be made. P_L=large parent; P_S=small parent.

Author	Date	Table	Species	Character	Mean P_L	Mean F_1	Mean P_S	P_L-P_S	F_1-P_S	F_1 as a percent of the difference between parental means
Punnett & Bailey	1914		Poultry	Body weight	1,350.00	1,230.00	800.00	550.00	430.00	78.2
Warren	1927	1		Body weight	2,315.80	2,120.90	1,537.40	778.40	583.50	75.0
		1		Shank length	89.69	79.94	77.88	11.77	2.06	17.5
	1924	Text	Drosophila	Egg weight	29.00	27.30	24.70	4.30	2.60	60.5
Jull	1931	1	Poultry	Body weight males	2,989.80	1,527.00	729.00	2,260.71	797.91	35.3
				Body weight females	3,037.98	1,305.00	545.00	2,492.98	760.00	30.5
Hayes		XL in Jones[8]	Tobacco	Number of leaves	26.50	23.60	19.10	7.40	4.50	60.8
		XLI in Jones[8]		Breadth of leaves	53.50	53.20	47.90	5.60	5.30	94.6
Russell	1919	XLVIII in Jones[8]	Sheep	Weight of fleece	9.50	10.00	7.20	2.30	2.80	121.7
					16.30	12.00	7.20	9.10	4.80	52.7*
Jones	1925	XXVII	Catalpa	Flower length	3-3.50	3-3.50	2.00	1-1.50	1-1.50	100.0
				Seed width	.7-.80	.3-.40	.2-.30	.5-.50	.1-.1	20.0
Green	1931	2	Mouse	Body length males	95.80	90.80	80.20	15.60	10.60	67.9
				Body length females	98.30	91.30	97.70	18.60	11.60	62.4
				Tail length males	93.70	85.70	68.80	24.90	16.90	67.9
				Tail length females	92.00	85.00	67.70	24.30	17.30	71.2
				Body weight males	31.40	22.00	15.50	15.90	6.50	40.9
East	1916	X in Jones[8]	Nicotiana	Corolla length	81.75	40.78	21.42	60.33	19.36	32.1
Emerson & East	1913	4	Maize	Number of rows	17.04	11.58	8.04	9.00	3.54	39.3
		5		Number of rows	12.01	11.36	8.04	3.97	3.32	83.6
		15		Ear length	16.80	12.10	6.60	10.20	5.50	53.9*
		17		Ear length	14.64	15.20	5.21	9.43	9.99	105.9
		28		Height of plant	101.18	94.53	68.22	32.96	26.31	79.8
		29		Height of plant	22.85	17.50	8.47	14.38	9.03	62.7
		30		Height of plant	21.27	19.91	11.32	9.95	8.59	86.3
		30		Height of plant	18.03	17.53	9.68	8.35	7.85	94.0
		31		Number of nodes	18.14	12.61	9.00	9.14	3.61	39.5
		31		Number of nodes	18.04	12.95	8.87	9.17	4.08	44.5*
		31		Number of nodes	21.27	12.95	8.87	12.40	4.08	32.9
		34		Internode length	105.03	112.36	72.92	32.11	39.44	122.8

be unknown. (This is a matter of considerable significance, because it limits the application of the theory of inhibitors in size inheritance).

2. The offspring of a mating between individuals of F_2 or later generations do not always average exactly halfway between their parents.
3. The offspring of a pair of individuals within a race which is not homozygous are often much more like one parent than the other.
4. Since the offspring on the non-dominant multiple factor hypothesis always average halfway between their parents, this theory is unable to explain hybrid vigor, in which the offspring exceed both parents in size.

The difficulties which the half dominant size gene theory encounters is well shown by the conflicting interpretations of inheritance in corn, which affords a most conspicuous example of hybrid vigor—commonly explained by dominant genes—yet where experiments in crossing races which differ in some quantitative character furnish equally conspicuous examples of inheritance which are referred to the classical multiple factor explanation. It is incredible that such contradictory explanations should both be applicable to the same form. It appears, therefore, that the usual explanation of the inheritance of quantitative characters needs to be modified if it is to fit all observations. On examination of the list of features of this explanation it appears that:

1. It is necessary to retain multiple genes.
2. The genes must be essentially cumulative.
3. Changing the relative values of genes, other than members of a pair, without further changes does not change the general consequences of this explanation.
4. The only change possible, then, is to modify the value of the homozygote relative to that of the heterozygote. Complete dominance of *A* may be assumed so making *AA* numerically equal to *Aa*.

To make *Aa* equal in value to *AA* appears at first sight to lead only to difficulties as great as those which beset the unmodified multiple factor hypothesis, and so it does if we assume that crosses between races of different sizes are crosses of a homozygous race *AABBCC,* etc., with a recessive race *aabbcc*. It is obvious under these circumstances that if we make the assumption of complete dominance of the plus genes, that F_1 should be exactly like the larger race in size, a result that is rarely if ever realized experimentally.

Heterozygosity of Parent Races

The fact that the experimental results fail to agree with the results expected from homozygous dominant size genes is not, however, proof that the genes are non-dominant because a mating of multiple heterozygous dominants with recessives also produces offspring which are intermediate between the parental stocks, and so the fact that the offspring are intermediate may merely mean that the size genes to which the larger race owes its greater size are heterozygous. This is the more probable because homozygous races are available for crossing only when close inbreeding has been practiced for a sufficiently long time. Even then the resulting race may not be homogeneous, but may be a mixture of homozygotes. In any case we may be quite sure that, with some exceptions, the races used in the crosses reported in the literature have not been homozygous, because most of the animal crosses and many of the plant crosses have been made between domestic races. While domestic races may be termed pure-bred by the uncritical, in reality they are rarely either homozygous or homogenous, but on the contrary are both heterozygous and heterogenous.

As Babcock and Clausen[1] state it, p. 230, "In the preceding chapter p. 218, it was shown that populations of allogamous species are highly heterogenous and that they consist largely of different combinations of highly heterozygous individuals." We must, therefore, reexamine the theory of size inheritance in the light of this condition, for it is quite clear that individuals from highly heterozygous races, being multiple heterozygotes, will give very different results when crossed, than would homozygous individuals. Of course the mere fact of heterozygosity is not proof of the inadequacy of the non-dominant gene theory because the average size of

the offspring will be halfway between their parents whether the genes are dominant or non-dominant. The difficulty with the hypothesis of non-dominant genes is that the offspring always average halfway between parents, regardless of whether they are homozygous or heterozygous. Since experience shows they are sometimes nearer one parent than the other, or may exceed the larger parent it is evident that the assumption of non-dominant size genes is insufficient to account for all observations. But before proceeding to consider the consequence of the hypothesis of dominant genes certain subjects of importance for our discussion need to be considered. We begin with dominance.

Dominance of Size Genes

Dominance of qualitative characters when complete, means that the appearance of the heterozygote is indistinguishable from the homozygote. Partial dominance means that the heterozygote can be distinguished more or less clearly from the homozygote. The conception of dominance is also adapted to the conception of inhibitors, by which is meant the ability of genes to suppress a character, such, for example, as the ability of the gene for the polled character in cattle to inhibit the development of horns. Dominance as applied to a pair of genes that are responsible for a quantitative character has much the same meaning as when applied to genes for a qualitative character, but there are some differences. If A is a gene producing a given effect on size and a its recessive allelomorph, complete dominance of A implies that individuals having simplex A are as large as duplex A, for example if $Aa = 2$, $AA = 2$ also. Partial dominance of size genes means that individuals possessing simplex A are not as large as duplex A. As for the numerical value of simplex A, considered as a partially dominant size increment, there seems to be no good reason why it should not have any value between zero and duplex A. However, it must be recognized that identification of instances in which the value of Aa is less than half the value of AA is not readily made even if such a pair of genes could be dealt with separately, because of the difficulty of distinguishing such an instance from a partial inhibitor. To be sure, an inhibitor is theoretically quite different from a plus gene, since it acts to suppress the effects of other genes, but as plus size genes must be an essential part of life itself, they are always present, thus affording the necessary basis for the action of inhibitors. Consequently there seems little hope of being able to distinguish between a plus gene of a low degree of dominance and an inhibitor of a strong but incomplete degree of inhibition.

Size genes, like other genes, are studied indirectly through their effect upon the organism, but with a few exceptions such as ear length in mice or height in peas they differ from qualitative genes in that they cannot be studied individually, because we have only the joint effect of all genes concerned by which they may be judged. We are forced, therefore, to draw conclusions of their nature and activities from genes which can be studied individually, and from the observed effects of the size genes themselves.

The relation of size genes to the size of the individual varies according to the assumption made regarding dominance. If the genes are assumed to be non-dominant, each plus gene adds an increment to the size of the individual and its size varies directly with the number of genes. But if the genes are assumed to be completely dominant, the size increment added by the homozygote is no greater than that added by the heterozygote, and so the size of the individual is not closely related to the number of size genes as it is in the case of non-dominant size genes. If all the size genes are in the heterozygous condition, the size of the individual varies with their number, but if all are homozygous, then the size of the individual is determined by half the number of plus genes. Between these limiting con-

ditions, there are of course, many intermediate stages.

Differences in size among individuals when dominant genes are concerned arise when the number of pairs of plus genes in either the homozygous or heterozygous condition in one individual exceed the number of such pairs in the other. This means that certain pairs of plus genes in the one are represented by recessives in the other. As any mating of a larger individual with a smaller one involves the mating of certain pairs of plus genes of one individual with recessive pairs in the other, it is necessary to consider whether the plus pairs are entirely homozyzous or heterozygous or a mixture of the two, for the results will quite clearly depend upon this fact. If all the plus pairs of the larger individual are homozygous then all the offspring will be like this parent in size while if all the plus pairs are heterozygous, the offspring will be variable with an average size halfway between the size of their parents. If part of the plus pairs are homozygous and part heterozygous, then the size of the children will be variable, with the average being nearer the larger parent in proportion to the number of homozygous plus genes.

In addition to these cases, there is also the case where two individuals produce children which are larger than either parent and which obviously results from the mating of individuals which possess different sets of dominant plus genes. There is also the case of heterozygous individuals of the same size and having the same genes where the children average less in size than either parent.

It is not likely that all size genes exert exactly the same effect on size. Some will be more powerful than others, but since size is a result of so many physiological activities, it is probable that most size genes do not differ much, one from the other, in their effect on size. If size genes are numerous, each gene as a consequence of number contributes only a small part to the total. Moreover in ordinary crosses between races, the differences are not sufficiently great to permit of large contributions from each gene even if they should, as a matter of fact, be relatively few in number. Genes that have a very large influence on size are not unknown, but they are readily recognized and can be handled individually.

Having considered dominance of size genes and their relation to size of the individual, let us next consider how a race may remain in a highly heterozygous condition. As shown by Hardy in 1908, an ordinary F_2 generation will maintain itself indefinitely under conditions of random matings and equal fertility. Thus with one pair of genes, *A, a,* one of every four individuals of generations later than the F_2 will be a homozygous dominant, two will be heterozygotes and one will be the recessive. If instead of one pair of genes we consider four pairs of genes, each individual on the average will have one pair of homozygous dominant genes, two pairs of heterozygous genes and one pair of homozygous recessive genes. That is, we can represent the four pairs of genes in four individuals in the following way.

AABbCcdd
aaBBCcDd
AabbCCDd
AaBbccDD

To be sure, not every individual will contain exactly this distribution of genes for the actual distribution of genes is given by the continued product of the ratios in which the contributing genes occur such as, for example, $(AA + 2Aa + aa)\ (BB + 2Bb + bb)$ $(CC + 2Cc + cc)\ (DD + 2Dd + dd)$, which when extended gives a small percentage of individuals completely homozygous, and a larger number that are heterozygous. When the genes are sufficiently numerous the frequency of occurance of individuals that are completely homozygous becomes so small that they are rarely encountered. Ordinarily only individuals that are heterozygous for the majority of their genes are to be met, although this is not in-

variably true even under the conditions stated here. However, genes may exist in other proportions by virtue of which a race remains in a state of multiple heterozygosis. In the foregoing scheme the gene *A* occurs with equal frequency with gene *a*. But *A* need not bear this ratio to *a*. There is a large series of ratios possible, each one of which is self-repeating. A few of these ratios are given in the following table.

If we examine the conclusion to which Table II leads, we see that it contains very much that is of importance for the theory of inheritance of quantitative characters.

This table is divided into three sections. In the first are shown successive ratios of *A* to *a*, in which the coefficient of *a* is always 1 and that of *A* beginning with 1 is the next higher number in successive ratios. Only a glance is needed to show that this part of the table leads directly away from multiple heterozygosis to a condition of multiple homozygosis of the dominant genes.

In the second section of this table the ratios are reversed and the coefficient of *A* is held constant while that of *a* increases by the next higher number. This section leads to a condition of multiple heterozygosis of the dominant genes. Under these conditions the size of the animal will be determined by heterozygous—rather than homozygous—genes in increasing proportion as the ratio of *a* to *A* increases. It is quite possible then to have races which fulfill the statement of Babcock and Clausen in regard to heterozygosity.

In the third section of this table will be found other ratios of *A* to *a*, which illustrate the wide variety of ratios possible, and the way in which they influence the proportion of homozygotes, heterozygotes, and recessives.

Genotypic and Phenotypic Distribution

A complete understanding of the significance of gene ratios is possible only when considered in the light of their influence upon the characteristics of the

Table II.—GENE RATIOS

Ratio of the Dominant Gene to its Recessive	Genotypic Ratio	Phenotypic Ratio: Dominants		Recessives
PART I				
The dominant gene occurs more frequently than the recessive.				
$1A : 1a$	$1AA+2Aa+1a$	3	:	1
$2A : 1a$	$4AA+4Aa+1a$	8	:	1
$3A : 1a$	$9AA+6Aa+1a$	15	:	1
$4A : 1a$	$16AA+8Aa+1a$	24	:	1
$nA : 1a$	$n^2AA+2nAa+1a$	(n^2+2n)	:	1
PART II				
The recessive gene occurs more frequently than the dominant.				
$1A : 1a$	$1AA+2Aa+1a$	3	:	1
$1A : 2a$	$1AA+4Aa+4a$	5	:	4
$1A : 3a$	$1AA+6Aa+9a$	7	:	9
$1A : 4a$	$1AA+8Aa+16a$	9	:	16
$1A : na$	$1AA+2nAa+n^2a$	$(1+2n)$	:	n^2
PART III				
A few other ratios				
$A : 0a$	All AA	all	:	zero
$0A : a$	all aa	zero	:	all
$2A : 3a$	$4AA+12Aa+9a$	16	:	9
$3A : 2a$	$9AA+12Aa+4a$	21	:	4
$4A : 3a$	$16AA+24Aa+9a$	40	:	9
$3A : 4a$	$9AA+24Aa+16a$	33	:	16
$2A : 5a$	$4AA+20Aa+25a$	24	:	25
$nA : n_1a$	$n^2AA+2nn_1Aa+n_1a$	(n^2+2nn_1)	:	n^2_1

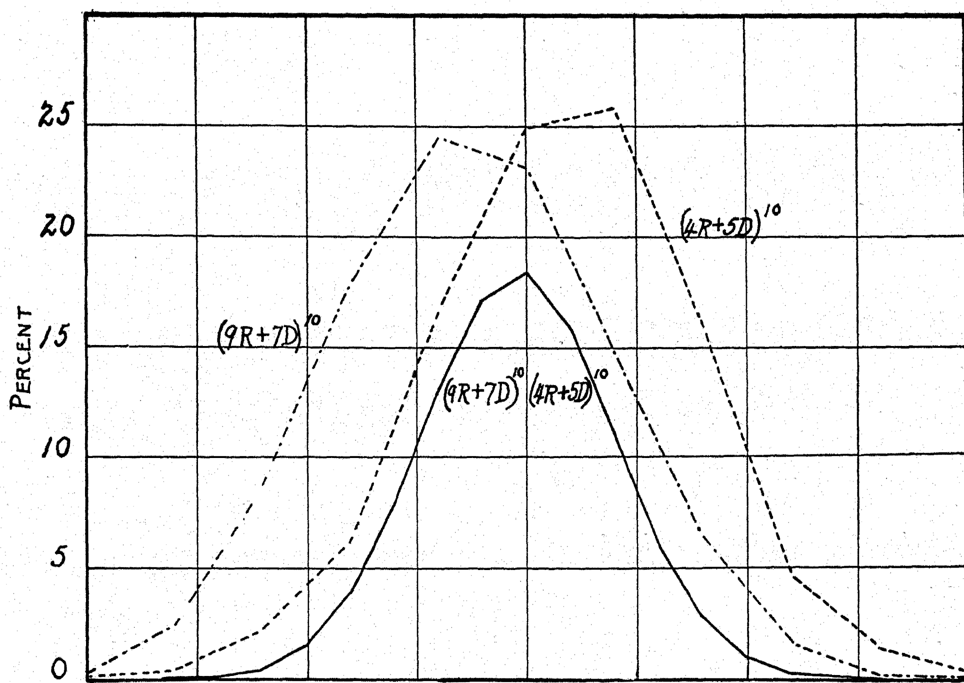

Figure 3

Curves of variation resulting from the independent assortment of phenotypic ratios corresponding to certain gene ratios. The dot and dash line represents the curve resulting from ten pairs of genes, each pair existing in the ratio of one dominant gene to three recessive genes (1A : 3a), with a phenotypic ratio of 9 recessives to 7 dominants. The broken line is produced by ten pairs of genes, each pair existing in the ratio of 1A : 2a, giving a phenotypic ratio of 4 recessives to 5 dominants. The continuous line is formed by combining the two ratios on which the other two lines are based.

population which result from their activities as expressed in the usual curve of variation. Since the phenotypic composition of a population of a quantitative character is given by multiplying together algebraically the phenotypic ratios of the pairs of contributing genes, such as $(3D + 1R)$ $(8D + 1R)$ $(5D + 4R)$ $(7D + 9R)$, etc., where D is the phenotypic dominant, and R the recessive, the coefficients of the terms of an expanded expression give the resulting curve of variation, whose shape obviously depends upon the contributing phenotypic ratios, D to R. Almost any kind of a curve can be produced by making suitable choice among the many ratios available. Phenotypic ratios of the form D equals R, result in curves which are normal or nearly so. The ratio 3D : $1R$, when only a few pairs of genes are involved, gives a shed roof distribution, which changes with increasing number of genes, as Collins[2] has shown, to a curve of fairly normal shape. A normal or nearly normal curve may be obtained by selecting the ratios in such a way that the dominants are balanced against the recessives. Asymmetrical curves of various kinds can be had by selecting suitable ratios.

Three curves of variation based upon comparatively simple gene ratios are given in Figure 3. The first, indicated by the broken line, is formed by the expansion of the phenotypic ratio $(5D + 4R)^{10}$ corresponding to a gene ratio of $1A : 2a$. The expansion of this formula gives the distribution of the

number of dominant size increments per individual and so the distribution of their size, but as the expanded distribution—if plotted from the formula as stated—would bring the larger individuals to the right of the chart and the smaller ones to the left—exactly opposite the customary method of plotting a curve of variation—it is more convenient to write the formula $(4R + 5D)^{10}$ which brings the small individuals with few size increments to the left of the chart, and the large individuals with many size increments to its right.

The curve of variation shown by the broken line is formed by the expansion of the phenotypic ratio of $(9R + 7D)^{10}$ while the curve shown by the continuous line is formed by multiplying $(9R + 7D)^{10}$ by $(4R + 5D)^{10}$. The coefficients of the terms of each expanded expression were reduced to a percentage basis before the curves were plotted.

Results of Crosses

Having discussed the peculiarities of dominant size genes, we can, with the aid of the ratios given in the table, or which may be deduced therefrom, together with the characteristics of populations, understand the varied results which are obtained when individuals of different sizes are crossed. The case which is the foundation of the non-dominant gene hypothesis, where F_1 is exactly halfway between the parents, comes first. If the differential size genes are practically all heterozygous, they are necessarily represented in the smaller race by homozygous recessives. The cross then will represent a mating of multiple heterozygotes with recessives and the average size of the offspring will be halfway between their parents. F_1 will be variable, which as a matter of fact, is quite often the actual experimental result.

An F_1 whose average size is halfway between the parental means may be the result of non-dominant size genes or it may be the result of dominant size genes which are heterozygous. But an F_1 which is nearer one parent race than the other can be explained by non-dominant size genes only when some of the individuals used as parents in the experiments are so far removed from the racial mean that they cannot be regarded as suitable representatives of it. For example an exceptionally large individual from the larger race mated to an average representative of the smaller race would produce offspring in size halfway between their own size, but which would be considerably above the middle point of the difference between the racial means. Or a mating may be made between an exceptionally small individual of the smaller race with an average individual of the larger race, which would then produce offspring whose average size is below the middle point. But instances in which the offspring are not exactly halfway between the parent races are so numerous that they cannot all be accounted for by these suppositions. And so it is necessary to turn to dominant genes to see if through their use a satisfactory explanation may be developed for these instances.

Taking first the case where F_1 is nearer the larger parent, it is apparent that it can be satisfactorily accounted for by several ratios in Table II, of which the second ratio in Part I of this table is a good example. The genes exist in the proportion of $4AA:4Aa:1aa$. Since the larger race possesses more of the dominant genes than the small one it follows that on the average when the races are crossed one homozygous dominant pair of genes meets a pair of recessives, while one pair of heterozygous genes is also meeting recessives. This can be represented by *AABb* meeting *aabb*. The children are *AaBb* and *Aabb*, thus averaging nearer the higher parent than exactly halfway, because of the influence of the homozygous pair of genes, *AA*, which by itself takes the children to their own level, while the *Bb* pair of genes takes the children only half way to its level. The combined effect of the two pairs of genes is to produce offspring which average three fourths of the dif-

ference between the parents above the lower parent's size.

The second case, where F_1 is nearer the lower parents, rather than nearer the higher parent, may be explained in the same manner except that the dominant genes must now be regarded as inhibitors.

Partial Heterosis

Another instance with which the text book explanation of size inheritance involving the hypothesis of non-dominant genes is wholly incompatable, is the dominant gene theory of heterosis, because non-dominant genes plainly cannot account for hybrid vigor even though two races owe their size difference to different sets of plus genes, since non-dominant genes inevitably bring the size of the offspring halfway between that of their parents. Consider such a mating as *AABBCC* with *EEFFGGHHIIJJ*. The first race contributes genes *ABC*. The second contributes genes *EFGHIJ*. Give each gene a value of 1. Then the small race *AABBCC* has a value of 6 and the large race *EEFFGGHHIIJJ* a value of 12. The cross bred offspring has a value of 9 which is exactly halfway between the parents. The only exception to this rule is when non-dominant genes are also complementary, but such instances are probably not common.

We cannot leave this subject of hybrid vigor without pointing out an inconsistency which has crept into its use. Hybrid vigor refers to the fact that the offspring of a cross possess a degree of qualities greater than that possessed by either parent, particularly such qualities as larger size, greater longevity and so on. The meaning of the term has been extended, under the designation of partial heterosis, to include cases in which offspring intermediate between parents, resemble one parent more than the other. It is very difficult, however, to understand why a first generation cross which fails to equal the large parent's size—or other characteristic—should be credited with any degree of hybrid vigor, seeing that it is actually smaller than the larger parent. The fact is that as far as size is concerned, there is no hybrid vigor, for true hybrid vigor exists only when the vigor of the hybrid exceeds that of the larger or better parent.

The development of the conception of partial heterosis has undoubtedly resulted from the necessity of accounting for those instances in which F_1 fails to come exactly halfway between the parents, as is required by the non-dominant gene theory, especially those cases in which F_1 is nearer the larger parent. Some explanation is needed to account for the discrepancy between expectation and fact and so resort has been made to the mystical conception of physiological stimulus which is assumed to step up the vigor of the hybrid beyond that expected on the basis of non-dominant size genes. While it may be true that we can account for the phenomena known as partial heterosis by assuming that it is brought about indirectly by the activities of other genes than those immediately concerned with the character in question, we are obliged in the last analysis to determine how these other genes, which provide the physiological stimulus, acquire their potency. This assumption, which lies outside known genetic principles, is not necessary because a satisfactory explanation for the phenomena which leads to it is found in multiple dominant size genes, part of which are homozygous and part heterozygous.

In indicating that many of the recorded observations on size inheritance require dominant rather than non-dominant size genes, it is not intended to imply that all observations are to be explained in this manner. There is no reason to conclude that non-dominant genes are excluded from further consideration in particular cases, or that Babcock and Clausen's suggestion that the intermediacy of F_1 is due to a mixture of dominants, some of which increase size and some of which reduce size (inhibitors), may not apply in

other cases. But there is good reason to re-examine the whole question of size inheritance in order to determine how far non-dominant, partially dominant and completely dominant plus genes and inhibitors are operative. For this purpose genetics is not without means of determining these facts much more fully than has been done. Much may be accomplished by the linkage studies which are now so popular. Among other methods the crossing of races may still be useful, though it cannot be counted upon to add very much to our present knowledge, except when each of the races or strains crossed is homozygous, because only then is the race correctly represented by the individual. If a cross is made between species which are mixtures of homozygous races, the results apply only to the strains used and not to the species as a whole. But homozygous strains or races form only a portion of the material to be investigated. When heterozygous races are under investigation the best that can be done is to cross representatives of the races, and the results of the experiment must be interpreted in the light of the mating of the individuals selected for the experiment. The use of racial means as a measure of individual performance is justified only as a first crude approximation to the facts.

The Progeny Test in Studying Size

There is, however, a method of research which promises to yield much valuable information upon size inheritance. This is the method of analysis of the genetic constitution of individuals, in principle no different from the determination of the genetic constitution of an individual where qualitative characters are concerned but adapted to the peculiarities of quantitative characters. As it will not be possible in most cases to use the convenient method of determining the genetic constitution of individuals under test by mating each to multiple homozygous recessives it is necessary to adopt the plan of mating each individual to a variety of other individuals in number sufficient to thoroughly explore its possibilities. The result of such exploration gives a very practical and useful knowledge of the genetic nature of each individual tested, which can, if desired, be stated in a single term much as size itself is stated. While this is not the place to enlarge upon the details of the method it is clear that individuals possessing more plus genes will give quite different results when thus tested than will individuals that possess fewer plus genes. Also an indication of the relative homozygosity of the individuals tested can be determined from a consideration of the character of their progeny relative to the character of their several mates. Thus an individual that is relatively homozygous for plus dominant genes will have no offspring below a certain size, although its mates vary much in size. The relative homozygosity may also be determined by use of the coefficient of correlation, because the value of the coefficient varies inversely with the degree of homozygosity of the individual under test. The coefficient of correlation between the size of each mate and the average of its children will be relatively small,—zero in the case of an individual completely homozygous in all its size genes, since its children are all alike while its mates are varied—as compared with the offspring of an individual with few plus genes where the correlation between the size of each of its mates and the average size of the children of each mate will be high, since most—in the case of the multiple recessive all—of the children's inheritance is derived from the mates.

Along with the method of individual genetic analysis additional information on size inheritance may be gained through the development of strains by inbreeding or by selection based upon progeny testing. For the latter purpose the determination of the genetic constitution of individuals is a very valuable if not indispensable aid. The character of the races thus developed, together with their behavior when mated

among each other should throw further light on the character of the genes involved.

The development of methods for the exploration of the genetic constitution of individuals for quantitative characters promises not only to enlarge our knowledge of this branch of genetics but should be of the greatest benefit to agriculture through improvement in the economic characters of plants and animals.

Summary

1. The current explanation of size inheritance by means of non-dominant genes is inadequate to explain all observations.

2. The observations which non-dominant size genes do not explain, can be explained by dominant genes, which exist in various degrees of heterozygosity.

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Cyto-Genetics in 1932

RECENT ADVANCES IN CYTOLOGY, *by* C. D. DARLINGTON. Pp. xviii + 559. 109 text figures, 8 plates. Price, $4.00. P. Blakiston's Sons & Co., Inc., Philadelphia. 1932.

TO THE average biologist the title of the book is misleading, for it treats almost solely of chromosomes and their behavior. The author states in the preface that the purpose of the book is threefold: (1) an introduction for the student who is a beginner, (2) a résumé for the research worker who requires classified observations, and (3) a theoretical treatise in which the author's own views are developed.

Concerning the first purpose, it is difficult for the reviewer to consider the book as an introduction to the subject of karyology, for the presentation is not sufficiently lucid and concise. Nevertheless, it should be highly stimulating to a beginning student when used in conjunction with a more elementary text.

Throughout the work the second purpose, namely, that of classifying observations for the research worker, is emphasized. This applies even to the method of presenting the subject matter. The author tabulates many data on chromosome types and behavior in an effort to organize the descriptions given by many authors. Since the analysis of chromosome behavior by different authors, particularly with reference to abnormal situations, is frequently incomplete, inconclusive or of a fragmentary observational nature, conclusions from such tabulations must be cautiously accepted. Although the value of some of the tabular material is thus inevitably diminished, the well

informed investigator can derive much benefit from such an organization.

To the reviewer, the main value of the book lies in the third purpose—a theoretical treatise in which the author's views are developed. In this phase of the work the cytologist is frequently jolted into a frame of mind which undoubtedly will color his future investigations in an effort either to refute or to confirm the author's contentions. The method of presentation, although in part unintentional with the author, continually centers the attention of the reader on the inadequacies of our present knowledge of the fundamental composition of the chromosomes and, to some extent, of their behavior. There are too many unproven postulates. By such a procedure, however, a clearer view of the present state of the subject is made possible and a definite direction for future investigation is indicated. The theoretical postulates in the field in which the author has carried on his investigations and centered his thoughts are admirable and provocative. Unfortunately, the author has a tendency to dismiss evidence which, due to his lack of acquaintance with the material, appears inconsequential to him.

The first two chapters deal with the nature of the chromosome, its size, structure, division and potential permanence. The next chapter is devoted to genetic and structural modifications which affect individual chromosomes or the entire chromosomal complement. The following chapter deals with meiosis in diploids and polyploids. It is here that the fundamental assumptions and arguments necessary for the elaborations in the following chapters are presented. Mainly on the basis of the assumptions that pairing is due to a 2-by-2 attraction of homologous parts of unsplit chromosomes and that chiasma formation is the product of crossing over and is solely responsible for continued association of chromosomes in late prophase and metaphase, the behavior of chromosomes in various types of hybrids and polyploids is analysed. In the several chapters devoted to polyploidy and hybridity, the determinable types of nuclear changes associated with modification in the appearance and behavior of the organisms are considered with regard to their evolutionary significance, *i. e.*, the origin and constancy of new types. Several chapters are devoted to a discussion of the author's theories regarding the cause of pairing of homologous chromosomes at meiosis, the time and nature of crossing over, the formation of visible chiasmata and the meaning of the reduction in number of chiasmata from mid-prophase to metaphase. These views of meiotic behavior serve as a basis for the presentation and interpretation of many of the yet unsolved and insufficiently understood or examined phases of cytological investigation. When called for, subsidiary hypotheses are introduced in order that an explanation may not be lacking. Whether or not these interpretations will hold in the light of future investigations, they serve at present to coördinate and illuminate much that has been overlooked in previous investigations. No student of cyto-genetics can fail to derive stimulation and much valuable information from this presentation.

The author's desire for a unification of our knowledge of karyology goes beyond the range of tabulated observations for their own sake or for that of some particular theory and culminates in the last chapter with a treatise on the evolution of genetic systems such as the origin and differentiation of the chromosome, chromosome complement and the meiotic process, the genetic and cytological changes responsible for sexual differentiation and the part hybridity has played in the development and permanence of species. Emphasis is placed upon the view that variations of evolutionary significance have their origin in single effective changes which arise in advance of the use to which they are put.

The book possesses a valuable glossary and biblography.

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TWIN RESEARCH IN HOLLAND

Dr. C. Ph. Schokking. Extension of Twin Research in the Netherlands; Leiden P. J. Mulder and Sons, 1931.

Dr. A. M. Legras. Psychosis and Criminality by Twins; Utrecht, Kimink and Sons, 1931.

WITHIN a year's time there have been published in Holland two theses on the heredity of twins. That so distinguished a student of this subject as H. W. Siemens has become professor of Dermatology at the University of Leiden undoubtedly partly accounts for this. The author of one of these theses is his pupil and assistant, Dr. C. Ph. Schokking. The author of the other is assistant to Dr. L. Bouman, Professor of Psychiatry and Neurology at the University of Utrecht.

Schokking's thesis, *Extension of Twin Research in the Netherlands,* is based on his investigation at Leiden, of 91 pairs of one-egg twins and of 95 pairs of same-sexed two-egg twins. For that purpose all characteristics of the examined persons were recorded, from which data it was later determined by comparison how great was the influence of heredity. In this manner were treated successively cutaneous affections, defects of the eyes, internal abnormalities and infectious diseases, defects of the bones and muscles and of the external build of the body, and evidence of asymmetry. The results obtained were added to those of other investigators, especially to those of Siemens and von Verschuer, to increase the number of cases available as a basis for conclusions. When we consider that the author of the thesis specialized in cutaneous diseases, it will cause small wonder that these diseases have received a lion's share of attention. No less than 59 of the 106 pages that contain the results of his research have been given over to these affections of the skin. Through this fact the book suffers from a certain lack of equilibrium. The psychic characteristics and defects have not been treated at all and others have received but casual attention. But in spite of those facts many very important conclusions can be gathered from this investigation. The following table summarizes the information with regard to the similarities and differences of twins, in those instances in which Schokking was able to obtain enough twin pairs to render the data significant or suggestive (see Table 1).

This table, which gives chiefly the results of the minute research of Schokking, shows sufficiently why this thesis is an important addition to the literature on twins. Schokking has reached the following conclusions: Acrocyanosis and cutis marmorata are but little effected by the environment. Teleangiectases of the cheek, interscapulae, and back of the head are preponderatingly determined by heredity. This is also the case with the angioma senilis, while naevi aranei are more determined by external conditions. Acne vulgaris is an hereditary disease (probably depending on two or more genes), which is also the case with the miliae; but these are more variable in expression. Keratosis umbilicis, too, seems to be very much dependent on the hereditary factors; the striae cutis distensae is on the contrary much more influenced by environment. The naevi are independent of hereditary factors. The occurrence of eczema is based on an idiodisposition, likewise that of eczema folliculari and of verrucae vulgares. Strabismus convergens appears to be largely independent of hereditary factors. The defraction of the eyes on the other hand is controlled largely by an hereditary component. A distinct tendency towards rickets appears to exist, while for tuberculosis and the acute infectious diseases, the evidence of an hereditary basis is much less conclusive. Left handedness seems, according to these data, to be to a certain degree dependent on heredity.

That these researches of Schokking have been guided by Siemens is shown by the many quotations from the books of the latter. It is to be hoped that other pupils will follow Schokking's example, and that Siemens himself will again take a more active part in research on twins, and continue at Leiden the very fruitful investigations he began at Munich.

Psychoses in Twins

Legras's thesis is entitled *Psychosis and Criminality in Twins.* As yet only a few psychiatrists have occupied themselves with the occurrence of psychoses in twins. Ruedin, of Munich, and his pupils, Lange and Luxemburger, have done the chief research in this field. Legras collected his material not only from the psychiatric clinic at Utrecht, but also from Dutch lunatic asylums, penitentiaries, reformatories, and government educational institutions.

Before beginning a report of his own investigation, Legras devotes several chapters to a review of other research on twins. Publications in this field have

TABLE I—Similarities and Differences of Twins: ++=Concordant. +0=Discordant. +(+)=Similar But With a Little Difference

Diagnosis	One Egg			Two Egg		
	++	+(+)	+0	++	+(+)	+0
1. Acne vulgaris				3	4	8
2. Acrocyanosis	10			3	2	4
3. Albinotic fundi	4			1		
4. Angioma senillis	3	1	2		1	
5. Asymetry of the face	9		6			
6. Blepharitis squamosa	8		1			
7. Bronchitis	5	2	1			
8. Conjunctivitis follicularis	2					
9. Deviation of the septum	3					
10. Diphtheritis	2		3	4		3
11. Eczema erythemate-squamosum	20		8	10	6	38
12. Eczema folliculare	11		1			4
13. Eczema of the lips	12			2	3	9
14. Enuresis nocturna	5		4	5		10
15. Exophory	2	1	2	1		2
16. Hyperthely	2		4	1	1	4
17. Impetigo contagiosa	2		1	1		3
18. Keratosis umbilicalis	5					2
19. Lefthandedness	3		3			1
20. Lentigines in the face	5			3	3	3
21. Leuconychy	4		11	2		10
22. Miliae	7		6	8		15
23. Morbilli	44		1	44		3
24. Naevi aranei	2		9	4		6
25. Naevus pigmentosus			7			3
26. Naevus vasculosis			1			6
27. Partil heterochrony	1		6			6
28. Pityriasis capitis	8			4		4
29. Pneumonia	3		6			6
30. Rachitis	12		4	9		7
31. Scarlatina	7		3	4		9
32. Scoliosis	4					
33. Speech defects	4		4	1		9
34. Striae cutis distensae	2		3		1	
35. Tabled fundi	23		2	11	8	15
36. Teleangiectases interscapulae	6			3	9	8
37. Teleangiectases of the cheek	34		1	9	10	28
38. Teleangiectases of Unna	3	1	1			2
39. Teleangiectases, other	10		1	11		0
40. Tuberculosis						4
41. Tussis quinta	6		2			21
42. Varicellae	2		4	1		3
43. Vegetationes adencideae	4		0	13		1
44. Verrucae vulgari	3			1		1

increased greatly since Siemens' summary in 1924. Some of Legras' statements in this part of the book are rather uncritical, and are not supported by sufficient evidence. His attempt to test Newman's view that each left-handed individual is the survivor of a pair of twins of which the right-handed half has been lost, is entirely inadequate. He did this by asking left-handed persons if twins occurred in their family. This method of approach, while having possibilities, must be undertaken on a much larger scale if any conclusions are to be reached.

Similarly his explanation of the fact that more twins are born in Germany and in Russia, and more in the country than in cities, is distinctly näive:

"A woman who once lived through a gravidity of twins and in this way saw the family grow rapidly, would not stand the risk of twins again. And now it is just in the cities and in France that birth control is practised."

Of course this is no explanation, for the difference in the percentage of twin births, compared with all births, is not explained by it. It is an ascertained fact that among the first born, twins arê not so frequent as among the later born; the reason of this is unknown. In France and in the cities, by means of birth control, the percentage of first born is larger than in Germany and Russia. It follows that the percentage twin-births must be smaller in the first mentioned regions.

The surplus of masculine births, Legras thinks to explain by simply declaring that we can assume the fact that nearly all parents wish for a son, parents with two sons are content; parents with two daughters still hope for a son. According to Legras, it must be clear that in this way the number of male births must dominate. This so-called explanation is wholly inadequate, for it does not explain the surplus of males among the first-born.

These defects can, however, readily be overlooked because of the extraordinary interest of the material which Legras presents on psychic abnormalities in twins. Legras examined twenty pairs of identical twins suffering from various nervous diseases and four pairs of criminal identical twins. As a control, he gives brief histories of nineteen pairs of non-identical twins, of which one of each pair has been confined to an institution, and of five pairs of two-egg twins, of which one of each pair had been convicted of a criminal offense. The following table summarizes his results:

TABLE II—Occurrence of Mental Disease in Identical and Fraternal Twins

IDENTICAL TWINS

Number of cases in which twins were:	*concordant*	*discordant*
Schizophrenic psychoses (Dementia Praecox)	6	
Manic-depressive psychoses	2	
Idiocy	3	
Epilepsy	2	1
Psychastheny	1	
Suicide	1	
Criminality	4	
Sclerosis multiplex (sclerose en plaques)	1	

TWO-EGG TWINS

Number of cases in which twins were:	*concordant*	*discordant*
Schizophrenic psychoses		9
Degenerative psychoses		1
Melancholic psychoses		1
Epilepsy		1
Hemiathosis post encephalitica		1
Idioty post encephalitica		1
Imbecilitas		2
Mongoloid idiocy		2
Hysteria		1
Criminality		5

A detailed case history of each of these identical twins is given as well as the brief summaries of the fraternal twins. From these data, Legras reaches the following conclusions:

1. Many of the symptoms of the schizophrenic psychoses are determined by heredity. External influences play little part in the history of these cases, except perhaps in affecting the gravity of the symptoms. If one bears in mind that 80 per cent of all inmates of institutions for mental diseases are schizophrenics, it is clear that for a great majority of lunatics, the histories of their

lives have had little influence on their present condition. Furthermore, it appears that the various forms and types of this disease, also have a genetical basis.

2. The manic-depressive psychoses are also little influenced by the history of the individual. Environmental influences cannot be shown to have much effect. One case studied by Legras, confirms the report of Lange, that mania and melancholia, have a similar genetic basis and ought not to be separated as advised by several authors.

3. On the basis of the literature on the subject and Lange's cases, one can say that idiocy is determined more by the hereditary constitution, than by such factors as alcoholism and syphillis. The psychoanalytic factors have little more than a pathological interest.

4. The four criminal cases are not so convincing as they might be because the twins did not grow up separately. When, however, we compare them with the five pairs of non-identical twins, one of whom was a criminal, the contrast is quite striking. In these cases, although the twins grew up together, all five are discordant and the other twins, in each instance, have not been convicted of crime. These cases when added to those reported by Lange indicate that while criminality could hardly be called a gene determined characteristic, it has, nevertheless, a definite hereditary basis.

5. The single case of identical twins, both with multiple sclerosis, is extremely suggestive. It is true that identical twins would possess the same susceptibility to infectious diseases and we might expect inflammation, induced by such diseases, to run the same course in identical twins. The fact, however, that multiple sclerosis occurs only in a few individuals and that it has a clearly defined and very characteristic course, militates against such a view. Thus, multiple sclerosis with its associated symptoms, a very characteristic squinting of the eye, rambling talk, gradual disappearance of reflexes, and defects of the viscera suggests a rather highly centralized chain of events. That we should attribute these to a diffuse process of inflammation as has been done before seems questionable. This case is of considerable interest and it is hoped it will lead to further research on the heredity of this condition.

Though the number of cases dealing with identical twins discussed by Legras is not large, the results are of considerable value, as the cases have all been accurately and exhaustively examined physically, physiologically and psychologically. The psychiatrical study of the identical twins shows only a few differences in their phychoses. This is true also of the criminal twins. The non-identical twins show no such close similarity. Legras' studies are further evidence that heredity has a very important place in the causation of mental disease and of criminal tendencies.

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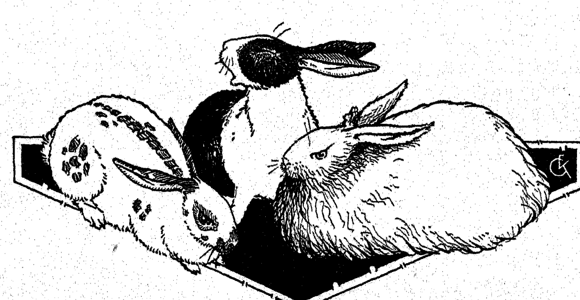
Rotterdam, Holland.



NEMATODE RESISTANCE OF PINEAPPLES

Varietal Resistance of Pineapple Roots to the Nematode *Heterodera Radicicola* (Greef) Muller

J. L. Collins and H. R. Hagan
University of Hawaii

(*Concluded From the November Number*)

Comparison of Nematode Infestation on Different Varieties

GALLS on the roots are divided into two classes, terminal and non-terminal, and each class is treated separately. The latter class indicates roots that were able to continue growth after being infested, on the assumption that original infestation was at the tip of the root. The terminal gall class includes those roots the growth of which was stopped by the formation of a gall at the root tip.

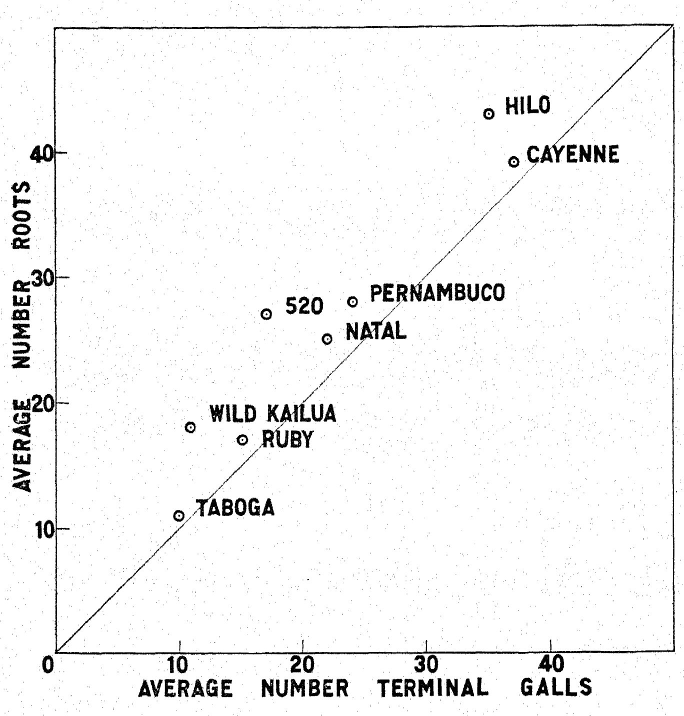
As stated in the description of methods, records of the galls present on the fine lateral rootlets were taken but have not been considered in this analysis, for two reasons. The infestation was found to be quite uniformly low on all varieties, and no measurement was made of the volume of these fine roots. The number of galls observed on these roots would to some extent depend upon the number of the roots present.

The infested varieties in Table III show distinct differences in the average number of roots produced as do the check varieties in Table II. These fall into three groups: Hilo and Cayenne with a large number of roots; Pernambuco, Lot 520 and Natal with slightly smaller numbers and finally Wild Kailua, Ruby and Taboga with the fewest roots.

The number of terminal and non-terminal galls per plant exhibits a similar classification with the notable exception that Lot 520 has a low number of terminal galls as compared with Pernambuco and Natal.

The average number of galls per root (Column 6) shows, apparently, only slight differences between varieties. However, in Column 7, a very different and significant picture is presented. Lot 520 and Pernambuco plants have 34 per cent and 29 per cent, respectively, of their roots free from galls while Taboga and Cayenne have 3.5 per cent and 4.7 per cent of their roots free. The similarity of the number of galls per root in Column 6 is only apparent due to the fact that some of the varieties, notably Lot 520 and Pernambuco, frequently have several galls on a single root, indicating an ability to continue growth after an initial infection. Varieties with a lower percentage of gall-free roots possess this trait to a lesser degree, hence the galls are distributed over a larger proportion of the total number of roots.

The varietal differences in terminal gall infestation is shown graphically in Figure 4, which gives the number of galls per plant in relation to the number of roots per plant. If every root exhibited a terminal gall the variety would be plotted on the diagonal line indicating 100 per cent infestation. The farther away from this line a variety is plotted, the greater the percentage of gall-free roots. This diagram shows Taboga, Ruby and Cayenne with approximately the same low percentage of gall-free roots. Natal and Pernambuco are slightly less infested than the three varieties mentioned. Hilo, Wild Kailua and Lot 520, on the other hand, are farther away from the line, thus revealing a larger percentage of gall-free roots.



VARIETIES DIFFER IN SUSCEPTIBILITY

Figure 4

Graphical representation of varietal differences in terminal gall infestation and in number of roots per plant. (Data from Table 3.)

Figure 4 also shows varietal differences in average numbers of roots per plant. The position of the variety with respect to the horizontal base line is determined by the averaged number of roots, those farther away having a larger number of roots per plant than those varieties closer to the line.

Table IV has been arranged to show the difference in the percentage of infestation of each variety with every other variety. The figures in the squares represent the difference in the percentage of infestation between the variety shown at the top of the column and the one at the left of the row in which a particular square occurs. The variety exhibiting the greater percentage of infested roots is indicated by an arrow in the square beneath the number. Thus, in the case of the difference between Cayenne and Hilo, Cayenne shows 10.7 greater percentage of infestation than does Hilo. The percentage of infestation in Natal is 20.7 greater than in Lot 520. It will be noticed that all the arrows point away from Lot 520 which has a lower percentage of infested roots than any of the other varieties, while all the arrows point toward Taboga which has the largest percentage of infested roots.

Comparison of Root System Development of Check and Infested Plants in Each Variety

In previous sections the differences between varieties have been considered. This section will be concerned with the average difference between check and infested plants of the same variety. An attempt will be made to express quantitatively the amount of damage which nematodes cause to root development within each of the several varieties.

It is clearly demonstrated by the figures in Table V, Column 3, that the number of roots produced by a plant, of the age dealt with in these experiments, is not influenced by the presence of heavy nematode infestation. A difference to be significant must be greater than four times its probable error. In no variety is there a significant differ-

ence in number of roots between check and infested plants.

In the matter of average length of root, however, a different picture is presented in Column 4. In all varieties except Lot 520, a significant difference is shown between mean length of roots of check and infested plants. In Lot 520, the $D/E = 0.93$ which indicates that in this variety there is no difference between length of roots of check and infested plants. In Column 5, this reduction is expressed as a percentage of the length of the roots of the check plants and makes possible a ready comparison of the amount of damage done to the roots of the different varieties. In other words, it shows the relative resistance of the varieties which is approximately the same as indicated by the determination of the number of galls per variety shown in Figure 4.

The percentage reduction in length of roots was determined in the following manner: Percentage reduction $= 100 \times \cdot \frac{C - I}{C}$ in which C is the mean length of the roots of the check plants and I the mean length of the roots of the infested plants of the same variety.

The classification of varieties, according to degree of resistance to be given later, is based upon this percentage reduction in root length due to influence of nematodes.

The question might justly be raised whether the influence of nematodes upon plants grown in pots would be typical of their effects on plants grown under field conditions. Godfrey[9a] obtained root measurements from three Cayenne plants growing in a nematode infested field and from three in an adjacent row where the soil had been treated with CS_2 to kill the nematodes. These plants in the treated soil did, however, exhibit a few galls. When the measurements were subjected to the same analysis used for the pot grown plants comparable results were obtained for average number of roots, average length of roots and percentage reduction in root length due to presence of nematodes. Also, part of the work of Morgan and Peters[21] was organized to learn whether a correlation existed between the appearance of potato plants and the *Heterodera schachtii* cyst population of field soils. No attempt was made to secure an initial, large, uniform population in the soil, nor were check plots used. The plants observed were classed as "good," "fair," and "poor." The first test showed no differences but the second gave a definite correlation between cyst counts and appearance of plant when "good" plants were compared with "poor."

In Table VI we have still further analyzed the differences between check and infested plants by dividing the roots into two classes; those over five inches in length have been indicated as long roots while those five inches or less in length have been considered as short roots. In the last column the percentage of old roots has been shown. This term was applied to those which had a brown, somewhat dried, outer covering, with lateral roots, and with the area of soft white tissue limited to the extreme tip. In many cases the tip of the root was decayed or absent. Old roots may not be an apt term for this class but it serves to distinguish certain roots from the frequently shorter, soft, white roots with almost no laterals which gave the appearance of being younger ones.

Old roots were usually longer than the new roots but the classifications of long and of old roots in the last two columns are not identical. This is due to the fact that old roots were classified on the basis of appearance regardless of length. Long roots, on the other hand, is an arbitrary classification which includes all roots more than five inches in length.

The percentage of old roots is greater in check plants in all varieties except Taboga and Wild Kailua. No explanation can be offered at this time for the appearance of more old roots in the checks inasmuch as the number of roots is not influenced by nematode infestation. The percentage of long roots is

greater in the checks. This substantiates our previous statement concerning the retarding effect of nematodes upon root growth. The slight difference in favor of Lot 520 checks in Table VI probably is not significant in view of the analysis given in Table III showing the absence of difference in root length between check and infested plants of this hybrid.

A very high proportion of the short roots of inoculated plants had become infested but there is a pronounced difference among the varieties in this regard. Lot 520, Pernambuco and Wild Kailua appear more resistant with 69 per cent, 71 per cent and 77 per cent, respectively, of their short roots infested as contrasted with Taboga 93 per cent, Cayenne 91 per cent and Natal with 88 per cent of the short roots infested with nematodes.

A consideration of Columns 2 and 4 shows that nematodes cause a slightly greater average reduction in the long than in the short roots. The difference between checks and infested plants in regard to mean root length is due, however, to the relative numbers of long and short roots in the two classes of plants. In Lot 520, the number of long roots is equal in checks and infested plants and the number of short roots is also equal in the two classes. This indicates a high degree of resistance in this hybrid. In contrast to this condition Cayenne and Taboga appear as very susceptible varieties with twice as many long roots in the check as in the inoculated plants, while for the short roots there are twice as many in the infested as in the checks. Other varieties show a reduction ranging from 35 per cent to 58 per cent as a result of nematode infestation (Table V, Column 5).

The varieties in this experiment can now be arranged tentatively in the order of their degree of resistance to nematode injury:

1. Lot 520 (F_1 hybrid, Cayenne$\times$Wild Brazil).
2. Wild Kailua.
3. Natal.
4. Pernambuco.
5. Hilo.
6. Cayenne.
7. Ruby.
8. Taboga.

It should be pointed out that while nematode galls develop upon some of its roots, Lot 520 exhibits a very high degree of tolerance based upon the fact that its root length is not reduced by their presence.

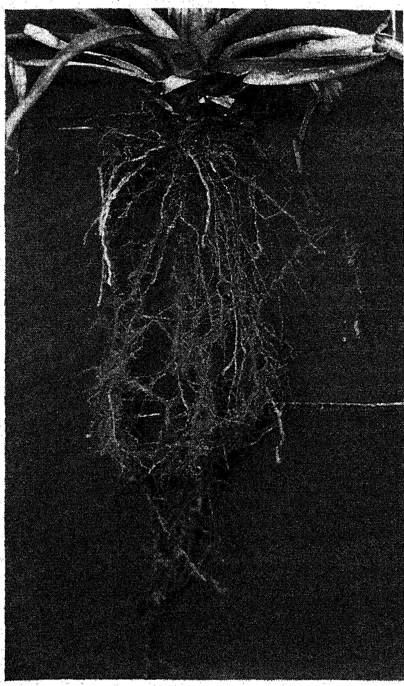
There has existed some confusion regarding the status of Hilo, whether to consider it a distinct variety or only a strain of Cayenne. Throughout this study Hilo has shown differences which lend support to the belief that it is a distinct variety. It has shown slight but consistent superiority over Cayenne in number and length of roots, in the quantity of fine fibrous roots, and in resistance to nematodes.

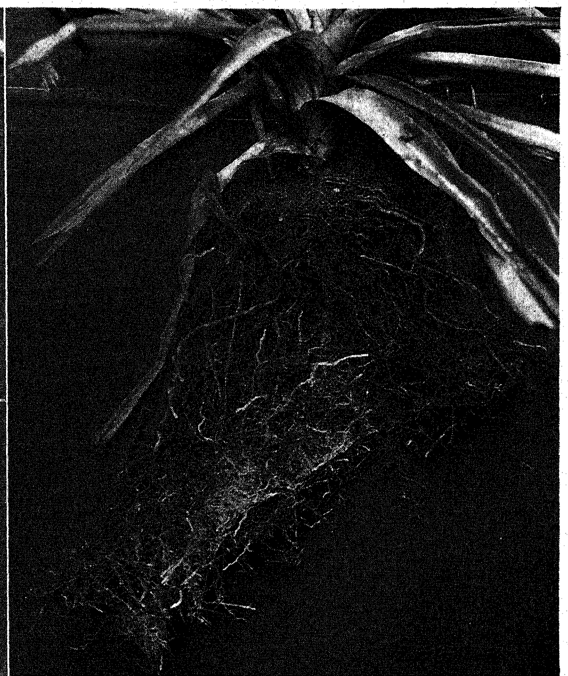
As each of the plants in this experiment was washed free of soil, notes were made regarding the general appearance of the plant and its roots. These observations were concerned chiefly with the uniformity of development, and vigor of the plants within a group.

After these notes were completed it was found that they contained observations of importance which did not appear in the records of measurements. Specifically, it was apparent that the fine fibrous roots were more numerous and better developed on the check plants than on the inoculated ones. It appeared that the presence of nematodes curtailed the development of these fine feeding roots. However, recorded observations upon the degree of gall infestation indicated a very low percentage of visible galls on these roots.

A summarized statement of these observations for each variety appears below.

CAYENNE. *Checks*: Plants show considerable variation in size, are poorly developed and have scanty root systems with poor development of fibrous roots. Roots appear healthy. (Figure 5.)





VARIETAL DIFFERENCES IN ROOT-SYSTEMS

Figure 5

Root system of a Cayenne check plant grown in a root observation box.

Figure 6

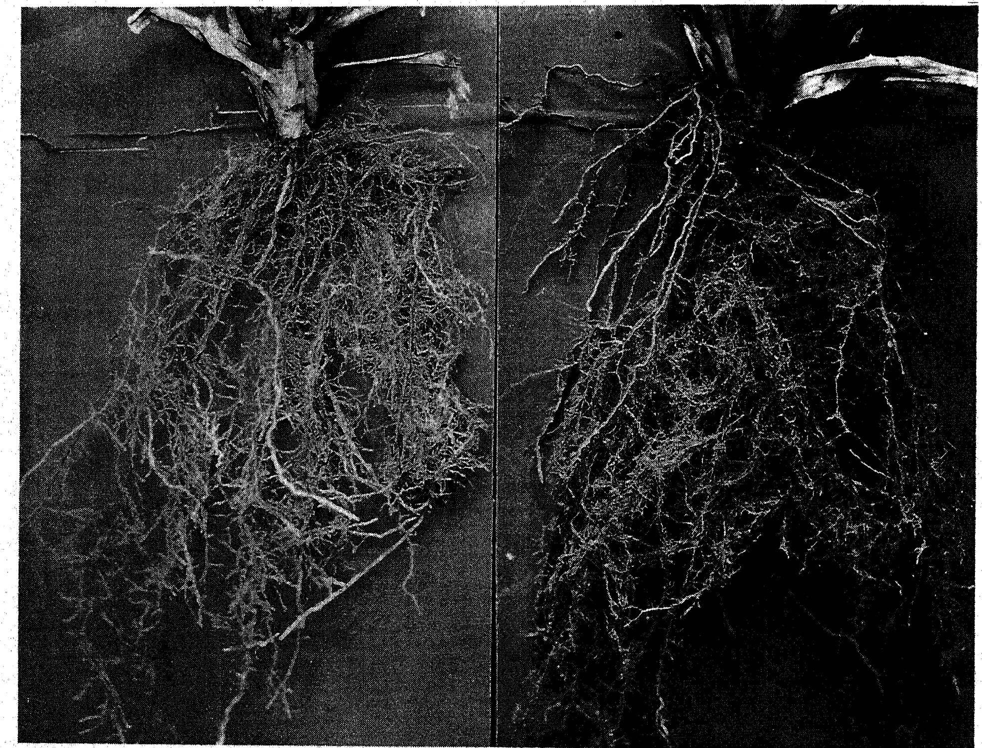
Right—Root system of a nematode infested lot 520 plants grown in a root observation box. It shows the mat-like development of fibrous roots. This hybrid is nematode resistant.

Infested: Plants show variation in size, and a scanty root system with every small development of fibrous and lateral roots.

HILO. *Checks*: Plants show slight variation in size but have well developed root systems throughout with many lateral and fibrous roots.
Infested: Some variation in size of plants; lack of fibrous roots as contrasted with the checks.

LOT 520. *Checks:* Large plants showing slight variation in size. Roots, while not as long as in other varieties, have an excessive development of fibrous rootlets, producing a thick mat of large and small roots. The main roots are larger in diameter than in other varieties, and possess healthy growing tips even on old, long roots.
Infested: Plants show some variation in size. Some have very few roots apparently due to poorly stripped planting material. Two outstanding features in the appearance of these root systems, as contrasted with other varieties of inoculated plants, are: (1) the presence of many fibrous and lateral roots, and (2) the old, long roots alive at the tips and apparently functioning normally. A photograph of one of these plants illustrates the mat of fibrous roots. (Figure 6.)

NATAL. *Checks*: Plants uniform in size. Good root systems throughout with many fibrous roots (Figure 7.)



FIBROUS ROOTS WELL DEVELOPED

Figure 7

Root system of a Natal check plant grown in root observation box. Shows fibrous roots well developed.

Figure 8

Root system of a Pernambuco check plant showing abundance of fibrous roots.

Infested: These plants are very similar to the checks but appear to have fewer fibrous roots.

PERNAMBUCO. *Checks*: Large, well developed, uniform plants. Good root systems with abundance of fine fibrous roots. Better in this respect than the infested plants. A photograph of one of these illustrates the root type. (Figure 8.)

Infested: Plants irregular in size with root system development inferior to the check plants. Fibrous roots well developed but less numerous than in checks.

RUBY. *Checks*: Variation in size of plants. Rather scanty appearing root system with sparsely developed fibrous roots.

Infested: Good sized plants, uniform in size. Rather scanty appearing root systems almost devoid of fibrous and lateral roots.

TABOGA. *Checks*: Plants much smaller than those in other varieties and the roots also are longer in proportion to the tops. They show a fairly good development of lateral and fibrous roots.

Infested: Small plants uniform in size. Roots much shorter than in checks with almost no lateral and fibrous roots.

WILD KAILUA. *Checks*: Plants uniform in size having uniformly good root system with well developed lateral and fibrous roots.

Infested: Plants show some variation in size. Root systems well developed but have less lateral and fibrous roots than in checks.

Summary

1. This investigation has included populations of the following pineapple plants: Cayenne, Hilo, Lot 520 (F_1 hybrid of Wild Brazil × Cayenne), Natal, Pernambuco, Ruby, Taboga and Wild Kailua. It was planned to determine the measurable inherent differences in root development; the amount of root damage by nematodes, if evident; and the relative degree of resistance to nematode attack in the different lots of plants. It was limited to the first 8 months of plant growth.

2. Of the plants on which measurements of fibrous roots were made, it was found that Lot 520 possesses a larger percentage than any other variety. Cayenne exhibits the smallest percentage of fibrous roots.

3. Length of roots and number of roots both appear to be varietal characteristics and differ with the variety.

4. Lot 520 plants have a much greater proportion of gall-free roots than any other variety, while Cayenne and Taboga have a very low proportion of their roots free from galls. The Lot 520 plants appear to be highly tolerant to the effects of heavy nematode attack and show no diminution in root length, at least during the first 8 months of growth.

5. The varieties show differences in general root system development in that some produce a large proportion of their total number early and the remainder much later.

6. The comparison of infested with check plants demonstrates that number of roots is not influenced by presence of nematodes.

7. There is a material reduction in length of roots in all varieties due to the presence of nematodes, except in Lot 520 which appears to be resistant to their effects. All varieties appear to suffer a reduction in quantity of fine fibrous roots.

8. In general, short roots are more heavily infested than are the long roots. Lot 520, Pernambuco and Wild Kailua show relatively fewer infested short roots than do Cayenne, Taboga and Natal and are considered more resistant than the latter three varieties.

9. No pineapple varieties were found to be immune to nematode infestation. Differences in degree of resistance to nematode injury were shown. These studies have made it possible to arrange the varieties in the following order which represents their relative degree of resistance to nematode injury, the most tolerant being listed first: (1) Lot 520, (2) Wild Kailua, (3) Natal, (4) Pernambuco, (5) Hilo, (6) Cayenne, (7) Ruby, (8) Taboga.

10. Hilo has consistently appeared superior to Cayenne in these tests.

11. These conclusions are drawn from observations on young plants and from limited populations. They are subject to revision should more comprehensive data later indicate different interpretations.

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Table III. Comparison of nematode infestation on roots of different varieties.

1	2	3	4	5	6	7
Variety	No. of Plants	Aver. No. roots per plant	Aver. number of galls: Terminal	Aver. number of galls: Non-Terminal	No. of galls per root	Per cent of roots with no galls
Cayenne	12	38.66 ± 1.76	36.75 ± 2.23	14.66 ± 1.93	1.35 ± 0.063	4.7
Hilo	32	42.44 ± 1.25	34.62 ± 1.54	16.19 ± 1.33	1.18 ± 0.039	15.4
Lot 520	16	26.90 ± 1.89	16.50 ± 0.42	14.00 ± 1.36	1.13 ± 0.063	34.0
Natal	16	25.00 ± 0.22	21.47 ± 1.00	17.00 ± 1.49	1.52 ± 0.063	13.3
Pernambuco	16	28.38 ± 1.25	24.25 ± 1.22	11.63 ± 2.55	1.27 ± 0.055	29.1
Ruby	16	16.93 ± 0.57	15.06 ± 0.60	4.25 ± 0.37	1.15 ± 0.004	9.5
Taboga	16	10.81 ± 0.53	10.00 ± 0.67	7.31 ± 0.84	1.54 ± 0.072	3.5
Wild Kailua	16	17.62 ± 1.22	11.07 ± 2.05	7.68 ± 0.92	1.36 ± 0.060	7.8

Table IV. Differences in percentages of infestation of each variety with every other variety (Terminal galls only). The arrow in each square indicates the variety exhibiting the larger percentage of infestation.

Variety	Cayenne	Hilo	Lot 520	Natal	Pernambuco	Ruby	Taboga
Hilo	10.7 ↑						
Lot 520	29.3 ↑	18.6 ↑					
Natal	8.6 ↑	2.1 ←	20.7 ←				
Pernambuco	24.4 ↑	13.7 ↑	4.9 ←	15.8 ↑			
Ruby	4.8 ↑	5.9 ←	24.5 ←	3.8 ←	19.6 ←		
Taboga	1.2 ←	11.9 ←	30.5 ←	9.8 ←	25.6 ←	6.0 ←	
Wild Kailua	3.1 ↑	7.6 ←	26.2 ←	5.5 ←	21.3 ←	1.7 ←	4.3 ↑

Table V. Comparison of root systems of check and infested plants (Lengths recorded in inches).

Variety	No. of Plants	Average number Roots per plant	Average length Roots per plant	Percentage reduction in length due to nematodes.
1	2	3	4	5
Cayenne				
Check	12	39.08 ± 2.48	9.08 ± 0.62	
Inoculated	12	38.66 ± 1.76	4.91 ± 0.28	45.92
Difference		0.42 ± 3.03	4.17 ± 0.68	
			D/E = 6.10	
Hilo				
Check	28	42.57 ± 1.81	10.50 ± 0.22	
Inoculated	32	42.44 ± 1.25	6.12 ± 0.17	41.71
Difference		0.13 ± 2.19	4.38 ± 0.08	
			D/E = 15.75	
Lot 520				
Check	14	25.00 ± 1.97	7.50 ± 0.31	
Inoculated	16	26.90 ± 1.89	7.10 ± 0.30	0.00
Difference		1.90 ± 2.73	0.40 ± 0.43	
			D/E = 0.92	
Natal				
Check	14	24.80 ± 1.07	12.50 ± 0.02	
Inoculated	16	25.00 ± 0.22	8.00 ± 0.29	36.00
Difference		0.20 ± 1.09	4.50 ± 0.29	
			D/E = 15.51	
Pernambuco				
Check	14	34.30 ± 2.21	10.80 ± 0.36	
Inoculated	16	28.38 ± 1.25	6.40 ± 0.27	40.74
Difference		5.92 ± 2.58	4.40 ± 0.45	
			D/E = 9.77	
Ruby				
Check	14	16.50 ± 0.81	11.71 ± 0.31	
Inoculated	16	16.93 ± 0.57	5.25 ± 0.24	55.16
Difference		0.43 ± 0.97	6.46 ± 0.39	
			D/E = 16.69	
Taboga				
Check	14	11.50 ± 0.50	13.14 ± 0.39	
Inoculated	16	10.81 ± 0.53	5.50 ± 0.34	58.14
Difference		0.69 ± 0.73	7.64 ± 0.51	
			D/E = 14.77	
Wild Kailua				
Check	14	20.14 ± 0.70	13.35 ± 0.29	
Inoculated	16	17.62 ± 1.22	8.68 ± 0.30	35.05
Difference		2.52 ± 1.39	4.67 ± 0.41	
			D/E = 11.39	

Table VI. Further analysis of root lengths of check and infested plants showing number and percentage of long and short roots and galls on short roots.

			1	2	3	4	5	6	7
Variety		No. of Plants	Long Roots: Mean No.	Long Roots: Mean length	Short Roots: Mean No.	Short Roots: Mean length	Per cent short with galls	Per cent long roots	Per cent old roots
Cayenne	Check	12	26.0	11.6	13.1	3.1	—	66.0	82.0
	Inoc.	12	13.9	11.2	24.6	1.8	91.2	36.0	58.0
Hilo	Check	28	34.4	11.9	7.4	3.7	—	82.0	91.1
	Inoc.	32	20.6	10.1	21.8	2.0	80.9	48.0	66.8
Lot 520	Check	14	13.5	10.8	11.5	3.3	—	56.9	83.0
	Inoc.	16	13.8	10.4	12.5	2.5	69.0	52.6	78.0
Natal	Check	14	22.9	12.8	1.9	3.8	—	92.0	79.0
	Inoc.	16	16.8	10.8	8.0	2.3	88.5	67.0	72.3
Pernambuco	Check	14	29.9	11.4	4.3	4.4	—	87.3	98.0
	Inoc.	16	13.8	9.2	14.5	2.9	70.7	48.0	86.9
Ruby	Check	14	14.7	12.7	2.5	3.2	—	85.0	90.9
	Inoc.	16	6.8	9.8	10.1	1.8	90.7	40.0	66.0
Taboga	Check	14	10.4	14.4	1.4	3.2	—	90.0	92.5
	Inoc.	16	4.4	9.4	6.3	2.8	93.1	41.0	91.3
Wild Kailua	Check	14	18.6	13.8	1.6	3.7	—	92.0	92.6
	Inoc.	16	12.9	10.5	4.6	2.6	77.3	73.0	93.3

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Topsy-Turvy Evolution

MAN AND ANIMAL: Five Aspects of Their Essential Difference. *By* Dr. Hermann Poppelbaum (Translated from the German by Edith Rigby and Owen Barfield), London and New York: Anthroposophic Press. 1931. 8vo., Pp. 174, 18 Illustrations and Diagrams.

THIS book is an example, with some innovations, of the anthroposophical concepts of the problems of evolution and life. The author, who has been highly stimulated by the writings and teaching of Rudolf Steiner, presents the subject matter in five parts, captioned as I, Form and Shape, II Descent, III Soul, IV, Experience, and V Destiny in which it is proposed to delineate the "essential" differences between man and the lower forms of life. In Part I comments are made upon the comparative anatomy of the hand, foot, head, hair, teeth, brain and some of the internal organs, tending to prove that these various structures in man have been interrupted in their development, and that the human being is a more backward form than some of the higher mammals. These considerations "overstep the boundaries customary in comparative anatomy" and seek to "find the inner connection between the 'primitiveness' of the human physical organization and its significance for the whole nature of man." Great emphasis is laid upon the polarity of the upper and lower system in man's organization, which "Rudolf Steiner described in a comprehensive way, drawing from spiritual sources to which he had access," because "all arrested development in a man's body depends upon the fact that his upper pole is, so to speak, withdrawn from the gravity-forces of the earth." Contrary to the human, the upper part of the animal's body is surrendered to gravity and "the lower refuses to submit to the forces of gravity, therefore, Man in his upper pole retains a condition which the animal abandoned in the past, and strives in his lower pole "toward a connection with the earth to which the animal is denied access."

In Part II the autho- criticizes the modern concepts of the descent of man.

The animal from birth to premature old age, hastens through its development, while man drags through a long period of middle age in which he has time to develop a free Ego-being. The "genealogical tree" of man and the animal kingdom as conceived by the earlier Darwinians, has, with more research, degenarated into a "shrub" whose very branches are threatening to fall off. Among other illustrations, a diagram of Haeckel's tree of vertebrate descent is here given to be compared with Gregory's (1924) table of descent, and we are informed that common ancestors are not to be found in fossil relics, as they were more fluid and plastic—the human archetype is still further back in some form of soul stuff. A concept, in terms highly metaphysical, of the earth's evolution according to a "spiritual scientific system" is given "as it is seen by supersensible perception," with an outline of the earth's epochs of development. The phrases "Everything that comes into being arises from a spiritual source" and "It is from the deeds of Spiritual Beings that evolution proceeds" seem to express the pivotal ideas around which the main hypotheses of the entire book have swirled and grown. These "Spiritual Beings" have reacted with intelligence from the first and their deeds have had much to do with the sun, moon and earth, to say nothing of the other elements in the cosmos.

Parts I, II, IV, and V dealing with the Soul, Experience and Destiny respectively attempt to outline the psychical and spiritual aspects and meanings of the differences between man and animal. The statement that "The animal has wisdom in its organs, man has not; man must first acquire it through inner effort" should be difficult enough for a biologist to digest, but the assertion that *"the forms of the mammals can be understood by regarding them as degenerate modifications of the human form"* must needs produce a reverse peristalsis. There is also much comment on the central position of consciousness in forms of behavior, on the importance of the "indestructible" human Ego, and on the bondage of the lower animals, reenforced by examples which are true only in part.

Although the book contains many biological truths and facts revealed by scientific experiments well known and accepted by students of comparative anatomy, physiology and behavior, its "fundamentals" are examples of wish fulfillment thinking; and one all too often encounters sheer nonsense. It goes without saying that knowledge gained from "spiritual sources" has always been unreliable in scientific procedures, and in the past, not seldom retarding to the growth of science.

N. D. C. Lewis.

Popular Evolution

EVOLUTION YESTERDAY AND TODAY, *by* H. H. Newman. Pp. X+171. Price, $1.50. Williams and Wilkins Co., Baltimore, 1932.

THIS book is an attempt to explain to "the man in the street" what the evolution theory is, how it arose as an alternative to the creation theory, and what are the evidences in its favor. In the later chapters recent developments of the evolution theory are discussed involving Mendelian inheritance, the mutation and pure line theories, genes and chromosomes. For better informed and more sophisticated readers, the author would doubtless suggest perusal of his more extensive and better known book, *Evolution, Genetics and Eugenics*.

This attempt at popularization of scientific results for non-scientific readers is not particularly successful, partly because it retains the technical jargon of the specialist quite needlessly, and partly because it contains numerous inaccurate statements, which would seem to imply that anything will do for the reader who does not know better. The most charitable interpretation would be to charge these errors to haste and

carelessness in preparation and publication.

The reader is told on page 5 that "nowhere else in the world than in South America have any living or fossil sloths been found," whereas even our daily papers contain frequent notice of the finding of bones and even dung of the ground sloth in caves of our southwestern states. These notices the man in the street is likely to have read. The Huxley who defended Darwinism in its early days is on page 27 christened William Henry. Even a well-informed proof reader should have changed this to Thomas Henry.

In describing the mechanism of heredity, in which the rôle of chromosomes is shown to be important, the diploid chromosome numbers of mouse and maize are stated to be 36 and 12 respectively, whereas even the beginner in the genetics of these much studied organisms knows them to be 40 and 20 respectively. On page 117 it is stated that the progeny of a single male-female (hermaphrodite) individual constitute a pure line, whereas if the individual is a species hybrid its offspring under continuous self fertilization may be extremely variable, as is known to every student of plant breeding.

On page 160 Heape is credited with an ovarian transplantation experiment demonstrating the non-existence of Lamarckian inheritance, whereas the first successful experiment of this sort was made by American workers in Newman's own time. Heape's experiment was a very different one, since it involved transfer of a fertilized egg,—not immature ovaries.

Popularization of scientific results should be done with the greatest care, if the public is not to be given erroneous impressions. A model popular exposition of evolution is found in S. Herbert's *First Principles of Evolution* published in 1913, prepared for working men in England, clear, simple, nontechnical, well illustrated but not over illustrated, and accurate to the last detail. With this the volume under review contrasts sadly.

W. E. Castle.
Bussey Institution.

A New Guidebook to Statistics

THE METHODS OF STATISTICS, *by* L. H. C. Tippett. Williams and Norgate, Ltd., London, 1932.

HERE is an orderly presentation of the efficient statistical methods which have been developed during the last half century by a brilliant school of biologically oriented British statisticians. The book is profusely illustrated with authentic data drawn mostly from the various branches of biological literature. Coming as it does hard upon the heels of the third edition of R. A. Fisher's *Statistical Methods for Research Workers*—and to be closely followed, if report is true, by a fourth edition—Tippett's book is the second to present to biologists an adequate working set of statistical tools. Completely up-to-the-minute, it contains most of the published developments of analysis of variance and co-variance.

Tippett has done no mere job of reporting. Besides making numerous minor contributions, he has enriched statistical methodology in two particulars. First, he has completed the integration, initiated by Fisher, of the correlation ratio, and its much disputed test of significance, into the general theory of correlation. The computation of r and η is incorporated in a single table. Second, he has presented in a remarkably simple fashion a new theorem in analysis of variance (page 93) which makes available an estimate of the variance in heterogeneous data in addition to the usual test of significance. This theorem first appeared in a mathematical paper by Irwin* in 1931. The presumption is

*J. O. Irwin, "Mathematical Theorems Involved in the Analysis of Variance." *Journal of the Royal Statistical Society,* 94:296. 1931.

that the two authors arrived at their results independently.

A remarkable amount of material has been packed into the 214 pages of this book. It is almost encyclopedic in nature. Nearly every user of statistics may find in it stimulating suggestions for the extension and improvement of his technique. Alternate methods of statistical treatment are emphasized, the identity of the results being insisted upon. Obscure points in the use of tables of x^2, t, and z are explained in detail. The theoretical conditions underlying each statistical method are persistently stated. Some excellent passages occur in the sections entitled "General Discussion," and worthwhile comments are frequent. For example (page 190), "The time to consult statistical principles is *before* the experiment is planned, not after the results are obtained."

It is inevitable that this work should be compared with its predecessor, *Statistics for Research Workers,* by Fisher. Of about the same extent, the two books cover substantially the same ground. Tippett's contains much more detail, including answers to many practical questions which arise in the minds of users of statistics. Indeed, this very abundance of detail makes it even more bewildering to the novice than Fisher's. Since Tippett has included no tables of the ordinary statistical functions, the practitioner must have access to Fisher's book in order to use his tables of t and z. The reader will miss the elegance of Fisher's style, while the biologist, especially the geneticist, will note the absence of that authority with which Fisher discusses biological problems. Fisher would never have given the illustration of the additive nature of x^2 to be found on page 67.

I cannot agree with Tippett that he has reduced correlation to "a special case" of analysis of variance. On the contrary he has placed it in its rightful place among the important statistical concepts. His statement seems to arise from an undue emphasis in his own mind—not apparent in the text—of the Fisher method of testing significance in correlation, of the new device, analysis of co-variance, which from one standpoint may be viewed as a method for pooling the estimates of correlation found in different but related groups, and of the well-known relation between analysis of variance and intraclass correlation. The author's repeated reference to the discovery of "association" by the analysis of variance appeals to me as purely academic.

Evidences of haste in preparation are numerous but not important. Many of the tables and figures are without titles, errors in grammar and construction occur, symbols are occasionally introduced without definition and promptly abandoned, and an elaborate system of section numbering is set up but used for cross reference only in the latter part of the volume. Such undefined terms as "real" and "association" are frequently substituted for the defined "significant" and "correlation," while the technical "non-significant" appears under the guises, "insignificant," "signifies nothing" and "has no measurable effect." The author is peculiarly unfortunate in the technical terms he has introduced. His "within an array" is less accurately descriptive than Fisher's "within arrays," and is abandoned in the later chapters. The term "mean variance" merely covers up some elusive details concerning which Fisher's "mean square" is at least a warning. All these items, however, are only minor details of the volume, detracting little from its excellence.

The mechanical perfection of the book is noteworthy. Among all the formula and computations, I noticed only a single error (formula, bottom of page 147). The work will command the interest and respect of everyone concerned with the methods of statistics.

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BLOOD GROUP RESEARCH SINCE 1900

A Review

HANDBUCH DER BLUTGRUPPENKUNDE. *Edited by* PAUL STEFFAN. Pp. xi + 669. München, 1932. J. F. Lehmanns Verlag.

OCCASIONALLY there appears on the scientific horizon a problem whose mushroom-like growth quickly carries it to all corners of the world. Such a problem was provided in 1900 by Landsteiner's discovery of the human blood groups. Scarcely a country has failed to supply literature on the subject, and the problem has grown amazingly in interest and vigor of attack. Vivid witness to these statements is provided by the volume on blood grouping which has recently appeared under the editorship of Dr. Paul Steffan, and especially by the bibliography of 108 pages which appears at the end of the book, in which are listed 2,979 separate articles on blood grouping. The title of the volume proclaims it a "handbook," yet it contains nearly 700 large pages of closely set type. The various phases of the blood group problem have broadened to the point where no individual can sufficiently encompass them all, hence the book is very properly written cooperatively by seven of the outstanding blood group workers of Europe.

The opening chapter, by Hesch of Leipzig, outlines briefly the development of the blood group investigations from the beginning of the century to the present time. Then follows a detailed discussion of the serology of the blood groups in man and animals, by Thomsen of Copehagen. The recent proof of the existence of two kinds of group *A* is carefully outlined in this section; so also is the bacterial phenomenon of receptor-formation discovered by Thomsen in recent years. A strong plea is made for the dropping of the term "blood group" and the substituting of the term "blood type." It is to be feared, however, that this desire will have about as much success as the attempt some years ago on the part of ornithologists to change the name of the robin to "Migratory thrush."

The third chapter, of more than 100 pages, is by Wellisch of Vienna, and is given over to the heredity of the blood groups. On statistical grounds as well as on the intimate inspection of family histories, the various suggested hypotheses of blood group heredity are analyzed. The statistical analysis is exceptionally complete. The triple allelomorph hypothesis appears to satisfy the scheme of four orthodox groups, with the probability that four allelomorphs really exist, thus including the two kinds of group *A* in the hereditary scheme. The new immune agglutinogens *M* and *N* are given a section at the end of this chapter.

No Linkages Established

Thomsen again provides an authoritative and interesting chapter as the fourth of the volume, on the relationships of the blood groups to other human characters. Many such characters are discussed, some known to be hereditary, others not. Emphasis is laid on the proper understanding of the difference between correlation and true linkage in such studies. To date no linkage between the blood groups and any other hereditary factor has been established. Relatively few of the studies made, however, have been so designed as to yield true linkage results.

The aid of a physician, Dr. Bürkle-de-la-Camp, of Munich, is enlisted to provide the fifth chapter, dealing with the transfusion problem. Technic, indications, and the physiology of transfusion are thoroughly discussed. In this chapter appear the first illustrations of the volume, a welcome addition to the pages of formulae in the preceeding chapters.

Blood Groups in Legal Medicine

Some of the most important practical applications of blood grouping have been in the field of legal medicine. Raestrup, of Frankfort am Main, provides the discussion of these questions. In addition to the well-known applications in cases of disputed parentage, mixed babies in hospitals, and dried blood stains, attention is drawn to the possibilities offered to the police in the identification of individuals, both living and dead, especially where the normal marks of identification have been purposely or accidentally rendered ineffectual. An enlightening series of case histories illustrates the legal possibilities in various circumstances. Nothing is said in this chapter, however, about the greatly increased legal applications offered by the new immune agglutinogens.

The anthropological applications of blood grouping form the basis of Chapter 7, by Steffan, of Wilhelmshaven. Here are listed all the determinations of blood group frequencies in various populations, numbering some six hundred. The various graphic representations of racial blood group distribution are discussed and compared. Steffan employs for illustrations in the book a triangular form of chart, which does not appear to offer much more in the way of clarity than do other suggested diagrams. The use of the blood groups as supplementary anthropologic criteria, not to be considered as sufficient by themselves, is ably discussed.

The final chapter, by Schött of Stockholm, is devoted to the technic of blood grouping. Various systems of group determination are described, and a method is derived for the determination of the groups in the absence of available known serums and cells. The determination of titre of both serums and cells is outlined, and its importance discussed. Interesting photo-micrographs are shown, and the methods of photographing agglutination are explained.

In addition to the large bibliography, an appendix of squares and square roots of numbers from 0.01 to 10.00 by hundredths is given, and of square roots of numbers from 10 to 100 by tenths. An adequate index is provided.

No one can realize the thorough manner in which the many ramifications of the blood group problem have been attacked, especially in the last few years, without a volume of this sort to organize and present the material. Coming as it does from the pens of those whose individual work along these lines is outstanding, the book carries with it an authenticity not to be questioned. It appears to be so complete that the certainty emerges that no one in the future can adequately plan further experimentation involving the blood groups without first consulting this mile-stone in human biology.

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Social Inventions

We live in an age of machines. In fact, some critics of the present state of human kind make the suggestion that most of our troubles are due to the fact that machines are running men, while men only operate machines. Machines in fact are pictured as something of a Frankensteinian nightmare of an industrial world.

In contrast to this amazing development of mechanical invention, our social organization has remained very much the same, world without end. Our politicians and social reformers are still clothed in skins and chipping flints, much as their Cro-Magnon ancestors did. The lawyers who tilt with each other in our crowded court rooms are armed and armoured with the bulky, if decorative, legislative steelwork of me-

diaeval times. Our mathematicians, as Professor Armstrong recently observed, can bewitch us with discussions of ultra-galactic nebulae, but seem unable to give our money-lenders a more satisfactory medium of exchange than was known to the Chaldees of Ur.

In the *Scientific Monthly* for October, Professor Arland D. Weeks discusses some of the contrasts between our advanced state of mechanical invention and our deplorably retarded state of social invention. Dr. Weeks believes that we may have before us an age of social invention comparable to the last two hundred years of mechanical invention. Swamped as we are with so many machines, this step may be forced upon the human race as a matter of self-preservation.

Noticeable in many analyses of the need for change in man's social organization is much of the naive animism that characterizes inventors of perpetual motion machines. The field of social invention is really a good deal more complicated than most of the seekers after "reform" seem to realize. A "planned society" will turn out to be rather more of a problem than just saying "einy-meiny-miny-mo" to the butcher, the baker and the candlestick maker. When we tinker with the cams and levers in a machine, we get results that can be fairly easily analyzed. When we tinker with the emotional cams and instinctive levers of the social organization of a biological species, we are very apt to get unlooked for results in a half dozen different dimensions. It is absurd to say, of course, that this makes the probem insoluble. The fact that so many species have succeeded in fitting themselves into so many kinds of environments in such a variety of ways proves that problems of biological adaptation can be solved.

Dr. Weeks lists tentatively the following imposing array of fields of endeavor for the social inventor:

Tax system
Jury trial
Wearing apparel
League of nations
Traveling libraries
Accident prevention
Capitalistic system
Medicine
Graft
Legal service
Weights and measures
Value of the dollar
War
Minorities
International language
Distribution of wealth
Noise
Health
Motivation of production
Disarmament
Idle time
Worry
Personal insulation
Duplication
Advertising
Tariff
Cities
Wild life
Jobs
Discovery of law breakers
Regulation of production to need
Moral code
Fundamentalism
Law schools
New wants
International trade
Alumni
Crime prevention
Poverty
Political platforms
Racial accord
Court procedure
The work of assessors
Investment
Waste of metals
Overcrowded professions
The "funnies"
Rackets
Simple life
Pedestrianism
Liquor control
Form of government
Red tape
Automatic referenda
Judgment test for voters
Education
Rumor damper and lie sterilizer
Conservatism
Rotation of occupation
Travel
Community buying and use
History

Strangely enough, Weeks leaves out what we suspect may prove to be the most important of all—*Eugenics*. Without a eugenic outlook, many of these problems (and some of the most important ones) would be beyond the possibility of an inventive solution. Their solution is not to be obtained by rule-of-thumb measures and rule-of-thumb half-measures, but by a good deal more concentrated and competent thought to these so vital matters than is now being accorded them. Invention in its highest form represents the application in novel circumstances of revolutionary ideas. Great is the need for them in this field.

The recently suggested solution of our problems which has reached the front pages of the metropolitan dailies under the alluring title "Technocracy" appears to be seriously inadequate in this direction. The Machine Age represents perhaps as drastic a modification of the human environment as the

coming of the last Ice Age, with the difference that the returning glaciers did not fill our ancestors' back yards with ice overnight. The Machine Age has literally buried us under an avalanche of mechanisms between dawn and dark, racially speaking, and to hope that a re-integration can be accomplished by simply juggling the counters in the economic poker game is fantastic. It is like believing that we can turn a reptile into a mammal by somehow substituting round blood corpuscles for the elliptical ones which snakes affect. Problems of education, of quality and quantity of population, of economics and of sociology must somehow be faced and integrated before we can safely say that the Machine Age has been assimilated as a non-lethal addition to the human environment. This is a job in inventive technique, and involves a coordination and integration of forces, prejudices, and passions that makes the construction of the Boulder-Hoover dam reminiscent of a beaver's playful stickwork in some quiet creek.

R. C.

ANTONY VAN LEEUWENHOEK

Leeuwenhoek, the tricentenary of whose birth occurs in December, 1932, was one of the most interesting apostles of science the world has ever known. An uneducated man, he nevertheless opened up the vast world of sub-visible living things. Lens-grinding was with him almost an obsession, and with maniacal accuracy and skill he produced the most perfect lenses that had ever been made up to that time. With these he spent nearly fifty years—peering into the strange world of the invisible. Where the cultured Newton "picked up a few pebbles on the shore of the sea of the unknown," this Dutch dry-goods merchant garnered a whole basket-full of priceless jewels with the gleeful and unselfconscious abandon of a child. That one man could have been the first to have seen and reported so many things is amazing. He is recognized as the discoverer of bacteria, but he also saw so much else! He did not elaborate Schleiden and Schwamm's cell theory, of course, but he apparently discovered the cells of which plants and animals are made. He first saw the capillary circulation of the blood, thus forging the last link in the chain which was postulated by Harvey. Leeuwenhoek did not see chromosomes, but in a way he may be said to be the father of genetics, as well as of bacteriology, for he first saw a male gamete at about the time that his friend, de Graff, made such fundamental contributions to our knowledge of female gametes that we have Graafian follicles to this day. His letter announcing the discovery of human sperm cells, while not included in the popular "lives" of Leeuwenhoek, is said to be an altogether charming example of his naive and puppy-like curiosity.

Was Leeuwenhoek a scientist or only a prying crank? He enunciated no far-flung principles: he founded no schools (because to have done so would have made him less free to *look!*), he had little formal education, and yet—he had a gift for accurate observation, and a priceless restraint in formulating hypotheses. "People who look for the first time through a microscope say now I see this and now I see that, and even a skilled observer can be fooled. On these observations I have spent more time than many will believe, but I have done them for joy—." It is not a bad epitome of the scientific method.

How thrilled this old Dutch merchant would be could be come back on this three hundredth anniversary of his birth and see his "wretched beasties" in all the detail that modern technique has brought out, with its stains, condensers, achromatic lenses, etc. Exciting as this would be, it could never equal that first glimpse, two and a half centuries ago into a world which had never before been seen by human eyes—Balboa at Darien, Columbus at Watling Island, and da Gama rounding the Cape of Good Hope, all rolled into one, as it were, and magnified enormously by one of his own tiny lenses!

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